IDEAS AND ACTION .... EMERGENCE
OF TECHNICAL INNOVATION AND FINANCIAL DISCOURSE

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NICHOLAS P. G. COX

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This Thesis is Dedicated
to the Memory of
DAN GOWLER
The Finest of Supervisors,
a Great-Heart and Valiant-for-Truth.

they came to a place at which a man is apt to lose his way. Now, when it was light their guide could well enough tell how to miss those ways that led wrong, yet in the dark he was put to a stand: But he had in his pocket a map of all ways leading to or from the Celestial City; wherefore he strook a light .... and takes a view of his book or map, which bids him be careful in that place .... And had he not here been careful to look in his map they had in all probability been smothered in the mud

"I do not repent me of all the trouble I have been at to arrive where I am. My sword I give to him that shall succeed me in my pilgrimage, and my courage and skill to him that can get it. My marks and scars I carry with me" (Bunyan)

ABSTRACT

IDEAS AND ACTION .... EMERGENCE OF TECHNICAL INNOVATION AND FINANCIAL DISCOURSE

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This thesis investigates how people's ideas lead to technical innovations, and looks at the problems and setbacks along the way. The research uses data gathered from two major case-studies, several minor ones, and from a wide range of potential sources of finance for innovation. The majority of this data was obtained through free-format interviews with the people involved, although one of the major case-studies used personal letters to access an innovation at the turn of the century. This demonstrated the utility of 'historical' data for management research.

The orientation is qualitative and interpretive, but the thesis demonstrates the utility of a rigorous and procedural approach to data and analysis in accomplishing its interpretation. Four distinct discourses, or world-views, emerge from the data, and a framework comprising these is proposed to aid understanding of innovation. This tentative model encompasses the agency and actions of individuals together with their social systems, and follows Giddens in seeing the former as recursively implicated in reproducing the latter. The model allows the progress of an innovation to be charted through the four discourses.

The argument of the thesis is sensitive to recent anthropological and sociological uses of ideas drawn from Saussure's structural linguistics. French structuralist developments of these ideas, particularly those of Derrida, are used to investigate problems with the proposed four-discourse model of innovation. Derrida allows a more complete synthesis of structure and process than Giddens; his complex ideas thus enable a better explanation of the observation that people involved in innovation can apparently talk within all four discourses at once without excessive anxiety.

TABLE OF CONTENTS

ABSTRACT i
TABLE OF CONTENTS ii
ACKNOWLEDGEMENTS vi

CHAPTER 1 : INTRODUCTION 1

CHAPTER 2 : THE PROVIDERS OF THEORY - A REVIEW 11
2.1 INTRODUCTION 11
2.2 THE THEORETICAL LEGACY 13
2.2.1 Introduction 13
2.2.2 Durkheim 14
2.2.3 Saussure 16
2.3 ANTHROPOLOGY 20
2.3.1 Symbolism - Meaning, Boundaries and Time 20
2.3.2 Lévi-Strauss and Structuralism 24
2.3.3 Sperber's Cognitive Critique of Lévi-Strauss 26
2.4 RADICAL MODERNIST SOCIOLOGY 29
2.4.1 Introduction - Discourse and Relativity 29
2.4.2 Giddens and 'Structuration' 34
2.5 FRENCH STRUCTURALISM .... AND BEYOND? 44
2.5.1 Introduction - Structuralism, Modernism and Rationality 44
2.5.2 Barthes, Lacan and Foucault 48
2.5.3 Derrida 55

CHAPTER 3 : METHOD 79
3.1 INTRODUCTION 79
3.2 DIFFERENT SOURCES OF DATA 82
3.3 METHODOLOGICAL QUESTIONS 85
3.4 ACCESS - INSIDERS AND OUTSIDERS? 91
3.5 PREPARATION FOR THE INTERVIEW 97
3.6 CONDUCT AND RECORDING OF THE INTERVIEW 99
3.7 FOLLOW UP 106
3.8 ANALYSIS - DIFFERENT TYPES OF DATA 110
3.9 WRITING ETHNOGRAPHY 116
3.10 A DATA PRIMER - DEFINITIONS AND CONVENTIONS ADOPTED 122

CHAPTER 4 : THE PROVIDERS OF IDEAS - SOME CASE-STUDIES . 126
4.1 INTRODUCTION 126
4.2 AN HISTORICAL CASE-STUDY - PARSONS AND THE MARINE STEAM-TURBINE 129
4.2.1 Introduction - the Material 129
4.2.2 Parsons, Fisher, and the Armament Business 131
4.2.3 The Marine Steam-Turbine - Early Days, Steam-Turbines and Patents - the 1880s. . 139
4.2.4 The Marine Steam-Turbine - Small Ships - the 1890s. 141
4.2.5 The Marine Steam-Turbine - Large Ships - the 1900s. 150
4.2.6 A Few Brief Observations on the Use of History 155

4.3 A CONTEMPORARY CASE-STUDY - HOTOL 157
4.3.1 Introduction 157
4.3.2 The Originators 160
4.3.3 1984 - Hotol becomes Public 164
4.3.4 Hotol - as a Government Assisted Company Project 1985-1987 169
4.3.5 "The decay of the programme" 174
4.3.6 Epilogue 178
4.4 OTHER CASES - A FEW VIGNETTES 182
4.4.1 Introduction 182
4.4.2 Another Aerospace Innovation 183
4.4.3 Starting up a Technological Company . . 186
4.4.4 A Management Buyout 188
4.4.5 Invention - the Last Word 190

CHAPTER 5 : THE PROVIDERS OF FINANCE - AN INTERPRETIVE SURVEY 193
5.1 INTRODUCTION 193
5.2 HISTORICAL ASPECTS 194
5.3 A FINANCE PRIMER 205
5.3.1 Introduction 205
5.3.2 A Few Statistics 206
5.3.3 Debt and Equity 208
5.3.4 Government and Publicity 208
5.4 THE TRADITIONAL SOURCES OF CAPITAL 212
5.4.1 Introduction 212
5.4.2 Traditional Formal Finance 212
5.4.3 Traditional Industry 216
5.4.4 'Short-Termism' 217
5.4.5 Traditional Informal Finance 219
5.5 VENTURE CAPITAL - THE DIACHRONIC STRUCTURE . . 221
5.5.1 Introduction - Statistics and Structures 221
5.5.2 Venture Capital Funds and Stages 223
5.5.3 Management Buyouts & Development Funding 226
5.6 VENTURE CAPITAL - THE SYNCHRONIC PROCESS . . . 228
5.6.1 Introduction 228
5.6.2 Entry to a Start-up 228
5.6.3 Products, Markets, People and Management 230
5.6.4 Technology Transfer 233
5.6.5 Returns, Failure and Success, Risk . . . 234
5.6.6 Exit from a Start-up 236
5.6.7 Start-up in Retrospect, Ideas in Prospect 237
5.7 SEEDCORN VENTURE CAPITAL 238
5.7.1 Introduction 238
5.7.2 Size and Scale - the Argument Surrounding Seed 238
5.7.3 People Outside of Finance 244
5.7.4 The Seedcorn Process 247
5.7.5 Seed - Insiders or Outsiders? 250

CHAPTER 6 : FOUR DISCOURSES .... A TALE OF SIX BOUNDARIES 253
6.1 AN INTRODUCTION TO THE DISCUSSION 253
6.2 FOUR DISCOURSE COMMUNITIES 255
6.2.1 Introduction 255
6.2.2 Moral Discourse 260
6.2.3 Industrial Discourse 261
6.2.4 Financial Discourse 263
6.2.5 Public Discourse 264
6.3 SIX BOUNDARIES 266
6.3.1 Introduction 266
6.3.2 The Moral-Industrial Boundary 267
6.3.3 The Industrial-Financial Boundary . . . 270
6.3.4 The Moral-Financial Boundary 272
6.3.5 The Moral-Public Boundary 276
6.3.6 The Industrial-Public Boundary 278
6.3.7 The Financial-Public Boundary 281

CHAPTER 7 : IDEAS, INNOVATION AND ACTION .... CROSSING THE BOUNDARIES. 285
7.1 INTRODUCTION 285
7.2 NETWORKS AND MYTHOLOGY 288
7.3 HOTOL CROSSING BOUNDARIES 293
7.4 PARSONS AND THE OTHERS CROSSING BOUNDARIES . . 305
7.4.1 Parsons 305
7.4.2 The Others 312
7.5 HOW LATOUR PERCEIVES BOUNDARIES 317
7.6 EPILOGUE - WHERE ARE THE BOUNDARIES? 324

CHAPTER 8 : THE TRACE OF A NEW IDEA .... DIFFÉRANCE IN TIME AND SPACE 328
8.1 INTRODUCTION 328
8.2 DIFFERENCES? IN SPACE AND TIME .... THE 'LONGUE-DUREÉ' OF INSTITUTIONS 331
8.2.1 Introduction - Concerning Levels-of-Analysis 331
8.2.2 Here & There 334
8.2.3 Now & Then 337
8.3 DAY-TO-DAY DISCOURSE AND 'REAL-LIFE' 341
8.4 FROM GIDDENS'S DUREÉ TO DERRIDA'S TIME OF WRITING 348
8.4.1 Introduction 348
8.4.2 Dureé 350
8.4.3 Intermezzo 360
8.4.4 The time of writing 364
8.5 THE TRACE OF A THESIS .... A SUMMARY 374

CHAPTER 9 : SOME POSSIBLE CONCLUSIONS 381
9.1 INTRODUCTION 381
9.2 THE METHOD 382
9.3 THE DATA 384
9.4 THE THEORY 386

BIBLIOGRAPHY i

APPENDICES xi

APPENDIX I - INTRODUCING THE RESEARCHER xi

APPENDIX II - THE PROVIDERS OF THEORY xv
II.I Economic Aspects of Innovation xvi
II.II The Venture Capital Literature xxvi

APPENDIX III - THE METHOD xxxi

APPENDIX IV - THE PROVIDERS OF IDEAS xxxv
IV.I Historical Data xxxvi
IV.II HOTOL xlii
IV.III Other Small Case Studies l

APPENDIX V - THE PROVIDERS OF FINANCE liv

APPENDIX VI - FOUR DISCOURSES lxiv

APPENDIX VII - CROSSING BOUNDARIES lxx
VII.I Interpersonal Networks lxx
VII.II Geographical Networks lxxiv

APPENDIX VIII - DIFFERENCES lxxx

APPENDIX IX - A SUPPLEMENTARY CONCLUSION lxxxix

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CHAPTER 1 : INTRODUCTION

So here I am, in the middle way, having had twenty years -
Twenty years largely wasted, the years of l'entre deux guerres
Trying to learn to use words, and every attempt
Is a wholly new start, and a different kind of failure
Because one has only learnt to get the better of words
For the thing one has no longer to say, or the way in which
One is no longer disposed to say it. And so each venture
Is a new beginning, a raid on the inarticulate
With shabby equipment always deteriorating
In the general mess of imprecision of feeling
Undisciplined squads of emotion. And what there is to conquer
By strength and submission, has already been discovered
Once or twice, or several times, by men whom one cannot hope
To emulate - but there is no competition -
There is only the fight to recover what has been lost
And found and lost again and again: and now, under conditions
That seem unpropitious. But perhaps neither gain nor loss.
For us, there is only the trying. The rest is not our
business. (T.S. Eliot 1974:202-3 - East Coker)

There seems to be a widespread perception that the people and industries of the United Kingdom, however inventive they may be today, are not very innovative. The notion that good technical ideas do not find commercial application seems to find ready acceptance. Sir Geoffrey Pattie (a government minister in the 1980s, interviewed in the course of this research) caught this national mood in evidence he gave to a House of Lords Select Committee:

> I think that it really should be the subject of a psychological study, this national psyche over some period of time. .... It is something to do with us here, that we start on certain programmes, then we get into a great panic about them, start saying 'We can't see where they are taking us' or 'What's going to happen? The costs are going to be terrible', and we come out of them. (Lords 1988:para.764)

This thesis is intended to be a study of ideas and of how they do, or do not, become major technological innovations. It is not purely, or simply, a psychological study - the

approach has been not to presuppose a theoretical or disciplinary standpoint, but to follow the paths into theory that the data suggested might be fruitful. This theory has tended towards sociological and anthropological critiques, but those of a sort sensitive to individuals as well as social collectivities. The shortcomings of these have led further still - into contemporary philosophical developments in the French structuralist tradition.

The choice of this subject for investigation grew out of the researcher's own experience of industry (further described in the appendix to this chapter), which had itself prompted questioning of the national competence at technological innovation. However, this thesis does not try directly to answer whether such questioning is rooted in fact, myth or fantasy - it approaches the issue with the more modest intention of simply illuminating the progress of ideas as they pass through diverse social milieux on their way to becoming innovations (or not).

The investigation uses the words of the people that have the ideas, and who become involved in their progress. These people were chosen in three different ways. First are those who are associated with a particular idea, interviewed to produce a case-study. Second are those who <u>were</u> so involved - who wrote letters to one another that allow construction of a case-history. Third are people involved in providing the funds without which an idea cannot survive.

The main case-study is of a proposal for a new way of launching satellites into space - the HOTOL project - ideas

still poised somewhere between success and failure. The case-history, very similar in many ways, but ultimately a pervasive innovation, looks at Charles Parsons's work with marine steam-turbines at the turn of the century. A few other interviews with technologists provide 'snapshots' of other ideas. The finance interviews concentrated on the City of London, but, aware of the possibility of national differences, also looked at New York.

This thesis tries to articulate all of these diverse threads, but - lest the result become too structural, inflexible, and moribund, in its form - it also tries continually to reintroduce the insistent voices of the people in the interviews to bring it back to life. It is their actions that constantly reproduce the structure by using it to turn their own ideas into action - to innovate.

The approach to data and to theory that has just been introduced is very evidently an eclectic one. This reflects not only the heterogeneous character of the data, but also the sense of fragmentation and disciplinary unease in the social theory of the last ten or twenty years. The next chapter considers contemporary theories, and outlines how they question and rework the legacy of their predecessors. This chapter introduces something of this long critical tradition, and, in the process, illustrates that an eclectic approach to theory can result in a coherent (and above all useful) whole.

Two broad social critiques will be used to do this - those of Kumar (1978) and of Harvey (1989). Kumar (from a sociological tradition) considers the industrial revolution, industry today, and what might take its place. Harvey looks at many of the same issues, but, writing ten years after Kumar, he has access to a very different focus, and very different theory, with which to approach the radical shifts that occurred around the turn of the 1970s. The present thesis comes out of a background of management scholarship that has used Kumar's sort of sociology. However, to understand its data it has been driven towards the ideas with which Harvey treats. The two critiques provide a primer in the use of these ideas, and set the historical and industrial scene.

The core of Kumar's (1978) book - 'Prophecy and Progress' - is the vast series of innovations that have come to be termed the 'industrial revolution'. The thought that shaped this change, and the thought that arose as a reaction to it, still largely define debate of such issues today. Kumar looks at the development of such thought, and its break with the classical tradition:

> the backward-looking spell of the memory of the world of classical antiquity remained, to bewitch thinkers into a sense that the great, golden age of man was really in the past .... This spell was decisively broken only towards the end of the seventeenth century. It came in the victory of the 'Moderns' over the 'Ancients' (Kumar 1978:14)

He adds - "With this victory .... the idea of progress became firmly established in the European mind" (p.14). Harvey

(1989), in his book 'The Condition of Postmodernity', takes up this thread:

> Enlightenment thought .... embraced the idea of progress, and actively sought that break with history and tradition which modernity espouses. (Harvey 1989:12)

Both writers define what the Enlightenment idea of 'progress' entails - Kumar sums it up:

> The great tool of this was to be science, especially social science. The social agency was to be the experts, the men of knowledge, the scientists, engineers, mathematicians and economists, with a leavening of those - bankers and industrialists - who could claim to be honest men of affairs with no political axes to grind and with special skills to offer. (Kumar 1978:44)

The core of Harvey's book is the question whether 'modernity' itself has now ended with the victory of 'postmodernism' - he thinks not, and in stating his argument outlines his own radical project:

> There has been a sea-change in cultural as well as political-economic practices since around 1972.
>
> This sea-change is bound up with the emergence of new dominant ways in which we experience space and time.
>
> .... But these changes, when set against the basic rules of capitalistic accumulation, appear more as shifts in surface appearance rather than as signs of the emergence of some entirely new postcapitalist or even postindustrial society. (Harvey 1989:vii)

Harvey accumulates and critiques postmodern art, literature, philosophy and above all social thought. Kumar critiques views of postindustrial society - views he dismisses as futurology. This thesis is ineluctably driven towards the same areas of debate as Kumar and Harvey, and their excellent discussion makes the extraordinarily problematic character of such debate very clear. Ideas labelled with words such as modernism, postmodernism, enlightenment, structuralism, and so on, are used in the present thesis, but such use demands

caution, and an awareness of others' rhetorical use of the terms.

Much of the debate reduces to whether differences between these ideas are definitional or substantial, as Harvey's continual playing of modernity against postmodernity well illustrates:

> the continuity of the condition of fragmentation, ephemerality, discontinuity, and chaotic change in both modernist and postmodernist thought is important. (Harvey 1989:44)

He poses a key question about postmodernism:

> Does it have a revolutionary potential by virtue of its opposition to all forms of meta-narratives (including Marxism, Freudianism, and all forms of Enlightenment reason) .... Or is it simply the commercialization and domestication of modernism ....? (Harvey 1989:42)

In answering it he considers French structuralist thinkers like Foucault and Derrida. This thesis will avoid any easy equation of French structuralism, particularly that of Derrida, with the problematic definition - postmodern; it will, however, draw heavily upon Derrida's ideas.

Sturrock (1979:1-3) discusses the birth of French structuralism in the Paris of the 1960s in much the same way that Kumar describes the birth of sociology in the Paris of a couple of centuries earlier. Kumar describes the ideas of Turgot, Condorcet, Comte (who named sociology) and especially Saint-Simon. Their thought reconceptualized time:

> The chronological line - past, present, and future - was barren as well as deceptive. Only the perspective of the future revealed what was important in the past, and linked it to our lives in the present. (Kumar 1978:14-15)

The French Revolution of 1789 was the epochal event that ushered in this new future:

> The very word 'revolution' was recharged, and given a new significance. Since classical times .... Revolution meant a turning back, or a turning round, as in the motions of a wheel. (Kumar 1978:19)
>
> It was in the course of the French Revolution that the word 'revolution' acquired its modern meaning, its modern associations of novelty and fundamental change. (p.20)

Saint-Simon saw the industrial revolution in the same revolutionary way - both revolutions were conceptualized under the rubric of 'progress'. Progress, nonetheless, was seen as occurring in stages - Kumar (1978:59-60) notes the appealing suggestion that whereas liberals see two stages - an imperfect past and a wonderful present - more critical thinkers see three stages, with the troubled present as a way to a wonderful future. The importance of the idea of stages here is twofold - firstly, it embodies social life as a series of 'presents' within a linear conception of time. Secondly it implies that ideas of history are moulded by the theories that use them - history is a rhetorical and ideological resource.

Harvey focuses on 'postmodernity' where Kumar looks at various 'post-industrial' critiques, but both question whether such futures are breaks with the past or not. This clearly suggests the relevance that a careful use of history might have to the study of innovation in this thesis. Harvey conceives these 'crises' of modern capitalist industrialism in terms of changes in conceptions of space and time - periods of 'time-space compression'. The Renaissance, with its voyages of discovery, maps and chronometers is a good example (Harvey

1989:241-249), the more so because it "laid the conceptual foundations in many respects for the Enlightenment project" (p.249).

Both writers make clear the range of human activities that was involved in industrialization - not merely finance, commerce and industry, but also the worlds of sociology, science, philosophy, and the arts. Both describe the crisis, and widespread cultural questioning, that occurred at the start of the twentieth century. Harvey cites many examples of the search for new ways of conceiving space and time - Einstein in physics, Joyce in literature, and Picasso and Braque in painting:

> abandoning 'the homogeneous space of linear perspective' that had dominated since the fifteenth century. (Harvey 1989:267)

He also notes the growth of museums and libraries, the "ideological labour of inventing tradition" (p.272), as well as the new interest in crafts represented by such as William Morris and art nouveau.

Harvey carries this analysis on to the present day, saying:

> This trend to privilege the spatialization of time (Being) over the annihilation of space by time (Becoming) is consistent with much of what postmodernism now articulates (Harvey 1989:273)

Central amongst Harvey's fine range of examples is Henry Ford; Harvey suggests that the twentieth century's progress from modernism to postmodernism can be represented as a passage from 'Fordism' to 'flexible accumulation', which he defines as:

> characterized by the emergence of entirely new sectors of production, new ways of providing financial services, new markets, and, above all, greatly intensified rates of commercial, technological, and organizational innovation. (Harvey 1989:147)

Kumar and Harvey both present their ideas with elegance, and in considerable detail. The purpose of this chapter has not been to explicate these ideas, but rather to use them to show the complexity and history of the subject matter of the present research. This thesis tries to understand how innovation happens, and it is very clear that it must be considered closely bound to its social and temporal context, and theoretically intertwined with current and past ideas in social science. It is hoped that this introductory chapter has concisely illustrated the task ahead, without over-simplifying it.

To summarize the approach it is appropriate to take up just a few of the hints that Harvey and Kumar drop in the course of their texts. Harvey's insistence on the centrality of space and time is worth noting again here - analysis of people's descriptions of chronologically ordered case-studies, or of the 'processes' by which they are funded, show regularities across, and outside of, space and time. Harvey also suggests where to look:

> if we are to look for anything truly distinctive (as opposed to 'capitalism as usual') in the present situation, then it is upon the financial aspects of capitalist organization, and on the role of credit that we should concentrate our gaze. (p.196)

This thesis considers the financing of innovation using many of the ideas that Harvey discusses. It is reassuring to note, in passing, the thread in contemporary management

scholarship that also sees the usefulness of 'postmodern' notions; Cooper & Burrell (1988) provide an excellent introduction, and Vargish's (1991:86) suggestion of postmodern money is an intriguing illustration[1]. Harvey adds social setting and discourse to this investigation, and provides a clear summary of the way to proceed:

> If we view culture as that complex of signs and significations (including language) that mesh into codes of transmission of social values and meanings, then we can at least begin on the task of unravelling its complexities under present-day conditions by recognizing that money and commodities are themselves the primary bearers of cultural codes. (Harvey 1989:299)

The approach adopted here will look at inventions, and their funding, through the discourses of people that are involved in them, and analyse them in terms of the cultural codes that they use and sustain. The aim is a deeper and more robust understanding of innovation than would be possible using other methods. Harvey and Kumar both show the need for such complexity and self-reflexivity in analysis:

> While crises in the experience of space and time, in the financial system, or in the economy at large, may form a necessary condition for cultural and political changes, the sufficient conditions lie more deeply embedded in the internalized dialectics of thought and knowledge production. (Harvey 1989:345)

> So with all problems. They come trailing the conventional wisdom of the very time and circumstances that gave rise to them, and which therefore constitutes precisely part of the problem. Inventing the future, allowing it its own freedom to find new solutions, cannot involve perpetuating the hold of the dead hand of the past. (Kumar 1978:327)

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[1] Dan Gowler's free conceptual explorations of the relevance of 'postmodern money' to present-day financial and economic issues have been seminal to this thesis. The limited development of this area here does not give his ideas, sadly never published, the prominence they deserve.

CHAPTER 2 : THE PROVIDERS OF THEORY - A REVIEW

.... Had they deceived us,
Or deceived themselves, the quiet voiced elders,
Bequeathing us merely a receipt for deceit?
The serenity only of a deliberate hebetude,
The wisdom only the knowledge of dead secrets
Useless in the darkness into which they peered
Or from which they turned their eyes. There is it seems to us
At best, only a limited value
In the knowledge derived from experience.
The knowledge imposes a pattern, and falsifies,
For the pattern is new in every moment
And every moment is a new and shocking
Valuation of all we have been. We are only undeceived
Of that which, deceiving, could no longer harm.
(T.S. Eliot 1974:198-9 - East Coker)

2.1 Introduction

This chapter cannot attempt a comprehensive review of the theoretical background to this thesis. Such a project is tempting, but it is incapable of execution within a limited space, and ultimately futile. This thesis, in using theory to understand technological innovation, deals in two classes of idea - the 'theoretical' ideas of philosophy and social science, and the 'practical' ideas of engineering and the natural sciences. Later chapters must allow the two to intermingle, each will inform the other; this chapter serves as theoretical reference merely.

The problem of arranging a 'literature survey', however, is considerable. An initial focus is provided by the importance of different 'world-views' that was implicit in the research interviews; this directs attention to literature which deals with how such world-views, or discourses, come to be constituted in people's minds and in society. However, to

categorize such literature is all but impossible - its key themes are extremely diverse, and the theorists who have engaged in some of the most fruitful debate encompass a wide range of traditional academic disciplines. The study of different discourses produces, in its turn, a polysemic academic discourse.

If organization of this chapter along purely historical, thematic or disciplinary lines is unwarranted, the only solution is the pragmatic one of incorporating elements of all of these into a framework that will suit the future development of this thesis. The literature will be discussed under four headings: historical legacy, anthropology, radical modernist sociology, and French structuralism. Many of the ideas considered would fit equally well into more than one category, even into all of them - a fore-warning of the problems of debate to come.

This theory emphasizes subjective, interpretive ideas of analysis, rather than quantitative positivistic ones. It is something of a paradox that the canons of thesis writing, by inviting an impossible taxonomy of theoretical knowledge, privilege what is included over what might have been included. It might, of course, be argued that supporters of the ideas that are 'de-privileged' here, have had a century and more to demonstrate their ideas' utility (which this thesis suggests they have failed to do), and that they still hold the high-ground as theory-in-use amongst most organizations, so it is high time that their ideas be de-privileged. But such ideas do still have their relevance and utility, and do not deserve

to be ignored altogether. For this reason the appendix to this chapter includes a very brief review of a few economic analyses and empirical investigations of innovation.

This chapter, then, is a theoretical introduction to the discussion of later chapters which will use it, amplify it, and, where appropriate, add more. This chapter inscribes something of a 'memory trace' with which the material to be introduced later might perhaps resonate.

2.2 The Theoretical Legacy

2.2.1 INTRODUCTION

Two writers are considered here - Durkheim and Saussure. Durkheim's ideas of empirically observable moral orders have been of great relevance to modern sociology and anthropology. Saussure's linguistics is the true basis of modern structuralism - seminal to what Atkinson (1990:6) terms "the 'linguistic turn' of the human sciences" in recent years (Cooper 1989:479 describes the parallel "'symbolic turn' in organizational studies").

Culler (1988:70) introduces the relevance of Saussure and Durkheim to the present enterprise, together with Freud[1]:

> These three thinkers revolutionized the social sciences by creating for their work a new epistemological context: that is to say, they conceived of their objects of study

---

[1] Freud, too, is very relevant to the material considered in this thesis - his ideas are important to both Giddens and Derrida (not to mention Lacan). However, since both of them, in discussing Freud, present good exegeses of his ideas, these ideas will not be further discussed in this section.

> in a different way and offered a new mode of explanation. (Culler 1988:70)

This 'new mode of explanation' is remarkably relevant to this thesis.

2.2.2 DURKHEIM

It is convenient to consider the work of Émile Durkheim through Giddens's monograph on him (Giddens 1978). The relevant area of Durkheim's work, here, deals with the sociological investigation of moral constraints - the moral orders that influence action. Durkheim espoused empirical methods, and applied them to his analysis of ethics:

> What we must do .... is to study empirically the various forms of moral code that exist in different societies. 'Moral facts', Durkheim claimed, 'are phenomena like any others ....' (Giddens 1978:21)

Giddens develops this, saying of Durkheim: "Social facts, he affirmed boldly, must be treated as 'things'" (Giddens 1978:35, see also Culler 1988:71), moreover: "Social facts are external to individuals and exert constraint over them" (Giddens 1978:36), and thus: "The properties of a totality cannot be deduced from those of the individuals who combine to form it" (p.36).

Central here is the problem of self-interest, in hunting out moral codes, Durkheim is rejecting the utilitarian philosophies of such as Spencer (Giddens 1978:22) - these see isolated individuals in economic exchange, but in so doing imply a moral order:

> in Durkheim's celebrated phrase, there is 'a non-contractual element in contract': the existence of

> contractual exchange presupposes moral authority, the authority which renders contracts binding. (Giddens 1978:10)

Morality is a social, not individual, phenomenon: "Morality derives from a source which goes beyond individual interests" (p.67).

Durkheim's comparative study of religion developed the empirical investigation of more secular moral codes:

> although the hold of religion over day-to-day life declines in the course of social differentiation, and the old deities disappear, the new ideals of moral individualism still preserve an intrinsically religious character. (Giddens 1978:81-82)

Durkheim sees religion and morality as social in origin - they come before, and have their effects in, human cognition:

> The cognitive formulation of religious ideas is an expression of pre-existing social sentiments which antedate their expression in conscious reflections (Giddens 1978:80)

This view of a social origin not only for morality, but also for cognition itself, prefigures the debate of Lévi-Strauss, Sperber, and Derrida, that later sections will take up.

Giddens (1978:85) discussion of Durkheim's search for a definition of religion bears upon the border between moral codes and everyday life. Durkheim sees all religion as differentiating the 'sacred' and the 'profane'[2] - ritual controls the passage between the two. Parallel to this dichotomy are Durkheim's views on the categorization of knowledge - a final element of relevance to this thesis:

> the idea of 'category' involves the formation of clear-cut logical oppositions or dichotomies. The concept of category is not an inherent property of the human mind;

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[2] Giddens (1978:102) stresses the problematic nature of this assertion, as he does many others (see ch.6).

> neither are the particular categories of space, time or causality .... Concepts are not, like experiences, particular to individuals; they are shared by members of the group, and must therefore have initially been derived from features of the life of the collectivity. (Giddens 1978:98)

This has considerable affinities with Saussure's ideas of language, and even those of Derrida (Giddens 1978:104-105 makes a similar point).

2.2.3 SAUSSURE

"Ferdinand de Saussure is the founder of modern linguistics" (Culler 1988:7). However, Saussure's 'Cours de Linguistique Générale' - the book upon which his reputation is based - was assembled from his students' lecture notes, by Bally and Sechehaye (Culler 1988:16-17). Beyond the pleasing curiosity of this fact, Culler (1988:17) points out a rather more pressing concern - subsequent scholarship, together with Saussure's other writings, modify and supplement certain of the important arguments of the 'Course'. Expert guidance is needed, so Culler's (1988) monograph on Saussure will be followed here (in particular the development of his chapter 2). As with Durkheim, only those of Saussure's ideas of later relevance in this thesis will be described.

The basis of Saussure's theory of language is simple - "Language is a system of signs" (Culler 1988:19):

> The sign is the union of a form which signifies, which Saussure calls the 'signifiant' or signifier, and an idea signified, the 'signifié' of signified. Though we may speak of signifier and signified as if they were separate

> entities, they exist only as components of the sign. The sign is the central fact of language (Culler 1988:19).

Saussure's first, and momentous, principle regarding signs is that they are arbitrary. "There is no natural or inevitable link between the signifier and the signified" (Culler 1988:19). Language is the structure that defines these links - but it too is subject to infinite change over time. Nor is there any reason why the signifieds of one language should be consistent with those of another - ideas themselves need not be translatable simply by swapping the signifiers of the two languages. <u>It is not only the link (the sign) that is arbitrary - the signifiers and the signifieds are completely arbitrary too</u>.

Saussure's relevance to the inventor trying to convey his ideas to potential supporters, begins to appear. Saussure's next step is the important principle that "both signifier and signified are purely relational or differential entities" (Culler 1988:23). A signified is recognized as conceptually distinct simply because it differs from similar concepts around it. The identity of a signifier resides simply in the fact that it is different from the next word, say, in a continuum of words. Many subsequent developments of Saussure's ideas derive from the corollary that there is no reason whatever why the signifier has to be a word, or a visual symbol, or a sound in speech - anything can signify:

> It may often be difficult to assign a precise form to those things that function as signs, but if a difference bears meaning for members of a culture, then there is a sign, however abstract, which must be analysed. (Culler 1988:51)

This promises an analytical project relevant far beyond written or spoken language:

> We can therefore imagine a science which would study the life of signs within society .... We call it semiology, from the Greek semeion ('sign').
> (Saussure - quoted in Culler 1988:90)

The ideal semiological system would consist of purely arbitrary signs, resistant to any ideological attempts to imbue them with 'natural' meaning - Saussure privileged linguistics as the best model, since it most nearly met this ideal.

The field of study for such a project needs to be defined, and Saussure's distinction between 'langue' and 'parole' is important here:

> 'La Langue' is the system of a language, the language as a system of forms, whereas 'parole' is actual speech, the speech acts which are made possible by language.
> (Culler 1988:29)

Langue is thus the object of linguistic study. But the analyst of language has to face the problem that, in actual use, in parole, language is changed. Saussure did this squarely, and in a way that has been "least clearly understood and least perceptively investigated by his successors" (Culler 1988:84). Saussure distinguished a synchronic and a diachronic perspective, and gave priority to the first:

> The fact that the sign is arbitrary or wholly contingent makes it subject to history but also means that signs require an ahistorical analysis. (Culler 1988:36)

Signs change historically - diachronically - yet they are arbitrary, so the only way to analyse them is in relation to other signs around them, such analysis therefore has to be fixed at a particular time - has to be synchronic. Much of

Saussure's radical force lies in this questioning of a teleological search for an origin and end in linguistic change.

The language that emerges from analysis has many levels, but at every level its units are still identified purely by virtue of their difference from one another. There are 'paradigmatic' classes of letters, words, and suchlike - but there are also 'syntagmatic' classes of the ways that letters can be combined to form words, words to form sentences and so on:

> the concept of a hierarchy of linguistic levels, in which the constituents of one level (such as phonemes) combine to form constituents of the next level (such as morphemes), and in which the combinatory potential of elements serves to define them, is common to a range of descriptive theories (Culler 1988:87)

Saussure's dichotomies of signifier-signified, langue-parole, synchronic-diachronic, and finally paradigm-syntagma, have provided his successors with sensitive and powerful tools for structural analyses. However, these dichotomies continually question and topple each other - the fecundity of such structural analysis resides largely in this inherent refusal to accept structure as an easy, or a simple, analytical construct.

2.3 Anthropology

2.3.1 SYMBOLISM - MEANING, BOUNDARIES AND TIME

The developments of anthropology and sociology through the twentieth century have provided two parallel tracks in the study of people in social settings. This section introduces the anthropological ideas that have been found very relevant and useful in this thesis. This sub-section begins to ask how the study of individual people and their multifarious social groups can proceed, and on what analysis should focus.

Many possible theoretical courses present themselves, so some initial statement of theoretical intent is necessary; in addition, various philosophically contentious words threaten to swamp debate here, and must be acknowledged. The basic intention is to accept Saussure's invitation to study the differences that bear meaning for people - the symbols that emerge from their discourse. To do this, words like empiricism, function, and meaning, have to be used with care. A pure empiricism must be avoided that would deny any philosophical or theoretical construct, but so must any functionalism that consists in such a construct, and sees everything cultural having a social purpose. Likewise, the concept of meaning can all too easily drag discussion into premature theoretical closure. This section, therefore, will defer the question of meaning, place it in 'phenomenological brackets' as it were. This is not to sweep it away as an empiricist might, or to raise it to a transcendental or metaphysical level, but merely to signal the need for caution

wherever the term is used - different theorists see meaning in very different ways.

Thus armed, anthropological ideas of symbolism, and of cultural analysis, can be sampled. The intention is to learn how to hunt the symbols that are salient to the people interviewed in research, regardless of what they signify or mean. All that is important - at this stage - is that these symbols seem rich in meaning. 'Community' is a word that includes just such an excess of meaning relative to words like 'group' or 'organization'. Cohen's (1985) short book - 'The Symbolic Construction of Community' is a useful vehicle upon which to base an introduction.

Cohen grasps the question of meaning early in his discussion - his standpoint is that:

> Symbols do not so much express meaning as give us the capacity to make meaning. .... But their <u>range</u> of meanings can be glossed over in a commonly accepted symbol - precisely because it allows its adherents to attach their own meanings to it. They share the symbol, but do not necessarily share its meanings. Community is just such a boundary-expressing symbol. (Cohen 1985:15)

What is characteristic of a community, then, is that:

> its members make, or believe they make, a similar sense of things either generally or with respect to specific interests, and, further, that they think that that sense may differ from one made elsewhere. (Cohen 1985:16)

Community "exists as something for people 'to think with'" (p.19). Cohen notes the distance from Durkheim's functional views of community as an integrating device here - he sees it as an aggregating device, as:

> a commonality of <u>forms</u> (ways of behaving) whose content (meanings) may vary considerably among its members

(Cohen 1985:20-21)

He adds that community "continuously transforms the reality of difference into the appearance of similarity" (p.21).

The members of Cohen's communities use symbols to mark the boundaries they perceive around them:

> The boundary represents the mask presented by the community to the outside world .... the boundary as the community's public face is symbolically simple; but as the object of internal discourse it is symbolically complex. (Cohen 1985:74)

The differential nature of the boundary recurs throughout Cohen's discussion, and through it the nature of the symbols that might be found can be approached - Cohen concludes by saying:

> Since the boundaries are inherently oppositional, almost any matter of perceived difference between the community and the outside world can be rendered symbolically as a resource of its boundary. (Cohen 1985:117)

Cohen sees symbols as consisting in difference, but not quite the arbitrary difference of Saussure - as his discussion of the importance of symbolic reversal makes plain:

> it has been argued that the very nature of symbolism itself contains not merely the competence of discrimination, but the sense of negation: in other words, that the very rationale of symbols is that they are different in some way from the entities they symbolize. (Cohen 1985:58)

He develops this point in a rather functional way, of some interest here:

> People create a symbolic world which is a kind of fantastic reconstruction of empirical society: the dialectical contrast between the two is resolved by a reassertion of the inevitability and desirability of the first through recognition of the fantasy and impossibility of the second. Through such symbolic behaviour, people draw the conventions of community about them, like a cloak about their shoulders, to protect them from the elements - other people's ways of doing things,

other cultures, other communities. (Cohen 1985:63)

Babcock (1987) makes very similar points in her introduction to a book dedicated to such reversible symbols - they allow: "a space in which to take chances with rôles and ideas" (p.25). Her comment that symbolic inversions "remind us of the arbitrary condition of imposing an order on our environment and experience" (p.29) once again evokes Saussure.

Communities exist across time as well as space; this dimension is well introduced in Leach's (1961) two short essays on time. Leach differentiates a mathematical linear conception of clock time from a cyclical oscillation of day and night, of the seasons, of death and re-birth. Leach discusses the "orthodox Durkheimian" view where: "the year's progress is marked by a succession of festivals" (p.134). A festival arrests the normal flow of secular time, there is a rite through which the person passes from the everyday world of the profane into a sacred state (sometimes associated with role-reversal) where "ordinary social time has stopped", another rite of 're-birth' follows, and so a return to ordinary secular life. Leach notes the similarity of festivals to 'rites-de-passage' - the symbolism associated with passing from one community to another.

All this raises questions that might be characterized as ones of form versus content. Cohen sees commonality of form in symbolism, but meaning as individually malleable - community as a mental <u>construct</u>. Leach, on the other hand,

follows Lévi-Strauss in implying something much deeper and elementary in the human mind:

> I would maintain that the notion that time is a 'discontinuity of repeated contrasts' is probably the most elementary and primitive of all ways of regarding time. (Leach 1961:134)

2.3.2 LÉVI-STRAUSS AND STRUCTURALISM

A mere flavour of the richness and breadth of Lévi-Strauss's ideas will have to suffice here. The fact that they would fit equally well, and are mentioned, elsewhere in this chapter, and in other chapters, is testimony to their relevance.

Sperber (1979:49) says that Lévi-Strauss's use of structural linguistic ideas was a reaction against the empiricism dominant in social science[3]. Boon (1985:162) says that: "Lévi-Strauss approaches human forms as language"; he goes on to discuss the structuralist use of linguistic codes in social analysis:

> Such codes interrelate values and practice; they establish terms of exchange among different social divisions and cultural categories; and they constrain the possibilities of translation across languages and cultures. In the extreme, structuralism equates life, or knowledge of it, with language .... It is intent on how things signify, or the way societies produce meaning, not truth. (Boon 1985:169)

There are two key points here - first, the elements of symbolic codes are arbitrary and signify in a Saussurean

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[3] Although Sperber judges structuralism: "an uninspiring frame for an otherwise stimulating and inspired picture" here (Sperber 1979:25).

sense:

> Lévi-Strauss views all cultural forms as 'necessary illusions', systems of signification substituted for experiences that cannot be communicated, cannot be 'known', directly, however they are lived. (Boon 1985:171)

The second point is that Lévi-Strauss sees a mental process underlying symbolic and cultural forms:

> Lévi-Strauss views everything as pulsating cycles coding both affect and intellect, a rhythm repeated in his works' organization. (Boon 1985:172)

Lévi-Strauss (1963:206-230) takes up Saussure's distinction between langue (belonging to reversible time) and parole (non-reversible time), and arranges the basic elements of myth - mythemes - in a matrix accordingly. He observes that meaning lies in the assembly of these language-like elements - the true constituent units of myth are the synchronic regularities of these elements. Moreover these constituents are related to each other in a consistent way - "contradictory relationships are identical inasmuch as they are both self-contradictory in a similar way" (p.215). The end result is that:

> By systematically using this kind of structural analysis it becomes possible to organize all the known variants of a myth into a set forming a kind of permutation group, the two variants placed at the far ends being in a symmetrical, though inverted, relationship to each other. (Lévi-Strauss 1963:223)

In this way "mythical thought always progresses from the awareness of oppositions towards their resolution" (p.224) - a sort of 'slated structure' is formed as: "myth grows spiral-wise until the intellectual impulse which has produced it is exhausted" (p.229).

Sperber (1979:34) observes that myths: "should provide an exceptional insight into the spontaneous workings of the human mind", and that Lévi-Strauss is here: "the explorer of a mental continent", unearthing patterns by comparing myths which gives: "the exciting suspicion that the fleeting shapes and contours one can glimpse through the mist are those of a true 'terra incognita'" (p.40). Lévi-Strauss ties cultural forms to the deep structures of the human mind - not in an empirical way, or one subject to functionalist dangers of teleology - but through the rationally based ideas of linguistics. Sperber catches the tenuous nature of this vision, as does Boon when he cites the world-weary style of Lévi-Strauss's (1989) 'Tristes Tropiques', and when he concludes of Lévi-Strauss:

> He describes both his own studies and cultures themselves as means of setting in abeyance not <u>anomie</u> but <u>ennui</u> (Boon 1985:175)

2.3.3 SPERBER'S COGNITIVE CRITIQUE OF LÉVI-STRAUSS

Sperber is an admirer of Lévi-Strauss, but not of structuralism. He sees Lévi-Strauss's linguistic view of symbolism as misplaced, and suggests an alternative cognitive approach. In some ways this mirrors Derrida's philosophical reservations, which are considered at the end of this chapter. Sperber's (1975) book - 'Rethinking Symbolism' - starts by reviewing two ideas of symbolism:

> According to the first criterion, the symbolic is the mental minus the rational; according to the second, it

> is the semiotic minus language. In one case as in the other, it is a residue. (Sperber 1975:1)

Sperber carefully considers, but ultimately rejects both views. He then turns his incisive gaze on structuralism, adopting, as a rather bold starting point, the suggestion: "that the notion of the symbol .... be removed from the vocabulary of the theory of symbolism" (Sperber 1975:50). Sperber dismisses semiotic use of signification as simply shifting the question from <u>what</u> symbols mean to <u>how</u> they mean.

Sperber points out the seeming paradox that structural linguistics is based in arbitrary difference, yet the interpreter still has to <u>choose</u> between different interpretations (it is also at this point that Derrida's applies his force, as will be seen). The interpreter thus has to claim: "that his partially explicated intuitions more or less match the unconscious intuitions that are the basis of symbolism itself" (p.68).

Sperber applauds Lévi-Strauss's refusal to make this choice (and thus his implied disavowal of his own semiological foundations), observing that: "the other homologies are kept in reserve and not abandoned" (p.70). Symbolism thus becomes a means of organizing information, <u>not</u> encoding it, and Lévi-Strauss like a: "'bricoleur' gathers objects, various odds and ends of which he may always make something but never just anything" (p.71). Sperber summarizes:

> The device that would generate myths depends on an external stimulus; it is thus similar to cognitive devices and opposed to semiological devices: it is an interpretive, and not a generative, system. (Sperber 1975:82-83)

Sperber outlines his own, cognitive, theory of symbolism. Knowledge of the world in the brain is seen as true, guaranteed by experience or tradition, it is 'encyclopaedic' knowledge. However, belief is also sensed as true, though not guaranteed in this way - it is 'put in quotes' in the brain. This is symbolic knowledge, which is:

> neither about words nor about things, but about the memory of words and things. It is a knowledge about knowledge (Sperber 1975:108)

Thus: "the symbolic mechanism is the 'bricoleur' of the mind" (p.113), it puts things that are not explicable in quotes, and then invents:

> a relevance and a place in memory for them despite the failure in this respect of the conceptual categories of meaning (Sperber 1975:113)

It does this by focusing on the cause of this failure, and then evoking the field of memory that this focus delimits - much as a smell can evoke an old memory.

These evocational fields are localised, possibly idiosyncratic, even though those of different individuals and groups may overlap - thus: "Cultural symbolism creates a community of interest but not of opinions" (Sperber 1975:137). Sperber's underlying ideas of the universal cognitive focal structure here are close to Lévi-Strauss's ideas of the deep structure of the mind. Sperber's recasting of symbolism within an individual cognitive context, is of considerable relevance to the concerns of this thesis.

To summarize Sperber's useful ideas then - first of all he advocates removing "the absurd idea that symbols mean" (p.85) - he sees:

> No meaning in universal myths, but, broadly, a universal focalisation, a cultural evocational field, and an individual evocation. (Sperber 1975:140)

This symbolic mechanism acts as a feedback device, and takes over when conceptual mechanisms fail - when paradox is encountered. It then reconstructs the memory that is evoked to resolve the paradox, these:

> conceptual representations are probably not extracted from passive memory, but rather reconstructed by means of the traces left by previous acts of construction. (Sperber 1975:141)

This feedback cycle is thus continually evoked - "the repetitive side of cultural symbolism is there to set the endless evocation periodically in motion again" (p.145).

2.4 Radical Modernist Sociology

2.4.1 INTRODUCTION - DISCOURSE AND RELATIVITY

This section further broadens the review of theory - it looks at the ideas of modern sociologists writing within a radical tradition that sees the dilemma of the individual versus society not merely as problematic, but as needing resolution within sociological theory. Central here is the work of Anthony Giddens, and his theory of structuration:

> In structuration theory a range of dualisms or oppositions fundamental to other schools of social thought are reconceptualized as dualities. In particular, the dualism of the 'individual' and 'society'

> is reconceptualized as the duality of agency and structure. (Giddens 1984:162)

Language is an obvious link between the individual and society, and the structural linguistic tradition recurs here. Linguistic ideas have been taken up in sociology's use of the concept of discourse as an analytical tool - a use that will be followed in later chapters of this thesis. Dant (1991) provides a detailed survey linking structuralist conceptions of discourse to Mannheim's sociology of knowledge and to Marxist ideas of ideology[4]. He sees both knowledge and ideology as capable of analysis through discourse:

> As a theoretical category 'discourse' does not do the same work as the category of 'knowledge' or 'ideology' but it does describe an empirical phenomenon where knowledge and ideology are effectively produced. (Dant 1991:195)

Dant defines discourse:

> By discourse then, I mean the material content of utterances exchanged in social contexts that are imbued with meaning by the intention of utterers and treated as meaningful by other participants. (Dant 1991:7)

This definition might better be rendered: the material context of utterances exchanged in social interaction that may or may not be intended or treated as meaningful by the participants. Dant's definition is too limiting, and analytically impractical - loading it with unobservable concepts like meaning and intention precludes easy empirical use. Nonetheless, his emphasis on the analysis of discourse

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[4] Wuthnow's (1987) thoughtful book - 'Meaning and Moral Order' - provides an alternative, and more culturally based, conception of ideology here.

within a broad social context is important - to analyze 'langue', it must first be extracted from 'parole'.

The views of other theorists amplify the potential power of discursive analysis. Discourse is important to Giddens's analysis, and central to Derrida and Foucault (considered in the next section) as Dant (1991:ch.7) discusses, although his somewhat peremptory dismissal of Derrida (p.138) is unfortunate. There is a perhaps surprising measure of agreement between Giddens, Foucault and Derrida, that discourses must be seen in a full sense - both methodologically and theoretically - as embodying and being embodied by the social context that produces them, and that they produce. Discourse is a proper subject of analysis - reflexive analysis.

This leads to the issue of the status of such theorizing - theorizing that, as Derrida says of Husserl, has "to navigate between the Scylla and Charybdis of logicizing structuralism and psychologistic genetism" (Derrida 1978:158). The response of various sociologists has been an appeal to 'reflexivity' - to 'self-reflective' individuals. The problem is that such analysis is open to charges of relativism (what Berger and Luckmann 1967:17 describe as "the vertigo of relativity" in their famous contribution to the sociology of knowledge). Dant, however attacks this assertion:

> The spectre of relativism that accompanies reflexivity is often seen as a weakness but the ability to treat itself in the same way as the knowledge it studies means that the sociology of knowledge can never merely be an excuse for judging the merits and values of different types of

knowledge. (Dant 1991:4)

Dant's way out is to follow Mannheim's notion of 'relationism' - of situating the reflexive individual within the social and historical context of 'real-life'.

> Whereas absolutism evaluates according to the truth/falsity of the contents of knowledge, and relativism merely recognizes the impossibility of such an evaluation because of the multiplicity of truths, relationism eschews an evaluative approach in favour of understanding knowledge in its social context. The relationist position claims that knowledge is by no means illusory but is real and effective in guiding the business of life. (Dant 1978:17).

The question of reflexivity is a very current concern in sociology, and one final 'modernist' development must be mentioned in any debate about relativism. Jürgen Habermas uses structural linguistic ideas within the radical Marxist tradition of the Frankfurt School's 'critical theory'; he tries to get around the problem of the conceptual rigidity of earlier Marxist sociology:

> For if human behaviour is governed by ineluctable laws, there is nothing we can do to shape our own history by actively intervening in it. When understood as a science, Marxism ignores what Habermas calls the 'self-reflection', or 'reflexivity' of human agents. (Giddens 1985:125)

Habermas seeks to reconcile this positivism with a more hermeneutic approach. He uses the concept of communication (drawing on the structural linguistics of Chomsky and Searle - Wuthnow et.al. 1984:184-187), and introduces the idea of an 'ideal speech situation' where communication is "meaningful, true, justified and sincere" (Giddens 1985:129). Habermas situates these claims to effective communication in domains of

external nature, society, subjectivity and language (Wuthnow et.al. 1984:206-207); all of these have to be considered, thus avoiding any charge of positivistic reductionism say Wuthnow et.al.. There are clear parallels here with Giddens's ideas of knowledge claims within different discourses, mentioned in the next sub-section.

Giddens emphasizes that Habermas's project is a moral concern, and one based upon rationalism, though not the debased positivistic form of rationalism that Wuthnow et.al. describe:

> Reason is now that type of thought which assists in pragmatically selecting alternative means for the most efficient attainment of a given end. The broader rationalistic agenda that inspired the 'philosophes' during the enlightenment, by way of comparison, has been lost. (Wuthnow et.al. 1984:192)[5]

These theorists of radical sociology insist on a complexity in social life beyond positivistic attempts at reductionism. The data of this thesis insists on such a complexity to. This thesis follows them, and adopts the view that knowledge and truth are both local to a discourse - interpretation cannot say what they are in an absolute sense, but it <u>can</u> see how they are used in the actions of individuals that share the discourse.

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[5] Cooper & Burrell (1988:97) similarly mention Habermas, "whose project has been to reclaim the spirit of enlightened rationalism for late modernism".

2.4.2 GIDDENS AND 'STRUCTURATION'

Most of the issues that have come to the fore thusfar are covered by Anthony Giddens's theory of Structuration - it theoretically underpins much of the analysis of this thesis. This sub-section provides an overview of Giddens's ideas, supplemented by a few more specific considerations of areas of particular relevance, or areas where this thesis diverges from Giddens.

A sort of manifesto for Giddens's project can be set out as follows. He considers the social context of action; in doing so he embraces ideas of system and structure without attempting in any way to dodge their problematic nature. Without compromising his own views, he conceives of this social context in a way sympathetic to modern linguistic and semiological concerns, and to the notion of discourse. He refuses an atomist view of the individual in society, not only considering the cognitive and psychological bases of social interaction, but also - and this is of central importance in this research - the question of agency, the individual human being's capacity for intentional action. In linking the individual and the social context he fully conceptualizes such necessary categories as knowledge, history, and change, while at the same time incorporating sophisticated formulations of time and space drawn from anthropology, history and geography. The human being is reflexively implicated in the social world. Giddens holds the two sides of the debate together through his insistence on their duality, not their dualism.

The range of Giddens's conceptualizations captures something of the complexity of 'real-life'. Their rigour lies in their very lack of conceptual rigidity. Giddens says that his ideas provide an orientation with which to approach the analysis of data, more than a method, but few other sociologists lay out the whole ground of social critique so fully or so well.

Giddens thoroughly explicates structuration[6] in "The Constitution of Society" (Giddens 1984 - he provides a useful summary on pages 281-284). Its philosophical basis is, he says, ontological, not epistemological - a concern with human being rather than with relativism and verification (Giddens 1984:xx). He sees its theoretical basis in a dualism between, on the one hand, the 'three -isms' of functionalism, naturalism and objectivism, and, on the other, the subjectivism of critical responses to them, like hermeneutics and phenomenology[7]. As to "The basic domain of study" - Giddens defines this as:

> neither the experience of the individual actor, nor the existence of any form of societal totality, but social practices ordered across space and time.
> (Giddens 1984:2)

Thus Giddens starts from the position that: "human beings are knowledgeable agents" (p.281), and that this

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[6] "an unlovely term at best", but "I have not been able to think of a more engaging word" (Giddens 1984:xvi).

[7] For a discussion of modern hermeneutic thought see Outhwaite (1985) on Gadamer. The discussion in the next chapter of ethnomethodology is relevant to the phenomenological tradition.

knowledgeability is embedded in practical consciousness, bounded on the one side by the unconscious, and on the other by unintended consequences. The next point is that understanding how social life is reproduced needs the study of repetitive day-to-day life - conceived in reversible time, for which Giddens uses the term 'dureé'. Routine (that is unmotivated) behaviour is important both in the continuity of social life and in providing ontological security for those involved in it. The physical constraints upon human beings, and their paths through space and serial time also demand study in interpreting social systems. In this, symbolic boundaries are important, as is the co-presence of actors, and their awareness and reflexive use of these factors. In addition the social identities of actors are structural 'markers' implicated in their rights, obligations and rôles within groups.

Giddens debates the problematic nature of the concept of 'constraint'; he is also careful in his use of the terms system and structure - structural properties specify types of social systems, but such systems are very variable. At the level of whole societies, he calls the defining properties principles, but even here he is at pains to caution against either the notion that society is necessarily totally inclusive or that it can be easily defined or equated with a particular nation state.

Giddens sees power as basic, elemental, to social science - it cannot be added later, but neither is it any more essential than other primary concepts that relate action and

structure. Moreover, his conception of power is broadly considered - power is not merely divisive - action can be facilitated by empowerment as well as inhibited by conflict. Finally, Giddens draws no veil to protect sociological enquiry from lay thought - actors are experts in their own social worlds, and can (and do) use what sociologists may learn if it seems useful to them.

Structuration theory is clearly a comprehensive conception; closer scrutiny will be confined to three areas - the individual, structure, and their mutual implication in social institutions. In the first, agency, cognition, and knowledge will be considered, in the second there is a deliberate semiological bias; the third area implicates the whole of structuration theory, nonetheless, a tight focus upon space, time and institutions will be maintained. Along the way, a weather eye will be kept open for Giddens's ideas of discourse. All of these aspects intermingle throughout Giddens's discussion, as he says:

> A book has a sequential form, which can be overcome to some degree by 'circulating in and out' of a range of connected issues (Giddens 1984:163)

Individual agency is important here because innovations do not just 'happen' - people make them happen (Giddens (1984:346) discusses this). No conception that stops with the 'systemness' or 'structurality' of innovation can be adequate. For Giddens:

> Agency refers not to the intentions people have in doing things but to their capability of doing those things in

> the first place .... Agency concerns events of which the individual is the perpetrator .... Whatever happened would not have happened if that individual had not intervened. (Giddens 1984:9)

This denies the researcher easy attribution of odd happenings to 'structural' pathologies (indeed, seen thus, it probably denies much of contemporary management theory). Rather, the researcher must look at action and at actors' rationalizations of their actions - as Giddens says, "The 'dureé' of day-to-day life occurs as a flow on intentional action" (p.8). However, the agent's control over action is limited by unintended consequences - consequences of myriad actions intermingle as they spread wide in space and time, so easy assignment of agency becomes impossible.

Questions of cognition and consciousness are important in all this, and Giddens draws upon these areas throughout his discussion. He differentiates the unconscious from 'practical' and 'discursive' consciousness (pp.5-8). The agent's knowledgeability is contained in practical consciousness, whereas: "Discursive consciousness means being able to put things into words" (p.45). Discursive consciousness is clearly implicated in sharing knowledge - Giddens discusses the mode of articulation of knowledge, by which:

> I mean to refer to how far belief claims are ordered in terms of overall 'discourses' and to the nature of different discourses. Characteristic of most common-sense, everyday claims to knowledge is that they are formulated in a fragmentary, dislocated way. It is not only the 'primitive' who is a 'bricoleur': much day-to-day talk among lay members of all societies is predicated on claims to knowledge that are disparate or left unexamined. (Giddens 1984:92)

This plural nature of discourses, and the implication that they intermingle unchallenged, if not unperceived, is an important idea for this research. It has clear implications for interpretation:

> The reason why it characteristically makes more sense to speak about 'knowledge' rather than 'belief' when speaking of how actors find their way around in the contexts of social life is that the generation of descriptions demands the bracketing of scepticism. Beliefs, tacit and discursive, have to be treated as 'knowledge' when the observer is operating on the methodological plane of characterizing action. (Giddens 1984:336)

A final aspect of individual interactions that exercises Giddens is the question of 'presentation-of-self' implicit in Giddens's conception of individual action as:

> a continuous process, a flow, in which the reflexive monitoring which the individual maintains is fundamental to the control of the body that actors ordinarily sustain through their day-to-day lives. (Giddens 1984:9)

Giddens draws upon Goffman's ideas here. Actors have control over their own performance in social situations:

> Concern with appearance is manifest, for example, in the care with which an individual selects and arranges types of clothing or adornment in relation to participation in particular contexts of activity. (Giddens 1984:79)

This has as much relevance to the researcher as the researched, as the next chapter will discuss; in both cases, as Giddens says, "The point is that the sustaining of 'being seen as a capable agent' is intrinsic to what agency is" (p.80)

Moving on to consider structure requires a great deal of care (Giddens's 1984:207-221 well introduces the issues). Conceptions of structure are many and various, and tend to be

stultifying in insensitive hands, so it is vital to draw out the differences between Giddens's view and the more semiological conception adopted in this thesis, particularly in relation to discourse.

Giddens sees, in his carefully considered separation of structure from system, a resolution to the problems of other views, which he implies elide the two:

> In analysing social relations we have to acknowledge both a syntagmatic dimension .... and a paradigmatic dimension .... In structuralist traditions there is usually ambiguity over whether structures refer to a matrix of admissable transformations within a set or to rules of transformation governing the matrix. (Giddens 1984:17)

Giddens's use of these two dimensions from Saussure's own work does rather seem to be stealing his stick and then beating him and his successors with it[8]. Giddens, view of structure carefully emphasizes rules, and the distinction between structure and systems:

> Structure, as recursively organized sets of rules and resources, is out of time and space, 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. (Giddens 1984:25)

The inclusion here of considerations of time, memory, and the absence of the subject, parallels French structuralist developments of semiology. But in his incorporation of resources, as well as rules, differences of view start to emerge - for Giddens a semiological conception of what would

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[8] See Culler 1988:48-49 and especially pp.87-88, where he discusses the problems of the paradigmatic/syntagmatic distinction.

constitute 'resources' would be far too limiting. His view is that:

> we have to guard against the association of semiotics with structuralism and with the shortcomings of the latter in respect of the analysis of human agency (Giddens 1984:31)

His final demolition of structuralism clearly privileges the semantic over the semiotic:

> The foundation of a theory of meaning in 'difference' in which, following Saussure, there are no 'positive values' leads almost inevitably to a view accentuating the primacy of signs. .... The 'retreat into code' - whence it is difficult or impossible to re-emerge into the world of activity and event - is a characteristic tactic adopted by structuralist and post-structuralist authors. Such a retreat, however, is not necessary at all if we understand the relational character of the codes that generate meaning to be located in the ordering of social practices, in the very capacity to 'go on' in the multiplicity of contexts of social activity. .... Even the most complicated semiotic relations have a grounding in the semantic properties generated by the rule-governed properties of daily activities. (Giddens 1984:32)

This is the nub of the difference - Giddens accepts signification, <u>but only as one out of three</u> structural dimensions of social systems - structures of signification must be considered in connection with the other two: domination and legitimation.

This thesis will propose a 'retreat into code<u>s</u>' that might in fact allow an advance beyond Giddens's concerns here. Giddens, himself, uses 'codes' in the plural in the next sentence, and the possibility of signification in <u>multiple</u> codes may well allow access to activity and event by routes other than Giddens's own. The real activities and events considered in this thesis will allow this proposition to be tested.

This leads to one further point of divergence. Within the Saussurean semiological tradition, symbols, like anything else, can signify - Giddens, however, excludes symbols from signification:

> the 'signs' implied in 'signification' should not be equated with 'symbols'. Many writers treat the two terms as equivalent, but I regard symbols, interpolated within symbolic orders, as one of main dimension of the 'clustering' of institutions. Symbols coagulate the 'surpluses of meaning' implied in the polyvalent character of signs .... Symbolic orders and associated modes of discourse are a major institutional locus of ideology. (Giddens 1984:32-33)

Giddens sees structuralism as deficient in its consideration of agency, and separates semiotics from it, but adds 'resources', in his 'duality of structure' solution. This thesis will consider 'resources' and semiotics together with their social contexts, and call the result discourse. The synchronic regularities of structure that emerge if a Saussurean analysis of this discourse be allowed, can still maintain a place for agency through Saussure's own duality of langue and parole. Parole can be seen as the result of actors conscious use of the resources of discourse. As Giddens says:

> there is no such entity as a distinctive type of 'structural explanation' in the social sciences; all explanations will involve at least implicit reference to the purposive, reasoning behaviour of agents and to its intersection with constraining and enabling features of the social and material contexts of that behaviour. (Giddens 1984:179)

Seen thus, the different conceptions of structure, reduce to simple differences of definition and terminology. With care over such definition, the bulk of Giddens's potent social analysis is still available for use in the development of this thesis.

Giddens's conception of institutions, and their links with time and space, provides a final illustration of this potency. He distinguishes the reversible time of day-to-day 'dureé' from the irreversible time of the individual's life-span, and both from the 'longue-dureé' of institutions - again in reversible time. Institutions exist within societal totalities:

> Those practices which have the greatest time-space extension within such totalities can be referred to as <u>institutions</u> (Giddens 1984:17)

Giddens's careful separation of different forms of institution, and his definitions of social collectivities are worthy of note - he differentiates associations, organizations and social movements (pp.199-200). He also evokes something of Durkheim in introducing people's problematical conceptions of history:

> Modern organizations and social movements operate in a social world in which the retreat of the gods and the dissolving of tradition create the conditions in which reflexive self-regulation is manifested as history - and as sociology. The modern era .... is one marked by the prevalence of <u>historicity</u>. (Giddens 1984:203)

This shows the relevance of introducing Harvey's and Kumar's historical perspectives in the last chapter, as well as the need to term Giddens a <u>radical</u> modernist in this one.

One of Giddens examples of an institution is a happy one for this thesis, and provides a good point at which to leave him for the moment:

> Anyone who doubts the influence of the differentiation of space and setting in shaping and reflecting social patterns should ponder the position of the 'City' in Britain. Its spacial districtiveness from centres of 'industry', and its sheer concentration in one area,

> express major institutional characteristics of the society of which it is a part. (Giddens 1984:153)

Giddens use of Ingham's research here, is a fine example of the need for wide-ranging theory in the analysis of complex social situations; as Giddens says:

> They can be adequately understood only in terms of shifting allegiances and coalitions between strategically placed groupings of individuals, sometimes having outcomes that none of them intended. (Giddens 1984:325)

Giddens is modest about the place of structuration theory in empirical research such as this, which he sees as prey to "a welter of abstract concepts" (p.327):

> The concepts of structuration theory, as with any competing theoretical perspective, should for many research purposes be regarded as sensitizing devices, nothing more. (Giddens 1984:326)

He is too modest.

2.5 French Structuralism .... and Beyond?

2.5.1 INTRODUCTION - STRUCTURALISM, MODERNISM AND RATIONALITY

Sturrock (1979) includes Lévi-Strauss, Barthes, Lacan, Foucault and Derrida as the five key thinkers in his book on French structuralism. Lévi-Strauss has already been considered, the challenging writings of Barthes, Lacan and Foucault will be mentioned in the next sub-section, and Derrida considered in some detail in the final one of this chapter. The five writers are very different, but their ideas do all share a basis in the structural linguistics of Saussure.

Starting a discussion with definitional matters is never pleasing, but in the case of French structuralist thought it is necessary if confusion is to be successfully skirted. It will also bring out important themes that introduce later debate. The key words that need definition here are 'structuralism', 'modernism' and 'rationality'. These powerful terms are appropriated by different people at various times, and have come to trail clouds of popular definitions in their wakes. The problem is doubled by the frequent habit of adding the prefix 'post-' to them.

Structuralism is a widely used term. This thesis uses it in the restricted context of French structuralist ideas. Even here problems arise - Sturrock (1979:3) is careful to differentiate a structuralist way of looking at the world, from the cult of 'Structuralism' that grew up around such thought in 1960s Paris. Culler adds the problem of:

> the facile distinction between structuralism and post-structuralism, which may caricature structuralism and transfer to post-structuralism what is most interesting in structuralist writings. (Culler 1988:6)

Like Culler, this thesis will not differentiate the two terms; structuralism will be taken to mean thought in the very broad semiological[9] tradition that Saussure founded.

There are comparable problems in defining 'modernism' and 'postmodernism'. For the purpose of this thesis, modernism is used - suitably qualified as radical modernism - to refer to

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[9] See Sturrock (1979:7-8) for the distinction between the European term, semiology, and the US term, semiotics.

the ideas discussed in the last section. Radical modernism, then, is a tradition parallel to, but very distinct from, the structuralism considered in the present section. As such it is still a diverse definition - radical modernism might be taken to include cubism, writers like Joyce, modern Marxism of the Frankfurt School, right through to the sociology of Habermas and Giddens. Nonetheless, radical modernism certainly excludes other ideas that 'modernism' sometimes evokes, such as scientific management, Fordism and corporatism. Harvey's (1989) discussion is again useful here, in many ways his project is to define 'postmodernism' - a term that seems to have appropriated much of Sturrock's cult of 'Structuralism'. This thesis will skirt arid debate here, and take postmodernism simply to refer to that particular form of social critique that structuralist thought has spawned and that Harvey describes so well.

The word that defies definition is 'Rationality' - unlike 'structuralism' or 'modernism', it is less a collection of ideas or a social movement than a concept in its own right. Yet it is a concept uniquely liable to co-optation by the rhetoric of social movements. The last chapter discussed the Enlightenment, and it could be argued that the Enlightenment was itself appropriated by the much more positivistic project of modernist industrialism, so that anything beyond the proscribed compass of the latter became, by definition, unreasonable - irrational. Habermas's project of reclaiming Enlightenment reason was noted above, as Derrida's attempts to reclaim pre-Socratic rationality will be below. It is thus

necessary to resist closing debate, and to leave the question of reason open in what follows. This does not, it should be emphasized, licence debate to be irrational.

One final point remains about the five subjects of Sturrock's book - the point upon which Sturrock himself ends:

> the prose style in which those subjects have chosen to present their case to the world .... is a style in most cases of some, even extreme difficulty (Sturrock 1979:15)

Sturrock grades the order of difficulty from Barthes, through Foucault and Derrida, to Lacan[10]: "something of a legendary case" (p.16). However, this 'extreme difficulty' is central to the French structuralist project:

> They write, it should be remembered, in a country where clarity ('la clarté') has been regarded as a national virtue (Sturrock 1979:16)

> Writers like these know exactly what they are doing when they offend so egregiously against the canon. They are demonstrating that there is far more to language than lucidity. (p.17)

Derrida's own translator, Alan Bass, gives encouragement, if a little qualified, to proceed into their thought:

> despite Derrida's often dense and elliptical style .... It has been my experience that however syntactically complex or lexically rich, there is no sentence in this book that is not perfectly comprehensible in French - with patience. (Derrida 1978:xiv)

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[10] Derrida's comments upon first approaching Lacan's 'Écrits' strike a revealing note here: "I had not only to acquaint myself with it, but also to engage myself .... in a labor that announced itself as out of proportion with what my initial readings had led me to expect" (Derrida 1987a:110)

2.5.2 BARTHES, LACAN AND FOUCAULT

Mention of Roland Barthes is a good way into the rich and difficult resource of ideas that French structuralism provides for social thought. Barthes bridges the gap "between the academic study of literature and the actual practice of writing" (Sturrock 1979:55). The diversity of his thought is neatly summarized by the very chapter headings of Culler's (1990) monograph on Barthes - literary historian, mythologist, critic[11], polemicist, semiologist, structuralist, hedonist and - of course - writer. Sturrock (1979) says:

> Barthes would like us to understand <u>how</u> texts mean before we start worrying about <u>what</u> they mean. (Sturrock 1979:58)

> Society is a spectacle he can help to explain, by revealing to us some of the mechanisms by which it obscures its artificiality. (p.61)

Barthes sharp division of 'texts' from mere 'works' helps in this project. Texts are written by a higher order of writer, they actively involve the reader who effectively re-writes them as he reads, and responds to their fecundity with 'jouissance' - a disconcerting, extreme, orgasmic reaction that:

> rocks the reader's historical, cultural and psychological foundations, and the consistency of his tastes and values and memories; it brings about a crisis in his relations with language. (Barthes - quoted by Sturrock 1979:72)

This could well serve as a definition of invention itself, just as such a text's author might provide an insight into the inventor:

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[11] For a broader view of Barthes's (and, indeed, Lacan's) place within the field of literary criticism, see Eagleton's (1983) superb treatment.

> We are upset if we are asked to believe .... that an author had first decided how to say and only then discovered what 'it' was .... But Barthes could claim that his version of how signification works is frequently true to the facts. It has the enormous merit of not positing .... immaterial signifieds which somehow exist in the writer's mind even before signifiers are found for them. (Sturrock 1979:67)

What an author produces, what an inventor 'discovers', depends upon many other 'texts', many other discourses. The reader, the user, the critic or the researcher brings yet more texts, and re-writes what he reads in terms of them. If this led to an unbounded proliferation of meanings then such research would be an arid (if perhaps enjoyable) exercise, but Barthes's key point is that it does not - he sees "'a limited plurality'" of meanings (Sturrock 1979:75). Eagleton adds: "Barthes is careful to remark that the work cannot be got to mean anything at all" (Eagleton 1983:137).

Lacan, like Barthes, sees the context of other texts as vitally important; his patient's personal history matters in his psychiatry. Bowie (1979) provides an introduction into the complexity of Lacan's ideas (see Bowie (1991) for greater detail)[12] - he sees him, like Barthes, as concerned with the question how?, not what?:

> Lacan is a builder of loosely moored conceptual mobiles in response to which the question 'What does it mean?' is better asked of a given term in the form 'What does it do?' or 'What paths does it travel?'. (Bowie 1979:122)

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[12] For an outline critique of Lacan by Derrida, see his prickly note in 'Positions' (Derrida 1987a:107-113).

Lacan seeks to restore the original insights of Freud, which he thinks have been corrupted by his successors. To do so he uses Saussure:

> 'L' inconscient est structuré comme un langage' (The unconscious is structured like a language). This best known of Lacan's pronouncements makes plain the importance of his debt to linguistics (Bowie 1979:125)

One of Lacan's central ideas here is the 'signifying chain' which Bowie sees as conceptualizing signification in two contradictory ways, yet allowing a resolution of them - in the first case:

> signifiers extend to the horizon in all directions. When the signified seems finally to be in reach, it dissolves at the explorer's touch into yet more signifiers. The signifying chain of speech comprises the 'rings of a necklace that is a ring in another necklace made of rings' (Bowie 1991:64-65)

Lacan's second conception gives almost absolute authority to the signifier, now actively colonizing meaning:

> Responsibility for the production of meanings no longer falls to both interactive components of the sign but to one component, hugely re-energized (Bowie 1991:65)

The signifying chain thus represents both the systemic nature of language, chaining the speaker's freedom, yet also the fecundity of language, as the mobile chain loops back on itself, or joins itself to other chains. The unconscious is thus elucidated:

> if the signifier plays and the signified 'slips beneath', then the unconscious is speaking in its native tongue. (Bowie 1979:144)

Lacan integrates language and the unconscious into a shifting and loose triad of three orders; the first order is the 'symbolic':

> Lacan calls the domain of the signifier, in which this perpetual restructuring of the subject takes place, the symbolic order. (Bowie 1979:132)

This is opposed by Lacan to his second order - the 'imaginary' - where identity, the ego, is created:

> By way of the imaginary, the original identificatory procedures which brought the ego into being are repeated and reinforced by the individual in his relationship with the external world of people and things. (Bowie 1991:92)

But Lacan's conception does not end here - however the individual as a subject is constituted, there are things that already exist, things that just <u>are</u>. On the other hand, language has to operate upon something - it needs a primordial chaos upon which to operate, things to name. Lacan subsumes both of these divergent ideas within his third order - the 'real' (Bowie 1979:133, Bowie 1991:ch.4 is also very pertinent here):

> It is the irremediable and intractable 'outside' of language; the indefinitely receding goal toward which the signifying chain tends; the vanishing point of the Symbolic and Imaginary alike. (Bowie 1979:134)

Dipping into Lacan's theory in this way is hardly fair, but it does give a flavour of one way of conceptualizing the links between language, the psychical apparatus and the individual - links that this thesis itself will need to consider. Sperber offers another, and Derrida a third[13].

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[13] Although Derrida observes of Lacan's orders: "I have never been convinced of the necessity of this conceptual tripartition" (Derrida 1987a:84).

The work of Michel Foucault has many diverse elements; discourse and power are two that are commonly discussed, and it is his ideas on discourse that are particularly relevant here. Philp provides a very brief, but perceptive, introductory summary of Foucault's ideas[14]:

> The sciences of man have, he argues, subverted the classical order of political rule based on sovereignty and rights, and have instituted a new regime of power exercised through disciplinary mechanisms and the stipulation of norms for human behaviour. (Philp 1985:67)

Foucault continually employs historical material, and shows its implication in the exercise of power - this deliberately emphasizes the context of knowledge rather than the subject who creates or appropriates it (Wuthnow et.al. (1984:148-152) and Burrell (1988:222-224) discuss this).

For Foucault power, discourse and knowledge are thus inextricably bound together. Philp defines discourse here:

> a discourse can be seen as a system of possibility: it is what allows us to produce statements which will be either true or false - it makes possible a field of knowledge. (Philp 1985:69)

White (1979) says of Foucault's discourse:

> Discourse is the term under which he gathers all of the forms and categories of cultural life, including, apparently, his own efforts to submit this life to criticism. (White 1979:82)

Foucault's own discourse, White says, has no centre, is "all surface" and "willfully superficial". White continues:

> Foucault sets the free play of his discourse over against <u>all</u> authority. He aspires to a discourse that is free in

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[14] Burrell (1988) also provides a useful summary of Foucault, emphasizing his relevance to organizational analysis. He sees discourse and power as belonging to successive periods of Foucault's work, using the terms archaeological and genealogical to characterize them.

> a radical sense, a discourse that dissolves its own authority, a discourse that opens a 'silence' in which only 'things' exist in their irreducible Difference, resisting every impulse to find a sameness uniting them all in any order whatsoever. (White 1979:85-86)

All that is then left is style - style conjures the play of signifiers and signifieds from the 'silence', from the nothingness that precedes discourse, from the "'absence' at the heart of language" (p.87).

White emphasizes Foucault's relevance to the discourses that surround innovation in the data of this thesis:

> Wherever Foucault looks, he finds nothing but discourse; and wherever discourse arises, he finds a struggle between those groups which claim the 'right' to discourse and those which are denied the right to their own discourse. .... But the authority of his own discourse remains unspecified. (White 1979:91)

Foucault's answer is to return to linguistics, or rather to rhetoric, for he sees the orders of different ages - 'épistèmes' he calls them - as each characterized by one dominant rhetorical trope - by its style. His own discourse stands in ironic antithesis - catachresis - in the gaps between them; his 'archaeology' looks for the same tropes within the difference of a discourse. Difference occasionally asserts itself, and discourse must then seek a new trope in its 'will to truth' (p.95). For Foucault these 'lightning flashes':

> 'open a void, a moment of silence, a question without an answer, provoke a breach without reconciliation where the world is forced to question itself' (Foucault - quoted in White 1979:97)

In the final development of Foucault's ideas, "the 'void' out of which language was originally conceived to have spun its

fictions has been filled" - filled with power, with demonic force (p.113).

However - even here, White suggests in conclusion, Foucault has not achieved the liberation from restraint he seeks:

> although his thought is based primarily on a theory of language, he has not elaborated such a theory systematically. And as long as he fails to elaborate it, his thought remains captive to that very power which it has been his aim to dissipate. (White 1979:114)

This is very similar to Derrida's critique of Foucault, very shortly to be considered.

As a postscript, two reviews of the application of ideas like those of Foucault to analysis within the social sciences emphasize the relevance of the notion of discourse. Parker and Shotter (1990) highlight the power of professional discourse:

> the 'relations of ruling', as we might call them, are mediated by various discourses, texts, or idioms, certain accepted, proper, or professional ways of talking within which one can only properly have a place by being 'licensed', by gaining the appropriate credentials - otherwise, one runs the risk of having what one says not taken seriously, ignored as 'unprofessional'. (Parker & Shotter 1990:12)

Cooper & Burrell (1988) make a very similar point within an organizational context:

> In the organizational context, we are faced with a set of primary data constituted by programmes and technologies. These are essentially groups of discourses belonging to different systems of rationality (legal, economic, scientific, etc.). (Cooper & Burrell 1988:107)

Discourse will be the primary tool in the analysis of this thesis.

2.5.3 DERRIDA

> There is no sense in doing without the concepts of metaphysics in order to shake metaphysics. We have no language - no syntax and no lexicon - which is foreign to this history; we can pronounce not a single destructive proposition which has not already had to slip into the form, the logic, and the implicit postulations of precisely what it seeks to contest. (Derrida 1978:280-281)

Derrida is a philosopher who challenges the whole philosophical tradition from Socrates to Husserl, but in so doing he never departs from its rigourous canons of analysis:

> there is no <u>effective</u> and <u>efficient</u> position, no veritable force of rupture, without a minute, rigorous, extended analysis, an analysis that is as differentiated and as scientific as possible
> (Derrida 1987a:94 [original-1972])

For Derrida, this is not a matter of choice, it is not that critique from outside philosophy would be less effective, it is that there is <u>no</u> such position.

Derrida sees a privilege accorded to the spoken word in the Western philosophical tradition (logocentrism he therefore calls it), and a concomitant (but Derrida shows unsuccessful) attempt to marginalize writing. This pervasive tradition sees writing as a merely secondary and exterior aide-mémoire, whereas thought is 'inside' - hearing oneself speak. Derrida seeks to expose the metaphysical basis of this, where meaning is transcendental - a transcendental signified - and truth is Plato's 'writing-in-the-soul', truth unveiled, 'aletheia'

(Derrida (1987a:105), Norris (1987:54-55), Culler (1979:169-170)). To do this Derrida allows logocentrism's own inconsistencies to 'speak' for themselves - how can speech be true if truth is unveiled as writing-in-the-soul?

This sub-section considers Derrida. It uses, as guides, Norris's (1987) sensitive philosophical critique together with Culler's (1979) elegant introductory essay and Cooper's (1989) challenging paper on Derrida's relevance to organizational analysis. Above all, it uses Derrida's own words - his style is not easy, it uses intricate word-plays to show how meanings change in the spaces between words[15]. One of his translators, Alan Bass, provides an excellent introduction to reading Derrida:

> Like the analyst .... the reader must let his attention float, and be satisfied with a partial understanding of a given essay on any particular reading. As the manifest language begins to become more familiar, the persistence of the latent content .... will become a surer guide, a more salient thread in the weave of these texts. (Bass in Derrida 1978:xvi)

Bass's advice is followed here; a feel for Derrida will be provided first by briefly looking at his critiques of Lévi-Strauss and Foucault - case-studies of 'Derrida in action' as it were. An extended attempt will then be made to tease out the latent content using, as a very loose framework, one of Derrida's own densely packed passages.

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[15] Derrida's (1987b) 'The Post Card' provides many excellent and challenging examples.

DERRIDA ON LÉVI-STRAUSS:

The very title of Derrida's essay on Lévi-Strauss - 'Structure, Sign and Play in the Discourse of the Human Sciences' (Derrida 1978:278-293, Norris 1987:127-141) - provides commentary on it and emphasizes its place in the Saussurean structuralist tradition. The concept of structure is "as old as Western science and Western philosophy" (Derrida 1978:278), but before the era of Lévi-Strauss's questioning always had a fixed centre - a metaphysical concept of Being as presence:

> It could be shown that all the names related to fundamentals, to principles, or to the centre have always designated an invariable presence - 'eidos', 'arche', 'telos', 'energeia', 'ousia' (essence, existence, substance, subject) 'aletheia', transcendentality, consciousness, God, man, and so forth.
> (Derrida 1978:279-280)

Questioning this tradition, Derrida says, has involved de-centring the structure:

> the moment when language invaded the universal problematic, the moment when, in the absence of a center or origin, everything became discourse
> (Derrida 1978:280)

This de-centring allowed the ethnologist like Lévi-Strauss to step aside from his own culture - to see it as discourse, a discourse, moreover, that demands its own critique. This might follow two paths - daring to step outside and question the philosophical concepts, but:

> The step 'outside philosophy' is much more difficult to conceive than is generally imagined by those who think they made it long ago with cavalier ease
> (Derrida 1978:284)

The second path - closer to Lévi-Strauss's - is to separate method from truth - to pick, choose, and modify at will

whatever old conceptual tools come to hand, without attributing any truth to them. "'Bricolage' .... might be called the discourse of this method" (p.285).

Lévi-Strauss distinguishes bricolage, the disruptive tool, from the firmly centred scientific logic of the engineer's discourse. It is here that Derrida inserts his own linguistic crowbar:

> the odds are that the engineer is a myth produced by the 'bricoleur'. As soon as we cease to believe in such an engineer and in a discourse which breaks with the received historical discourse, and as soon as we admit that every finite discourse is bound by a certain 'bricolage' and that the engineer and the scientist are also species of 'bricoleurs', then the very idea of 'bricolage' is menaced and the difference in which it took on its meaning breaks down. (Derrida 1978:285)

This is an excellent example of Derrida's way of deconstructing a discourse - he never throws away meaning, but challenges the discourse by opposing one reading to another (both licensed by the discourse). To do this Derrida needs the licence of the philosophical discourse as his lever:

> What I want to emphasize is simply that the passage beyond philosophy does not consist in turning the page of philosophy (which usually amounts to philosophizing badly), but in continuing to read philosophers <u>in a certain way</u>. (Derrida 1978:288)

Derrida thus rethinks classical structure in terms of signification. Structure cannot then allow the centre that it's own presence, as a fixed concept within an analytical discourse, demands:

> One cannot determine the center and exhaust totalization because the sign which replaces the center .... is added, occurs as a surplus, as a <u>supplement</u>. (Derrida 1978:289)

This defines a stark choice - either to redeem classical structure by invoking history and an origin to explain its

changeable character, or to accept the logic of change through the play of signification. Lévi-Strauss contains elements of both, but Derrida's conclusion is more complex - a powerful statement of his difficult project:

> For my part .... I do not believe that today there is any question of <u>choosing</u> - .... we must first try to conceive of the common ground, and the 'différance' of this irreducible difference. (Derrida 1978:293)

DERRIDA ON FOUCAULT:

Derrida, "having formerly had the good fortune to study under Michel Foucault" (Derrida 1978:31), develops an interesting perspective upon his master in his essay - 'Cogito and the History of Madness' (Derrida 1978:31-63). The elegant and accessible clarity of this essay allows it to critique reason, and so explicate the considerable problems Foucault has in defining madness in contradistinction to reason. Foucault must avoid the trap:

> that would consist in writing a history of untamed madness .... from within the very language of classical reason itself (Derrida 1978:34)

Derrida applauds Foucault's determination to avoid this trap by by-passing reason, but adds that it is: "with all seriousness, the <u>maddest</u> aspect of his project" (p.34). Foucault determines to find the point at which madness and reason <u>first</u> separated. Derrida sees great problems here, none of which Foucault adequately answers, but Derrida holds these in suspension in order to perform a close reading of Foucault's conclusion that it was Descartes who first expelled

madness into the outer silence.

Derrida points out that Descartes did not expel madness - 'I think, therefore I am' even if I am mad - he says:

> It is therefore a question of drawing back toward a .... zero point at which determined meaning and nonmeaning come together in their common origin (Derrida 1978:56)

At that zero point the possibility of totalizing discourse, of logos, exists alongside the possibility of demonic hyperbole that would exceed it, but the cogito comes first, the origin of temporality, of thought itself. Thus, instead of a Cartesian expulsion of madness:

> Foucault's reading seems to me powerful and illuminating .... from the moment which immediately succeeds the instantaneous experience of the Cogito at its most intense, when reason and madness have not yet been separated, when to take the part of the Cogito is neither to take the part of reason or reasonable order, nor the part of disorder and madness, but is rather to grasp once more, the source which permits reason and madness to be determined and stated. (Derrida 1978:58)

Derrida locates history and philosophy here too, he is worth quoting at considerable length - he provides a dazzling summary of his whole project:

> The historicity proper to philosophy is located and constituted in the transition, the dialogue, between hyperbole and the finite structure, between that which exceeds the totality and the closed totality, in the difference between history and historicity; that is, in the place where, or rather at the moment when, the Cogito and all that it symbolizes here (madness, derangement, hyperbole, etc.) pronounce and reassure themselves then to fall, necessarily forgetting themselves until their reactivation, their reawakening in another statement of the excess which also later will become another decline and another crisis. From its very first breath, speech, confined to this temporal rhythm of crisis and reawakening, is able to open the space for discourse only by emprisoning madness. This rhythm, moreover, is not an alternation that additionally would be temporal. It is rather the movement of temporalization itself as concerns that which unites it to the movement of the logos. But this violent liberation of speech is possible and can be pursued only in the extent to which it keeps itself

> resolutely and consciously at the greatest possible proximity to the abuse that is the usage of speech - just close enough to <u>say</u> violence, to dialogue with itself as irreducible violence, and just far enough to <u>live</u> and live as speech. Due to this, crisis or oblivion perhaps is not an accident, but rather the destiny of speaking philosophy - the philosophy which lives only by emprisoning madness, but which would die as thought, and by a still worse violence, if a new speech did not at every instant liberate previous madness while enclosing within itself, in its present existence, the madman of the day. (Derrida 1978:60-61)

Foucault's project of speaking outside of reason is thus truly mad, but it does allow Derrida to gaze into the chasm whence madness and reason come. Derrida presents his own 'cogito', or perhaps 'philosopho' to describe this project:

> I philosophize only in <u>terror</u>, but in the <u>confessed</u> terror of going mad. The confession is simultaneously, at its <u>present</u> moment, oblivion and unveiling, protection and exposure: economy. (Derrida 1978:62)

DERRIDA ON DERRIDA:

The fecundity, the rigour, and the relevance of Derrida's thought have all become very evident. However, as the 'tour-de-force' just quoted illustrates, his thought requires close attention and work. This quotation will be used as a framework upon which to hang Derrida's thought. This, of course, will be a dangerously structural and logocentric route to follow, unless the quotation just marked is taken apart, and re-marked together with supplementary remarks from Derrida's other texts. The texture of these will provide something of a context for their own later use in the textile that this thesis seeks to weave. The first step in this is to split the quotation into manageable portions - these will be

used, enclosed in square brackets [....], to preface each element of the discussion.

Arche-Writing

> [The historicity proper to philosophy is located and constituted in the transition, the dialogue, between hyperbole and the finite structure....]

Derrida criticizes the search for origins as metaphysical, but his own thought has a sort of starting point in 'the dialogue between hyperbole and finite structure'. Their dialogue takes place within a 'fundamental structure' that binds everything that can be thought:

> If there are structures, they are possible only on the basis of the fundamental structure which permits totality to open and overflow itself such that it takes on meaning by anticipating a 'telos' .... This opening is certainly that which liberates time and genesis (even coincides with them), but it is also that which risks enclosing progression toward the future - becoming - by giving it form. (Derrida 1978:26)

This fundamental structure, as Norris says, "is what Derrida terms 'arche-writing'" (Norris 1987:122) - it orders everything, incorporates any finite structure or totality, but also any excess, any aporia, any madness, any hyperbole - it sets time, philosophy and history in motion. The rest is not even silence or absence, or the music of the spheres - it is nothing.

Structure and Process

> [....between that which exceeds the totality and the closed totality, in the difference between history and historicity....]

Saussure is of clear relevance here because his idea of structure, unlike any logocentric totality, is firmly rooted in arbitrary difference. Derrida catches this promise in 'Positions' - he says, of "the concept of structure":

> Everything depends upon how one sets it to work. Like the concept of the sign - and therefore of semiology - it can simultaneously confirm and shake logocentric and ethnocentric assuredness. It is not a question of junking such concepts, nor do we have the means to do so. (Derrida 1987a:24)

Cooper neatly sets out Derrida's intentions here:

> His work addresses a central problem of social analysis: the logics of structure and process and their interaction. He starts from the position that our traditional ways of thinking are structure-biased and are therefore incapable of revealing the nomadic and often paradoxical character of process. (Cooper 1989:480)

Derrida, in a passage worthy of Lévi-Strauss at his most maudlin, beautifully describes the appeal, but also the problem, of privileging structure over process:

> the relief and design of structures appears more clearly when content, which is the living energy of meaning, is neutralized. Somewhat like the architecture of an uninhabited or deserted city, reduced to its skeleton by some catastrophe of nature or art. A city no longer inhabited, not simply left behind, but haunted by meaning and culture. (Derrida 1978:5)

Norris (1987:224) also quotes from this passage in making the powerful point that Derrida's search for a discourse that exceeds this structure, that peoples the deserted city, is no epistemological enterprise - it is a search for an ethical discourse. Derrida rejects the traditional metaphysical way out of this dilemma - seeking truth as aletheia[16] - his course is deconstruction.

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[16] Derrida (1987a:104-105) develops these ideas of truth.

Spacing

> [....that is, in the place where, or rather at the moment when, the Cogito and all that it symbolizes here (madness, derangement, hyperbole, etc.) pronounce and reassure themselves then to fall....]

Derrida's first step is to seek the space where meaning is forged - the 'place' and 'moment' where and when hyperbole and the Cogito pronounce themselves: "I try to write (in) the space in which is posed the question of speech and meaning" (Derrida 1987a:14). Such a space, interval, place, moment, is generative - it is the possibility of structure, process, time, space. Derrida conceives this under the name 'spacing' - "spacing is neither space nor time" (Derrida 1987a:43):

> Spacing designates nothing, nothing that is, no presence at a distance; it is the index of an irreducible exterior, and at the same time a movement, a displacement that indicates an irreducible alterity. (Derrida 1987a:81)

Here it is that Derrida can probe the ultimate extent of a discourse - the 'irreducible exterior'. This is the space where deconstruction might find its workshop - the workshop of language itself, of difference and of signification.

Différance

> [....necessarily forgetting themselves until their reactivation, their reawakening in another statement of the excess which also later will become another decline and another crisis....]

Derrida (1987a:94) underlines that spacing is both irreducible difference and productive movement; it is where Derrida's concept, that is not a concept - 'différance' - operates. Culler provides the etymology of différance:

> The French verb 'différer' means both to differ and to defer. 'Différance', which did not previously exist in French, sounds exactly the same as 'différence (meaning difference), but the ending in 'a' .... makes it a new form meaning 'a differing or a deferring'. (Culler 1979:165)

Différance is an undefinable word that is a token for all the problems of the Western philosophical tradition and of 'ordinary language' - it stands as a 'statement of the excess' that re-awakens language from decline into a new crisis. Derrida spends large portions of 'Positions' discussing différance:

> 'Différance' is the systematic play of differences, of the traces of differences, of the 'spacing' by means of which elements are related to each other. (Derrida 1987a:27, also in Culler 1979:165 in a slightly different translation)

Norris shows something of its analytical promise:

> this is precisely what Derrida intends: that 'différance' should function not as a concept, not as a word whose meaning could be finally 'booked into the present', but as one set of marks in a signifying chain which exceeds and disturbs the classical economy of language and representation. (Norris 1987:15)

Speech, and the Time and Space of Writing

> [....From its very first breath, speech, confined to this temporal rhythm of crisis and reawakening, is able to open the space for discourse only by emprisoning madness....]

The full import of Derrida's views of writing are well seen in one of his critiques of Freud (Derrida 1978:196-231):

> It is no accident that Freud, at the decisive moments in his itinerary .... invokes signs which do not transcribe living, full speech, master of itself and self-present. (Derrida 1978:199)

Freud's model of the psychical apparatus allows Derrida to investigate the 'rhythm of crisis and reawakening' itself - a

concern very close to the need in the present research to link structure and process with human perception and agency.

Freud proposes a 'writing machine' as a textual metaphor for the psyche, but Derrida goes further - "We shall not have to ask", he says:

> if the psyche is indeed a kind of text, but: what is a text, and what must the psyche be if it can be represented by a text? For if there is neither machine nor text without psychical origin, there is no domain of the psychic without text. (Derrida 1978:199)

'Arche-writing' is clearly implicated in such a question. Derrida considers Freud's final model - the 'Mystic Writing-Pad'; the pad consists of three layers - a protective sheet of celluloid (the pre-conscious), a sheet of translucent paper (the conscious), and a wax slab (the unconscious). The external stimulus of a stylus presses the top two layers into the wax, to which the translucent paper adheres, thereby retaining the writing until the paper is lifted off again - but, even after this erasure from consciousness, it remains in memory as a trace broached in the wax. This meets Freud's:

> necessity of accounting simultaneously .... for the permanence of the trace and for the virginity of the receiving substance (Derrida 1978:200)

But all this is static - the rabbit Derrida helps Freud to pull from his hat is to add the rhythm of temporality:

> Until now, it has been a question only of the space of writing .... But there is as well a <u>time of writing</u>, and this time of writing is nothing other than the very structure of that which we are now describing. We must come to terms with the temporality of the wax slab. (Derrida 1978:225)

> Temporality as spacing will not only be the horizontal discontinuity of a chain of signs, but also will be writing as the interruption and restoration of contact between the various depths of psychical levels: the

> remarkably heterogeneous temporal fabric of psychical work itself. (p.225)

This rhythm is not an interesting addition to a neat psychical model - it is central to Derrida's analysis of the way speech can imprison madness. Deconstruction, by levering open the space of oscillation from within the writing that it constitutes, can shake the logocentric tradition and allow the arche-writing to be glimpsed.

Deconstruction

> [....This rhythm, moreover, is not an alternation that additionally would be temporal. It is rather the movement of temporalization itself as concerns that which unites it to the movement of the logos....]

The rhythm of thought - the rhythm of becoming - cannot be arrested for long by any logocentric concept of Being. Derrida exposes any attempts to privilege Being, presence, or meaning:

> the 'a' in 'différance' also recalls that spacing is temporization, the detour and postponement by means of which intuition, perception, consummation - in a word, the relationship to the present, the reference to a present reality, to a 'being' - are always <u>deferred</u>. (Derrida 1987a:28-29)

Derrida challenges the classical view of thought as revealed by (being present in) hearing oneself speak (Norris 1987:66). Derrida thus faces the problem of challenging the spoken word at the same time as using it[17] - his solution is deconstruction.

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[17] Of course, Derrida also exposes the problems of Plato's exactly inverse project of seeking to justify the inferiority of writing in writing - see Norris (1987) chapter 3.

There is no escape from the discourse of classical philosophy, so Derrida's aim is less to deconstruct a text than to let the text deconstruct itself, to allow the other possibilities of arche-writing to show through. Norris (1987:ch.2) provides a fine introduction to deconstruction:

> Any attempt to define 'deconstruction' must soon run up against the many varied obstacles that Derrida has shrewdly placed in its path. To begin with, at least, one can perhaps best proceed by way of a series of negative descriptions. Deconstruction is <u>not</u>, he insists, either a 'method', a 'technique' or a species of 'critique'. Nor does it have anything to do with textual 'interpretation' (Norris 1987:18)

Deconstruction is not a method or technique for the simple reason that it tries to shake the very tradition that would reduce everything to concepts like 'method' - fixed in identity and time. What Norris (1987:20) calls the "rigorous <u>work</u> of deconstruction" is a double process - in it Derrida marks both sides of the space and interval where philosophical discourse and its demonic excess coexist.

<u>Overturning</u>

> [....But this violent liberation of speech is possible and can be pursued only in the extent to which it keeps itself resolutely and consciously at the greatest possible proximity to the abuse that is the usage of speech....]

The first part of this 'double marking' is to overturn a discourse by spotting those elements whose equivocality signal most clearly that some abuse is at hand. Deconstruction reads a text written by two hands, which has:

> by means of this double play, marked in certain decisive places by an erasure which allows what it obliterates to be read, violently inscribing within the text that which attempted to govern it from without (Derrida 1987a:6)

To deconstruct this text:

> we must traverse a phase of overturning. .... we are not dealing with the peaceful coexistence of a 'vis-à-vis', but rather with a violent hierarchy. One of the two terms governs the other .... or has the upper hand. To deconstruct the opposition, first of all, is to overturn the hierarchy at a given moment. (Derrida 1987a:41)
>
> the signifier is a positive lever .... In this phase of overturning, one opposes, insistently, the pole of the signifier to the dominant authority of the signified. (Derrida 1987a:82)

Thus, Culler says:

> Derrida's readings focus on terms which commentators have thought unimportant but which in their double functioning reveal a problematic logic that exceeds and undermines the explicit system of a text. (Culler 1979:178)

Derrida's 'classic' example of such a lever is Plato's use of the word 'pharmakon' (Norris 1987:ch.3). 'Pharmakon' can mean both poison or cure; not only this - Plato implies, but does not use (or uses in erasure), the word 'pharmakos', scapegoat. The word is undecidable, and so shakes Plato's text. Norris well encapsulates this idea when he says that deconstruction seeks out:

> moments of self contradiction where a text involuntarily betrays the tension between rhetoric and logic, between what it manifestly means to say and what it is nonetheless constrained to mean (Norris 1987:19)

The Double Mark

> [....just close enough to say violence, to dialogue with itself as irreducible violence, and just far enough to live and live as speech....]

Any finite discourse can thus be made to do violence to itself - but to go no further would be to destroy (or, worse still, to leave untouched) the very thing that is being

analysed - it must be allowed still to 'live'. Derrida's challenge to metaphysical concepts demands a double mark:

> We must first overturn the traditional concept of history, but at the same time mark the interval, take care that by virtue of the overturning .... the interval be not reappropriated (Derrida 1987a:59)
>
> By means of the play of this interval between the two marks, one can operate both an overturning deconstruction and a positively displacing, transgressive, deconstruction. (pp.65-66)

This interval, or space, allows Derrida not to step outside of classical philosophy, but rather to show that its claims to truth, privilege, and especially to speech unveiling truth, are questionable:

> Derrida writes of the 'double mark', the supplementary inscription which escapes 'the pertinence or authority of truth', not simply by rejecting such ideas wholesale but by showing that they rest on a certain metaphysics, an ontology of language and being whose absolute reign must henceforth at least be open to doubt. .... It is not - he insists - a question of breaking altogether with that language, of coming out once and for all on the far side .... Such claims could only be self deluding, since there is simply no alternative ground on which to stand, no language that has not been endlessly worked over in its deepest conceptual resources by the logical grammar of Platonism. (Norris 1987:53)

The result of critical intervention is thus not a new philosophy, but a new, and thoroughly sceptical, way of using the old philosophy - a way that gives far-reaching insights into how such logocentric views become what they seem to be to those who share such discourse.

Supplementarity

> [....Due to this, crisis or oblivion perhaps is not an accident, but rather the destiny of speaking philosophy - the philosophy which lives only by emprisoning madness, but which would die as thought, and by a still worse violence....]

Derrida's project offers access not to an outside, but to a space where classical philosophical ideas and the excess of 'madness' meet. Spectres peep out of this space, but whilst philosophy can try to imprison them, it cannot escape them - they are still there in thought, in Derrida's 'arche-writing'. In Derrida's playful debate they are 'supplementary' - both added on, but also supplying a lack. This 'logic of supplementarity' is discussed by Norris - he quotes Derrida, who observes:

> that what adds itself to something takes the place of a default in the thing, that the default, as the outside of the inside, should be already within the inside (Derrida, quoted in Norris 1987:111)

Culler adds:

> The logic of supplementarity, as Derrida describes it, is powerful and pervasive; it makes possible everything which we think of as human: language, passion, society, art. (Culler 1979:168)

Thus, although Derrida may seem to do violence to the text of philosophy, he must work within philosophy - his own philosophy cannot escape the imperious Western metaphysical tradition:

> There is no possible leap 'beyond' philosophy except on terms that philosophy will always turn out to have conceived or somehow determined in advance (Norris 1987:171)

To interpret Derrida as attempting such a 'leap' is willfully to misread him:

> I have .... regularly tried to put philosophy back on stage, on a stage that it does not govern, and that the classical historians of philosophy, in the university and elsewhere, have sometimes judged a little difficult. (Derrida 1987a:50)

It now remains to essay a description of Derrida's 'stage' - this is, indeed, 'a little difficult'.

The Trace and Dissemination

> [....if a new speech did not at every instant liberate previous madness while enclosing within itself, in its present existence, the madman of the day.]

Norris firmly juxtaposes Derrida and his fellow-philosophers, some of whom disown him (Norris 1987:ch.7); he notes the tradition of 'ordinary-language' philosophy that writes Derrida off as literary, and not philosophical. Yet Derrida's ideas have proved relevant to the people interviewed in this research precisely because they give insights into their ordinary-language and 'common sense' assertions. Derrida provides an understanding of the apparent ease with which these people could, 'at every instant', think new, and quite contradictory, things.

Derrida shows how yesterday's 'madness' is liberated in the rush to suppress that of today, and how this can go unperceived by those who share the discourse. He uses two powerful notions here - the trace and dissemination.

Derrida provides a clear introduction to the idea of the trace in 'Positions':

> The play of differences supposes, in effect, syntheses and referrals which forbid at any moment, or in any sense, that a simple element be <u>present</u> in and of itself, referring only to itself. Whether in the order of the spoken or written discourse, no element can function as a

> sign without referring to another element which itself is not simply present. This interweaving results in each 'element' - phoneme or grapheme - being constituted on the basis of the trace within it of the other elements of the chain or system . This interweaving, this textile, is the text produced only in the transformation of another text. Nothing, neither among the elements nor within the system, is anywhere ever simply present or absent. There are only, everywhere, differences and traces of traces. (Derrida (1987a:26)

Meanings are endlessly substituted, and Derrida says:

> Dissemination affirms (I do not say produces or controls) endless substitution, it neither arrests nor controls play (Derrida 1987a:86)
>
> the force and the form of its disruption explode the semantic horizon" (p.45)

Dissemination, then, catches something of the arche-writing, the: "generating (disseminating) void in which the text is launched" (Derrida 1987a:86) - a 'general text', where:

> What I call text is also that which 'practically' inscribes and overflows the limits of such a discourse. There is such a general text everywhere that (that is, everywhere) this discourse and its order (essence, truth, meaning, consciousness, ideality, etc.) are overflowed, that is, everywhere that their authority is put back into the position of a mark in a chain that this authority intrinsically and illusorily believes it wishes to, and does in fact, govern. (Derrida 1987a:59-60)

This encapsulates Derrida's relevance to the present thesis - the quick and chameleonic changes between the various discourses in people's interviews are seen by Derrida as marks in a chain. Yet this chain has no serial temporal character - it is a trace of semantic change outside of time, a multi-coloured textile of possible meanings between which thought can leap. The fecundity of these ideas lies in their clear linkage to thought itself through the sophisticated model (if

model it be) of perception that has already been discussed above in relation to Derrida's ideas on Freud.

The problem is one of perception - how can all the discourses that surround people co-exist and yet separately be comprehended by them? Derrida's essay on Freud begins to answer this:

> We have already defined elsewhere the fundamental property of writing, in a difficult sense of the word, as spacing: diastem and time becoming space; an unfolding as well, on an original site, of meanings which irreversible, linear consecution, moving from present point to present point, could only tend to repress .... Now in every silent or not wholly phonic spacing out of meaning, concatenations are possible which no longer obey the linearity of logical time, the time of consciousness or preconsciousness, the time of 'verbal representations' (Derrida 1978:217)

Freud, in considering dreams, used ideas of writing, not speech; a consideration of how different meanings and discourses can coexist, must do the same. The fundamental point that emerges is that perception is not 'pure', not continuous, but continual - the mystic writing pad is continually refreshed with new writing - memory builds up a trace of each successive writing, each successive breaching of the wax remains after erasure from consciousness. Derrida describes all this in a memorable passage:

> If there were only perception, pure permeability to breaching, there would be no breaches. We would be written, but nothing would be recorded; no writing would be produced, retained, repeated as legibility. But pure perception does not exist: we are written only as we write, by the agency within us which always already keeps watch over perception, be it internal or external. .... The subject of writing is a system of relations between strata: the Mystic Pad, the psyche, society, the world. Within that scene, on that stage, the punctual simplicity of the classical subject is not to be found. (Derrida 1978:227)

This provides a complex, difficult, even terrifying, vision of perception, of people as the 'subjects' of their discourse (Norris 1987:207-208 develops this Freudian insight). But it fits the observations of the present research - of individual agency, and perception, constrained by different discourses. It also shows the impossibility of tying investigation to any one level-of-analysis - the psyche, society, the world are all inextricably implicated, not merely in shared discourse, but in perception itself. Chapter eight will repeat, and further develop, this discussion.

DERRIDA ON RATIONALITY - EPILOGOS?:

Derrida is a philosopher, and his philosophy has a rigour and integrity that sets him a little apart from the other thinkers considered in this section. In undertaking the task of using Derrida's ideas to investigate organizations, the followers of Mr Valiant-for-Truth need to arm themselves with rigour, they must clearly understand what they mean by rationality and truth, for as Derrida says:

> we must have [il faut] truth. Such is the law. .... How can we do without it? (Derrida 1987a:105)

Cooper & Burrell (1988), in their introduction to organizational use of these ideas, see enlightenment thought as having been misappropriated by the early sociologists such as Saint-Simon and Comte:

> We thus find the rudiments of organizational thinking in the Enlightenment philosophy. But at this historical point there occurs a schism within Reason itself, showing that it too is subject to the displacements intrinsic to self-reference: Reason is appropriated by an early form

> of systems thinking which subverts its critical edge to the functional demands of large systems. (Cooper & Burrell 1988:94-95)

This they call 'systemic modernism', as opposed to the 'critical modernism' of the Enlightenment. Norris sees Derrida as critiquing a "Western instrumental reason" that is surely allied to Cooper & Burrell's 'systemic modernism'.

Derrida questions what constitutes this 'rational' thought, but this in no way implies that Derrida is himself irrational:

> Derrida cannot be read by selecting statements to suit one's polemical purpose .... deconstruction is <u>not</u> the antithesis of philosophic reason (Norris 1987:140)

Indeed Norris writes an entire chapter (ch.6) on Derrida, Kant, and the Enlightenment tradition, showing Derrida's refusal to reject the philosophical tradition that he critiques:

> Simply to <u>reject</u> that tradition - thinking to occupy a whole new domain of 'post-modern' cultural debate - is effectively to give up any hope of informed rational critique (Norris 1987:157)

Norris sums up:

> My reading of Derrida is of course sharply at odds with that prevalent idea of deconstruction as a species of last-ditch irrationalism which denies both the principle of reason and the existence of any reality 'outside the text. (Norris 1987:169)

Derrida, then, allows the excesses that overflow the seeming rationality of 'systemic modernism' to be seen. He provides a matchless basis for critique by replacing an epistemological insistence upon classification, measurement, and static taxonomies with an ethical concern for its

repressions:

> This is where deconstruction parts company with the wider post-structuralist enterprise. For Derrida, the realm of ethical discourse is that which exceeds all given conceptual structures, but exceeds them through the patient interrogation of their limits, and not by some leap into an unknown 'beyond' which would give no purchase to critical thought. (Norris 1987:224)

Derrida's 'ethical discourse' provides a suitable scene for the analysis of this thesis, the reading of Derrida in this thesis follows that of Norris.

Various writers have advocated the use of writers like Derrida in similar contexts (for example, Cooper & Burrell (1988), Burrell (1988), Cooper (1989), Vargish (1991), Parker & Shotter (1990)). Cooper characterizes this the "'symbolic turn' in organizational studies" (Cooper 1989:479)[18] - allied to this are other writings that present some of the most perceptive contemporary readings of organization, through the use of ideas of rhetoric (for example, Gowler & Legge (1983), Gowler & Legge (1986), and Nahapiet's (1988) use of these ideas). Culler catches the promise of such approaches when he says:

> Derrida's readings show that so-called philosophical texts are most acute and precise when their figures and their rhetorical strategies are given close attention. (Culler 1979:178)

This section has suggested how Derrida's ideas might be used to resolve problems in the established traditions of management scholarship. The last section hinted at a similar

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[18] What Norris (1987:218) calls "the 'linguistic turn' across a wide range of disciplines".

use of Giddens work - and his comments upon the need for such sensitive theoretical innovation serve as an appropriate conclusion to this chapter:

> There is an undeniable comfort in working within established traditions of thought - the more so, perhaps, given the very diversity of approaches that currently confronts anyone who is outside any single tradition. The comfort of established views can, however, easily be a cover for intellectual sloth. If ideas are important and illuminating, what matters much more than their origin is to be able to sharpen them so as to demonstrate their usefulness, even if within a framework which might be quite different from that which helped to engender them. (Giddens 1984:xxii)

> to him the meaning of an episode was not inside like a kernel but outside, enveloping the tale which brought it out as a glow brings out a haze, in the likeness of one of these misty halos that sometimes are made visible by the spectral illumination of moonshine.
> (Joseph Conrad 1985 [1899]:30)

3.1 Introduction

> My own training as an ethnographer of a sociological sort reflects, I think, the training of many ethnographers and would-be ethnographers .... For better or worse, we lack a formal apprenticeship in the trade .... our appreciation and understanding of ethnography comes like a mist that creeps slowly over us in the library and lingers with us while in the field.
> (Van Maanen 1988:xii)

This chapter describes the method by which the data of this thesis has been obtained, analysed and presented. It leads into the rest of the thesis where the theory of the last chapter is brought together with the data of the next two. Each chapter would thus seem to take its logical appointed place within this thesis. Such a view, however, has problems - data often has a character no more substantial than Conrad's 'misty halo' in the quotation above - likewise, method, as Van Maanen says, 'creeps' over the researcher, it is also like a 'mist'.

Raising this problem of blurred boundaries is not a deliberate attempt to obfuscate what is self-evident and straight-forward. It is simply an acknowledgement that imposition of any adequate structure on the research topic, in advance of understanding that topic, is impossible. If it

were possible, then this thesis would probably not be necessary.

Dow's (1988) sophisticated discussion of organizational structure reinforces this point; he sharply distinguishes between 'configurational' and 'coactivational' views of structure. Broadly, he distinguishes the archetypal organization-chart view from one consisting in such as behavioural interactions. In the present data, as will be seen, structures do 'emerge' but the data very clearly do not warrant imputing these to conscious human design - discussion of organization charts and the like would not advance its understanding at all. 'Configurational' approaches look for structure, and find it; the present thesis looks for understanding; it finds structures of a sort, but it also finds them shot through with pathologies and problems. Understanding these problems demands the more open approach that will now be described.

The eclectic theoretical discussion of the last chapter has shown something of where this search for understanding has led, but it was, of course, written after the research 'event' - it begs the question of which comes first: theory, data or method? The question cannot be answered, research is reflexive - theory, data and method intertwine in such a way that causal reasoning is not merely impossible, it is irrelevant. If method and analysis are the prime concern of this chapter's text, theory and conjecture are still inscribed beneath its surface. It would be false to pretend that theory

led to method - method was learnt as the data was collected. Section 3.3 will take up these problems of linking theory and data in more detail, and 3.9 will introduce a few more theoretical questions relating to the data's presentation. All the other sections below will look at the practical business of gaining data[1].

In what follows, then, theory, data and method are regarded as categories of some little difficulty. Indeed, it is perhaps even a little contrary to refer to the emergent and unstructured texts of research interviews as 'data'. 'Data' is a hard objective word, and the interviews are interpretively and interactively accomplished discourses, of their very nature subjective. The use of the term 'data' is quite deliberate however - it emphasizes that management research, be it positivistic and highly focussed, or interpretive and reliant upon emergent ideas, is objective in that it depends upon observations of people's 'real-life'. Moreover, interpretive data still has much that is concrete and 'factual' about it, as the appendices to this thesis show. Interpretive data, then, is 'of' the people interviewed, the fact that it is 'of' the researcher as well does not diminish its status as 'data' - it adds a dimension that enhances it. The appendix to chapter one acknowledged this by considering the researcher's rôle in a little more detail. Van Maanen's

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[1] These sections largely record the personal experience of the researcher in writing this thesis - for this reason the masculine personal pronoun is used throughout this chapter.

view of this rôle emphasizes the everyday 'real-life' of research:

> Fieldworkers, it seems, learn to be among strangers while holding themselves in readiness for episodes of embarrassment, affection, misfortune, partial or vague revelation, deceit, confusion, isolation, warmth, adventure, fear, concealment, pleasure, surprise, insult, and always possible deportation. Accident and happenstance shapes fieldworkers' studies as much as planning or foresight; numbing routine as much as living theatre; impulse as much as rational choice; mistaken judgements as much as accurate ones. This may not be the way fieldwork is reported, but it is the way it is done. (Van Maanen 1988:2)

3.2 Different Sources of Data

The data were not collected in the order in which the next two chapters present them, nor are their natures as homogeneous as the presentation might imply. This section, therefore, discusses how choices were made regarding what data to collect.

The aim of this research has always been to study what might help or hinder the process whereby ideas become innovations. The researcher's personal experience in industry directed the initial search for data towards a case-study of an innovative technological project - the HOTOL project was chosen. This raised various substantive issues, two of which led to the next choices in the research process. First was the rather obvious observation that HOTOL is still in its early stages as an engineering project, it needed augmentation with another case-study. The second observation, almost a

truism, was that without money such a project will fail - this pointed to a review of how technology is financed. It has to be acknowledged, however, that both of these choices reflected the researcher's academic curiosity and predilections just as much as their relevance to the research topic.

First, then, a 'finished innovation' was needed, to extend HOTOL's unfinished story. This pointed to an historical case-study, which would provide the needed closure and might allow freer access to sensitive areas and personal feelings (since the actors were now dead, and material in the public domain). Also changes over time might be illuminated. However, the difficulties were also clear - it is not possible to interview dead people, and surviving material might be a poor substitute for first-hand contact, as well as being dangerously selective.

The HOTOL study gave encouragement through its limited, but successful, uses of non-interview data. A few of the HOTOL interviews had been compared with interviews of the same people conducted by a House of Lords Select Committee (who had published the full transcripts in their report (Lords 1988)); they seemed very similar. In addition, a number of articles by the people interviewed, as well as other secondary material, had been found useful.

This suggested that, if similar material could be found for an historical case-study, then data capable of comparison with HOTOL could be achieved. The development, by Charles Parsons, of the marine steam-turbine at the turn of the

century was chosen - a very significant and successful innovation, for which primary and secondary material was easily available (such as published private letters, official reports and technical articles), not to mention various biographies. The appendix to this chapter considers the choice of this historical case-study in much more detail.

The second thread of research to be pursued was finance. Here the approach was similar to that used with HOTOL - interviews were conducted with a wide range of people in the City of London and elsewhere. Two further issues emerged, and were followed up - the first, whether there were differences between UK and US experience, prompted conduct of a few interviews in New York. The second issue derived from the very generality of the approach taken to finance - financial aspects of innovation arose that the two case-studies did not cover, so a few more interviews were conducted with 'innovators', to provide a small set of 'one-person' case-studies. These were mainly conducted after the finance interviews (at which stage a very few more interviews to update the HOTOL story were also obtained).

The interviews that were conducted in all of these chosen contemporary settings raise issues of access, conduct of the interview, and the researcher's need for general cultural competence in these different areas. These issues are discussed in subsequent sections of this chapter.

3.3 Methodological Questions

> What really happens is that the anthropologist deploys various techniques of observation, and returns home with several packages of data. Each package, as a consequence of the observational technique from which it derives, has its own characteristics, and is in the first instance best summarized, or organized, in a specific and appropriate form. (Kuper 1983:202)

The question of why interviews were chosen as a vehicle to collect data prompts return to the more general concerns raised in the first section of this chapter. It is one thing to describe theories and data collection methods, but quite another to tie the two together in a discussion of methodology. As soon as method tries to use theory, their antithetical characters appear, they clash, and discussion steps onto shifting sand.

The usual interpretive response to this clash is to deny it, by way of the assertion that theory and data are so intertwined as to be inseparable. This response is to be found across the range of scholarship advocating qualitative methods - a classic of the genre is Glaser & Strauss (1967) who advise that "an effective strategy is, at first, literally to ignore the literature", and to wait for theory to emerge. The present research will demonstrate that this is good advice, but it, too, ducks the issue. Effectively, such a method is advocating a concentration on the practical business of getting data, in the knowledge that the intertwined theory will emerge with it. The problem is that, in practice, new method emerges as well - theory, method, and data, all

emerge, and a qualitative method must respect the autonomy of each emergent category. The method is thus forced to claim some privileged status, or to categorize itself. There is a strong hint of Giddens and Derrida here, and an indication why Giddens claims that structuration is only a sensitizing device, and Derrida asserts deconstruction cannot be a method.

All this section can do is face the problem, and plead that judgement be deferred. The case for such deferment is this - the problem of how data, theory and method intertwine is insoluble as long as data, theory and method remain undefined. Yet they are undefinable at present because, whilst they are clearly different, and sensed as such, the debate can only attempt to define them in terms of each other. They have not yet emerged from practical research, but only in discussion - as inherited philosophical categories. Further debate must therefore be at some new 'meta-level', and it must also be philosophical in character. Such debate must equally draw in the 'theory' of the last chapter, the 'method' of this one, and the 'data' of the next two. In fact, <u>methodological debate is one of the major themes of this entire thesis</u> - this is why any resolution or summary of the debate has to be deferred until later chapters. For the moment, faced with the paradoxical nature of 'method', 'data' and 'theory', the three terms will have to remain in invisible (from now on) inverted commas.

This approach is a practical one, it is consonant with Glaser & Strauss's imprecation to 'ignore the literature' and with Kuper's sense of 'what really happens'. The researcher

does not enter the field with clear pre-conceived methodological, epistemological or theoretical views - such views are much more likely to be 'post-conceived'. For this reason no detailed review of the prescriptive methodological literature is attempted here. Nonetheless, it would be questionable to suggest that the researcher enters the field with no pre-conceptions whatever - choices have to be made, and some sort of theoretical disposition is inevitable. The second half of this section, therefore, tries, post hoc, to present a flavour of the ideas of method and theory which the researcher used to make his choices before entering the field.

There is a clear spectrum of choices of method available, which can be rendered as a rough list of the form: questionnaires, administered questionnaires, formal interviews, unstructured interviews, participant observation, and action research. It is tempting to link this to a spectrum of theoretical standpoints, and beguiling similarities do exist between the two spectra. However, as has just been argued, thither lies the quicksand - no such consonance is claimed in this section.

In this research interviews were used, and these took a largely unstructured form. This choice became obvious because, even at the outset, it was evident that HOTOL spanned several organizations, and that those instrumental in it were spread wide - prolonged observation in any one place did not seem appropriate. The interviews were unstructured because this allowed a person to be asked to 'tell the story' without

knowing in advance what that story was. In addition, since the people involved were not known with any certainty before the first few interviews were conducted, and the problems they faced were certainly not known, the use of a questionnaire was clearly out of the question.

Unstructured interviews provided data in a textual form that bore later comparison with other, published, texts - particularly the Parsons historical material. It also proved readily applicable to the investigation of finance, where, once again, organizations were diverse, who to talk to was not known, and the topics to be discussed were not predictable with any certainty.

Turning to the spectrum of theory, it is apparent that the method, once chosen, does preclude certain theoretical dispositions - despite the need to separate theory and method discussed above. Positivistic hypothesis testing is difficult (but by no means impossible) if research does not start with an hypothesis; at another extreme a solipsist would find difficult the very definition of an organization, or a project. However, even if the ends of the spectrum are lopped off, a sizeable range of theory exists, and this inevitably influences the researcher's use of method. Some theoretical 'statement of intent' is thus desirable to indicate the areas in which the basic methods of this thesis sit.

The very idea of a 'theoretical spectrum' is something of a scholarly confection, but for careful heuristic use it remains a useful one. In the field of organizational analysis

Burrell & Morgan (1979) have become exemplars of such a spectral approach to theory. Morgan & Smircich (1980) further develop the methodological aspects of these spectra; along them, they associate assumptions about ontology and human nature with examples of method. They see all science as essentially metaphorical, and Morgan (1986) takes such an approach further still in his 'Images of Organization', by presenting a series of developed metaphorical views of the world.

All that is proposed here is to sample a couple of points along the spectrum of Morgan & Smircich where the present research might lie. The first of these sees "reality as a realm of symbolic discourse":

> Reality rests not in the rule or rule-following, but in the system of meaningful action that renders itself to an external observer as rule-like. (Morgan & Smircich 1980:494)
>
> The epistemology involved here does not hold that the findings thus obtained would be universally generalizable, but it does regard them as providing nonetheless insightful and significant knowledge about the nature of the social world. (p.497)

Second - "reality as a social construction", investigated through "ethnomethodology", where:

> Individuals may work together to create a shared reality, but that reality is still a subjective construction capable of disappearing the moment its members cease to sustain it as such (Morgan & Smircich 1980:494)

These two examples seem to catch something of the flavour of the data yielded by interviews. Indeed, ethnomethodology seems a good label for the method used here - Turner (1974:7) sees it as research having "a concern with <u>practical reasoning</u>":

> a style of research and argument responsive to its elected subject matter .... that there is an observable 'real world' is its point of origin: its destination is a characterization of the work members do to sustain a social order (Turner 1974:11)

The term's originator - Garfinkel - explains its origin in a search not for a research methodology, but rather in the study of people's own methodologies perceived as appropriate for everyday activities - he says of enthnomethodology:

> It is an organizational study of a member's knowledge of his ordinary affairs, of his own organized enterprises, where that knowledge is treated by us as part of the same setting that also makes it orderable. (Garfinkel [1968] 1974:18)

Either of the last two indented quotes could serve as eloquent statements of intent for the present research.

One final, very striking, observation has to be made - such statements sit ill alongside the idea of creating spectra of methodological theories. Turner says, of ethnomethodology:

> Theory and method lose their privileged position as part of the apparatus which belongs to the analyst, not themselves subject to inquiry, and take their place as phenomena whose status vis-à-vis other doings and accomplishments is not immediately obvious. (Turner 1974:7-8)

Wenger (1985:31) makes a similar criticism of the theoretical violence of Burrell & Morgan's (1979) attempt to taxonomize the interpretive. It is precisely this point that led to the refusal, discussed above, to conflate, and make equivalent, the distinct categories of theory, method and data in the way that Morgan and Smircich (1980), or Morgan (1986) do. A columbine remains a columbine regardless of how intimately it may be intertwined with a rose.

Nonetheless, the work of these writers is of great value in attempting to grasp the nettle of applied theory. The promise (failed though it be perhaps) of their multi-paradigmatic approaches is an influential thread in the radical theory-in-use in management studies today. To a degree, therefore, the methodological debate in the present thesis must answer the pathologies of these meta-methodologies. This debate will be taken up again in later chapters.

3.4 Access - Insiders and Outsiders?

There are several aspects to the question of access, but a central one is the potential difference between outsider and insider. The researcher starts outside, and, must first find out to whom or to what it is that he seeks access. The insider he then contacts will develop a view of the researcher that will determine whether access is granted and, if interviewed, the extent to which the researcher may gain access to the insider's thoughts and recollections.

Access initially depends upon the researcher's personal credentials - such as where his letter comes from, its content, his 'telephone manner', and who he is. With HOTOL, access to British Aerospace and Rolls-Royce was clearly needed, and here the present researcher's insider status in

the aerospace industry[2], and as an engineer, looked more useful than his more usual (and neutral) status as a member of a university. However, the original intention of seeking assistance from the researcher's old colleagues proved unnecessary - a visiting academic, once peripherally involved with HOTOL, suggested four possible contacts. Letters were sent to each of them, and all four replied. Two were interviewed straight away, and in turn suggested more people to interview. The other two were interviewed later, with others that they suggested being interviewed first. A network of people was thereby established, and letters requesting an interview were sent to those who seemed important, in some cases followed up with another letter or telephone call.

All the HOTOL interviews, save three, were carried out over a period of around eight months - the study was then written up. It has been taken up again, and re-written for the present thesis, incorporating two further interviews conducted over a year after the first set, and a final interview another year later. Only two of those contacted were not interviewed - in one case the man concerned did not reply, but was not re-contacted because he subsequently moved out of his position relevant to the project. With the other, a second letter elicited an interview with another very senior company member. Thus, all-in-all, access proved no difficulty, achieving a success far beyond that expected.

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[2] See the appendix to chapter one for further discussion.

The general intention was to interview as wide a range of people as possible, from those intimately involved through to those on the periphery. This intention was achieved, although the emphasis tended towards those most instrumental in the project. Nonetheless, some more tenuously linked to HOTOL were also interviewed, providing a sort of cross-check of the generalizability of the findings, as well as contextual details.

The next main data-gathering activity was the historical case-study into Charles Parsons. In the end this used published sources - the appendix to this chapter describes the conventional library searches that this entailed. Even here questions of access similar to those involved in interviews arise. Librarians and archivists are insiders within their own esoteric fields of scholarship, and entry to their store of knowledge, whilst easier to a student with scholarly credentials, must still be negotiated. Shared experience can still be beneficial.

Thusfar, the success in gaining access supports at least two possible interpretations - it either reflects a certain diligence and care in considering what people expect - or it reflects the researchers 'head-start' through being an insider to the aerospace industry. The decision to continue research in the City, looking at finance - an area completely unknown to the researcher - provided a good way to test this.

The first interview, conducted with a banker known to an academic contact (himself an ex-banker), was very useful. The next step chosen was to contact merchant banks, on the flimsy pretext that they had been active in finance since the time of Parsons, and they must know something about it. Fifteen letters were sent, mostly to the chairmen of merchant banks (but also to 3i and BTG) and this resulted in seven interviews (and one other short telephone interview, that was also used). This response rate seemed excellent, especially as no follow-up of unanswered letters was attempted, since the sample already seemed adequate.

Many of those interviewed, however, expressed the view that their experience of financing innovation was extremely limited (this belies their value to the research in providing the context for later interviews). However, as with HOTOL, these interviews led to suggestions for others, and also to a growing sense that interviews in New York would be very useful. Ten letters were sent to New York banks (mostly), along with twenty more UK letters - this time including a much wider range of institutions. Ten UK interviews resulted. The response from the US was, however, minimal - two replies, both refusals. Well over £100's worth of overseas phone calls, and only one or two tentative invitations, followed.

There were other reasons for a visit to the US, but, for a researcher sitting reading a paper on post-modernism in the airport lounge at JFK, the prospect loomed of explaining an expensive week in New York, and no interviews, to a generous research council. A new approach was clearly needed - a two

week break had been planned, and over this time twenty three faxs were sent (vetted for phrasing, idiom, and spelling, by a cultural insider) - three of the final interviews can be traced to these. Thus prospects of success looked almost as bleak as the Port Authority Bus Terminal did on arrival in New York (an experience akin to stepping into one of the sociological ethnographies of the Chicago School).

In the event, nine interviews resulted, all in the course of less than one working week - more than had ever been anticipated, and a most successful result. Securing them had, in the end, depended upon three things - a steady nerve, several rolls of quarters for the telephone, and an ability to keep track of multiple strands of communication inscribed in a small notebook, whilst shuttling between lower and mid Manhattan.

These observations are not intended to be flippant, although they might well serve as a caution to future researchers. Two conclusions of a sort emerge - the first was that setting up interviews from anywhere but New York, and anything more than a few hours beforehand was doomed to failure - the second was that multiple phone calls (and when requested, expensive faxs) were essential. People seemed to judge the researcher's resolve by his persistence, and they would then reward it by giving up any small fragment of spare time that may appear in their day. A subsidiary point is that adequate directories of financial institutions were not found until arrival in the US, although a more diligent search in the UK could possibly have unearthed some of them.

On return from New York, more UK interviews were set up, with the same facility as before. The most fruitful area for further research looked to be seedcorn venture capital. Letters were sent to eight (probably the eight) providers of 'seedcorn' funding, and seven were interviewed. A pleasing result. Around the same time three interviews with technologists were also carried out, another was added a little later - access here was also good - only three additional letters had been sent where no interview was carried out.

The conclusions to be drawn from this are not very clear. In terms of access, the aerospace industry, to which the researcher was an 'insider', was similar to the UK finance industry, to which he was an 'outsider', and what differences there were may well be illusory. Close to total success was achieved with HOTOL, whereas only around half the finance requests yielded interviews, but HOTOL interviews had to be with specific people - requests were followed up far more assiduously than with finance interviews, which merely sought representative people. In addition, lumping HOTOL interviews into a single category of 'aerospace' is hardly justified - the people interviewed came from diverse backgrounds, some of which were most certainly 'outside' the researcher's experience.

Even in New York interviews, being an English 'outsider' does not seem to disadvantage the researcher - he is treated with no less courtesy, or curiosity, on Wall Street than he is

in the City. The greater difficulty getting interviews there may reflect cultural incompetence, or it may simply be that US financiers have less free time. In all this, the insider-outsider debate emerges as a rather impoverished perspective.

A grand total of sixty seven interviews were conducted (the fact that only four were with women makes an interesting point about the cultural milieu of engineering and finance). Twenty five looked at HOTOL (including a second interview with a key participant to bring the study up to date); thirty eight investigated finance (including nine in New York); four interviews with technologists were conducted to illustrate specific points raised in the finance interviews. All of these are listed, with explanatory details, in the relevant sections of the appendices to the next two chapters.

3.5 Preparation for the Interview

The interview, central to this research, demanded care in its preparation and conduct. Methodological advice is readily available[3], but, in practice, interviews seem to have much more in common with everyday interpersonal experience than with such papers. Of more use were the comments, chapters and appendices retailing actual field experience that pepper the literature - what Van Maanen (1988) terms 'confessional tales'

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[3] McCracken's (1988) paper on the long research interview, and its place in interpretive research, is a good example here, as is Spradley's (1979) comprehensive, but rather proceduralized, overview of the whole gamut of practical ethnography.

- and even their use is primarily as a psychological balm, to re-assure the researcher that there is no mistake he can make that has not been made before, however bad it feels.

The main problem in approaching the interviews was that even the form of what might be learnt was unknown. Two things seemed necessary - to be prepared to guide discussion if required - but at the same time to avoid leading that discussion. These reflected very real fears, and prompted preparation of a notional theoretical framework for discussion, in case it was needed - but in practice it was not, and both fears proved groundless.

A final observation might be added here, on the psychological preparations that a researcher takes, even unconsciously. These emerge when he questions seemingly inconsequential, even trite, aspects of his research 'routine'. For example, why travel to finance interviews in London on the train, when the bus is cheaper? A little thought suggested that travelling in the same manner as those he interviewed was part of a mental preparation that felt important. Likewise, it helped to 'get the feel' of the City, or of New York, by walking around or sitting eating lunch surrounded by the workaday inhabitants, just trying to look like them. This micro-sociological theme will be continued in the next section, where the importance of dress will also be noted.

3.6 Conduct and Recording of the Interview

The aim of the interviews was to obtain each person's views and experience of innovation, along with the 'history' of the case-studies, and some insight into their 'worlds', however unexpected or strange. The results of each interview depends not only upon the person interviewed, but also upon the interviewer himself. There are three ways in which the interviewer influences the process - his acceptance as an insider influences the sort of things that will be discussed; his conduct, demeanour and control in the interview influences how they are discussed; his skill in recording them determines their subsequent value.

The first point has already been raised above - in the HOTOL interviews, being a professional engineer, and having worked in the companies involved, certainly seemed to lend the researcher an initial credibility that little else could have given him. This helped in small things as well as large - not needing to stop people to seek explanation of jargon, or some local nickname, is of great help. The flow of an interview, once arrested in this way, is seldom regained. Interviewing a set of people who know one another is also of considerable benefit - the key incidents and issues that emerge in one interview can guide the questions asked in subsequent ones. As Heclo and Wildavsky (1981) observe, the researcher: "cannot get information without at least a small fund to begin with". Indeed, in one of the finance interviews, the researcher had the distinct impression that it was he who was

being interviewed (although the interviewee did give the researcher lunch).

The finance interviews, did not seem markedly different from those on HOTOL - people seemed to accept the researcher, and most talked in the same unaffected and natural manner. Certainly, unfamiliar phrases, words, topics and abbreviations did arise, but there was still no need to stop the flow of the interview, the meaning of most emerged either naturally, or through judicious prompting, or from subsequent enquiries. More surprisingly perhaps, quite a few fellow ex-engineers were encountered in the areas of finance that were entered, and even a few mutual contacts. Also, judicious mention of the location of one interview could sometimes allay fears in another.

The New York interviews were the ultimate test of the interviewer in the present research. But even here no real differences were <u>felt</u> - the interviews proceeded as before. The jargon was a little stranger and idiomatic usages could cause minor problems (an elementary knowledge of baseball can be useful here). But discussion was again free and as much of it germane to the issues under investigation as in any of the UK interviews - in short, the researcher was accepted.

The question of acceptance is the main comment on the insider-outsider debate that emerges from real experience of interviews. The conclusion that the present research invites is that <u>once the researcher has negotiated access, he is to all intents and purposes then an insider</u>, regardless of how unusual to him the circumstances may be. This, on reflection,

hardly seems very remarkable - once admitted to the interview, any second thoughts on the part of the interviewer would question the action of granting access. Thus the person interviewed casts the interviewer (by admitting him) in his own cultural image; all the researcher has to do is use his cultural competence to live up to that expectation. This conclusion bears extension, beyond the researcher, to the people in HOTOL and finance when they encountered each other's worlds.

The interview is an interesting social-psychological encounter - its conduct is vital. The researcher must avoid giving the wrong signals, and must try to fit a favourable stereotype in the mind of the person he hopes will give him information. The probability that physical appearance is important led to a careful choice of clothing for interviews, the aim being to try and merge with what the people were likely to wear themselves. The most difficult choice was that of necktie. For HOTOL the choice of a Royal Aeronautical Society tie usually seemed the most appropriate, with a restrained silk tie for finance. Indeed, the researcher's most notable failure was the choice of plain blue tie for the interview with Sir Geoffrey Pattie, only to find he was wearing an RAeS tie. Appearance and symbols certainly seemed important to those interviewed.

Interview settings are usually chosen by the person interviewed (although settings as diverse as the researcher's own college room, Liberty's coffee shop, and the Royal Society

of Medicine were encountered), nonetheless the interviewer can sometimes do something to ease the formality - by choosing a chair that avoids too adversarial a position for example.

In the interview itself, imposing structure on discussion was seldom a problem, and experience was quickly gained. A useful introduction was a quick description of the researcher, or of his research, generally tailored to the interests of the person interviewed. This led up to an invitation to talk about the history, or the interviewee's experience, of the project, or of finance, and what the person considered to be important. Generally, this invitation was accompanied by the offer of an alternative strategy with the interviewer asking more specific questions; the framework for discussion mentioned in the last section, or issues raised in other interviews, were possible options here. In all but a few cases, however, people seemed more than willing to talk, almost unbidden, about their experience and experiences. When they did the result was generally more satisfactory, since a natural coherence and structure tended to emerge.

Usually, towards the end of the interview the question was asked whether anything else seemed important. Seldom was there anything, indeed in some cases such additional comments were freely offered during the course of the interview, prefaced by statements like: 'this is something you should know.' Neither were early fears realized of the interviewer imposing bias with 'leading questions' - a very broad area of discussion could be introduced, but within it, issues that the interviewee considered extraneous were politely but roundly

dismissed. All this suggests that the interview technique may have achieved data of a consistency belied by its apparently free format, it also, perhaps, raises the question of who is interviewing whom!

The final question is how the interviewer should record the data; this interacts with the way the interview is conducted. The three basic choices are to tape-record the encounter, to take hand written notes, or to reconstruct the interview from memory. Tape-recording provides a verbatim record, but requires lengthy transcription, and may miss nuances that interpersonal contact picks up. Hand-written notes were chosen here, the aim was to include as many verbatim quotes as possible, and to provide a précis of the rest in a manner as faithful to both the content and idioms of what was said as possible.

Without also trying tape-recording a comparison is impossible, but considerable benefits of the manual technique chosen here did emerge. First of all, it focuses the interviewer's attention to a quite extreme degree, so that his ability to return to points to request amplification, or to recall them afterwards, was considerably heightened. Second, some selectivity is inevitable, and this allows, in some degree, the non-verbal aspects of the interview to be reflected. Third, the ability ostentatiously to stop writing when a person talks 'off the record' does seem to offer a reassurance and a spur.

However, such a recording method was not, and must not, be approached lightly[4]. If something of importance is not noted, it cannot be regained. Written records demand humility totally to reject pre-conception and instant analysis, otherwise what is important to the interviewer, not the person interviewed, will be recorded. What a person tells may seem strange, it may seem irrelevant, it may seem disjointed, but often scrutiny after the interview reveals its significance. If the researcher eschews to record it, or tries to modify it, he loses that which is of the greatest value to him - the counter-intuitive.

Another valuable discipline, developed in the course of the interviews, was to review what had been written as soon as possible afterwards. This usually resulted in reconstitution or augmentation of some sections, and at the least a correction of errors, handwriting, and explication of extreme abbreviations. In one case, an entire interview had to be reconstructed after the event - this was not planned, but the interview in question, a key one, took place over an extensive, and good, lunch where note taking would have been so inconvenient as to be impracticable. Prior to this event methodological accounts of such a procedure (for example

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[4] Raffel's (1979) challenging analysis of bureaucratic record keepers is illuminating here, particularly his suggestion that completeness, not utility, is their arbiter of accuracy. Raffel's conclusion - that this is "claiming to know what they only think" (p.115) - pointedly underlines the observation of the present research that subjective interpretation is more challenging, more demanding of discipline, but also very much more productive, than positivistic methods.

Dalton 1959:ch.11) had been regarded with some scepticism, but in practice it was remarkably easy. As soon as possible afterwards, recollected topics were jotted down in random order over a period of time; these were later rearranged into the order of discussion, and fleshed out with remembered details and dialogue. These details prompted recall of other details, and these were added in a sort of iterative process until a full text emerged (even in typing this, a few additional fragments were added). Happily, the accuracy of the result was checked when the transcript was sent to the person interviewed, who replied just over two weeks after the original interview:

> What a remarkable recall of our conversation - without a notebook or tape recorder! I congratulate you. It is almost entirely correct but I have .... made some observations and a few corrections.

For completeness, a few minor points regarding the interviews deserve mention before ending this section. The general conduct of the majority of the interviews has been described, however there were some deviations from this norm. Their lengths varied - from half an hour to four hours - although one to two hours was typical. On one occasion a short interview was conducted by telephone. On three occasions, interviews with two interviewees present were carried out; on two more, the same person was interviewed again at a later date (one of these over three years later). None of these seemed to have any marked effect on subsequent analysis or use of the interview in question.

The original records of the interviews were kept; subsequently fair copies were made on a word processor, often much later. Any editing was confined to the removal of small amounts of material that were quite plainly peripheral, or to some grouping together of discussion of a specific topic. As facility with interviewing developed, this process of transcribing interview notes became increasingly routine.

3.7 Follow Up

A final step was taken in 'processing' interviews which, whilst administrative in intent, also served to inform interpretation. When asking people for an interview, it had been thought prudent to offer to send a transcript after the interview for comment. This provided an opportunity to check that no sensitive material had inadvertently been included, it was potentially useful in cross-checking purely factual matters such as dates and names, and - if objections were raised - allowed material to be treated as the interviewee wished with regard to inclusion within this final document.

People's responses were characterised by considerable generosity, and, if at times unexpected, they provided no major problems. People, when faced (often some time later) with the rather impressionistic raw transcript of their words, reacted without anger (even if slight shock was sometimes perceptible). Around three quarters of those interviewed in HOTOL responded with some comment, and only two requested that their material was not included verbatim. The first of these

(covering however a group of interviews) commented the "some quotes are included out of context", and that "incorrect inferences may be drawn from others", but helpfully included additional comments. With the second case, the person interviewed was kind enough to prepare a detailed written record of his recollection of the interview. This provided an invaluable resource, more than compensating for the loss of the original. In the case of HOTOL the 'raw' transcripts were used in the analysis described in the next section, and quotations within this document only attributed where the interviewee replied and had no objection.

About half of those interviewed in finance and technology replied when sent the transcripts (a third of those in the US replied). Comments in the replies were broadly similar to HOTOL, their general tone being friendly, helpful and encouraging. The discourse of these broad-ranging interviews was necessarily more fragmented, making the transcripts look a little alarming. A note of reassurance was struck in the covering letter when they were returned to people, but a lot wrote back, a little quizzically, saying the result was indeed a surprise. Most accepted this generously, a few expressed fears that their words might appear out of context, many included explanatory notes and emendations, and two sent re-typed amended copies; a small number also requested that they were not quoted. The general nature of the finance data means that there was no clear advantage in attributing comments, so this has not been done, although people have been differentiated by the broad category of organization from

which they come. Attributions to two of the four technologists have been made, however. Analysis of both finance and technology interviews was based upon these 'corrected' texts, informed in some instances by the originals.

Overall, the comments have provided an unforseen, but welcome, reassurance that what the researcher has interpreted and written is seen from the other side of the interview as a faithful (sometimes too faithful) record - particularly with HOTOL. Some of these comments are included below to illustrate this - responses to HOTOL interviews first:

> I have read your notes - one's words horrify one when seen in print, but the general tone is correct.
>
> While its cryptic style makes it difficult to alter I have little general objection.
>
> Most of them sound like me.
>
> Of course, I would love to rewrite them in a better light, but it would probably come out the same if we repeated the exercise.
>
> Somewhat frank in parts, but that is the way the story went.
>
> It reads a good deal racier than I remember, but perhaps you have recorded the juicier passages.

This last comment is one of some perspicacity. The researcher, hard pressed to write the last comment, while listening to the present one, gladly seizes the gift of a 'racy' phrase. However, in doing this the research method has perhaps succeeded in unearthing what it seeks - the symbols people use in their discourse to signal the things that are salient to them.

The comments after reading the finance and technology interviews were more mixed - they say a lot about how strange everyday discourse looks when lifted out of its everyday milieu and fed back to people:

> I have no comments save to compliment you on your ability to record longhand.
>
> I think your notes have the right feel, but there are bits I would want to sub-edit for publication.
>
> I find it difficult to comment; I accept your view that these somewhat disconnected quotations will ultimately come together in a coherent document
>
> I think that in the speed of taking down the conversation, some of the emphases appear to have got misplaced.
>
> it looks a bit like 'greek', but if you can make sense of it, ok.
>
> I will take your word that these disjointed utterances can be made intelligible.
>
> I fear that much is missing from the notes and the context of any comments is most important. Also the language used is conversational rather than scripted.
>
> You are quite right; the rather fragmented result can be a little alarming! However, you have done a remarkably accurate job of capturing the spirit of what I said
>
> At times I felt that the conversation reported was incomplete or wrongly reflected my views.

3.8 Analysis - Different Types of Data

> There is no real end to methodological analysis, no hidden unity to be grasped once the breaking-down process has been completed. Themes can be split up 'ad infinitum'. Just when you think you have disentangled and separated them, you realize that they are knitting together again in response to the operation of unexpected affinities. (Lévi-Strauss, quoted by Derrida 1978:287)

The initial aim of analysis was to see whether the interviews could be split into discrete themes. Such analysis demands a structured approach - this is, perhaps paradoxically, more, not less, necessary with an interpretive method. The researcher does not know what themes will emerge from the text, thus all the evidence in the interviews must be considered equally, otherwise his prejudice might bias where he looks.

However, structuring analysis does not imply a structural analysis. Categories emerge, but they are conceptual products of the researcher's mind - their full meaning is apparent only to him. For the interpretation to become fully apparent to others, the researcher has to re-interpret such categories through the action of writing an ethnography or a thesis. This section outlines how the basic analysis was structured - how the implicit categories emerged. The appendices to the next two chapters consider this analysis in greater detail, and also make these categories a little more explicit - but only to allow the reader a fuller appreciation of the data, not to imply any wider significance outside of the interpretation presented in this thesis.

It is best to start by stating the intentions of analysis - the texts of interviews and letters contain many layers and strands of discourse, the analysis seeks to tease these apart into more manageable elements. These sub-divisions cannot be imposed on the data, they emerge from it - for convenience here they are referred to as 'categories'. Further division is also helpful - 'factual' categories can be separated from opinions and affective discourse (arbitrarily termed 'topics' here).

Analysis of the HOTOL interviews allowed a method to be developed. It became apparent as more interviews were carried out that common elements were beginning to emerge. Many were expected, some were not. Others that had been expected never appeared. A false start was made - a list of emergent topic categories was drafted, and then applied to few interviews by trying to highlight (using coloured pens) passages that seemed to deal with each topic. This failed for four reasons - first, the list of topics seemed inadequately to encompass the data. Second, there were not enough different colours of pen to allow sufficient topics. Third, some passages obviously fitted more than one topic. Fourth, and perhaps the most damning, was that the researcher's recollection of what the topic label meant, changed over time. In addition, at the end there were still passages, large passages, unmarked.

A new approach was clearly needed, and this was suggested by another strand of analysis. Many different organizations and people were involved in HOTOL - in order to chart which

person mentioned which organization some form of cross-index of these factual categories was useful. Given this, it was in principle easy to extend such an index to include opinions expressed (the 'topics'), and also to include their substance.

This was the strategy adopted - to implement it several choices had to be made. The first was one of medium. A multi-dimensional medium was needed; a computer data-base approach was contemplated, but discarded as it was felt that the categories needed to remain visual to the researcher, be readily retrievable, and physically sortable. A card index was therefore chosen. Cards were separated into sections for each person interviewed (numbered in interview order); green cards were used for a cross-index of people, red cards for organizations (lettered for convenience), and yellow cards for the emergent topics (lettered, but with a question mark appended constantly to signal their speculative perceptual status).

Each interview was printed in a condensed typeface, and then physically cut up and the pieces glued onto the cards. Categories were allowed to emerge as analysis proceeded, they were not pre-conceived. A list was slowly created as each new category was encountered during analysis of the first interview to be considered (that of Alan Bond). As analysis proceeded the cards slowly filled up with their snippets of text. In principle new categories were allowed to emerge throughout analysis, although almost none did after the first interview. New organizations were added in similar fashion, and even a few additional people of whom mention recurred.

A few simple, even arbitrary, rules were imposed. First, everything in the interview transcript was included on some card or other, so nothing could be dismissed as irrelevant. Second, the problem of multiple subjects was covered by having a second copy that could be cut up as required. As a compromise, only this one additional copy was used, but only occasionally did this seem limiting. At the end, and sometimes later again, the uncut portions of this second copy could be reviewed, and additional fragments added to cards at that stage. The scheme also overcame the other problems - any number of topics could be accommodated, and previous extracts were readily accessible on the cards to allow the researcher to check his own consistency.

The Finance and technology interviews were treated in exactly the same way, as was the HOTOL evidence to the House of Lords Select Committee and the letters of Parsons and Fisher. These four sets of data generated four sets of categories, but, whilst the choice of later sets (insofar as they are chosen) was certainly informed by experience of earlier analyses, only one minor change was made to the analysis method. This was to 'cut up' the double copy of each interview into categories on the word-processor screen before printing. This proved more convenient, but the resulting print-out of categories was still stuck on to coloured cards identical to those used previously - thereby preserving all the benefits of the method.

The result was two HOTOL 6x4 inch card boxes, four finance boxes, and a further one containing the technology interview cards and the Parsons data (this printed twice as densely as the interview data, however). Comment generally seemed reasonably evenly spread between the emergent topics, but even where finer division was clearly possible, it did not seem necessary. Likewise, where two topics seemed to coalesce, it was perfectly easy to consider the two cards together.

These indices, then, formed the basis for the interpretation presented here - an easily accessible resource, distilling what had been learnt in the interviews. They were used as tools for writing the ethnographies of the next two chapters. To supplement the HOTOL and Parsons indices, separate chronologically arranged aggregations of interview or letter fragments were also used, to relate 'historical' details like dates, places, or events. A separate small index of people around Parsons was also built up while reading secondary sources. In the case of the HOTOL, Parsons, and technologists studies, the indices were used in 'person order'; for finance, the cards were rearranged in 'topic order' in the boxes - another argument for the use of physical cards.

Before moving on, four disconnected comments might be offered by way of a postscript to this section. First - one quite unforseen benefit of the analysis method emerged. This was that the time taken physically to cut up tiny pieces of

paper and glue them to cards, or to manipulate the files on a word processor, imposed a natural pace upon the analysis. This allowed time to think and prevented hasty judgements; the analysis benefitted.

Second, the analysis might seem open to the criticism that such small disarticulated pieces of information will be completely divorced from their context (as some returned comments implied). But such criticism does not seem appropriate in the case of the present interpretive methodology. All the researcher is doing is providing himself with cards as aides mémoire for subsequent interpretation; if the context is not in his head then he has already failed - the yellow cards only have meaning when interpreted as figures against that ground. Indeed, perhaps the main way in which analysis informs the text of this thesis is through the intimate familiarity and facility with the data that it leaves in the researcher's head. In any case, extracting successive comments on the same topic from a discursive conversation, and joining them end to end, often seems to put them into context, rather than doing the opposite - a far more lucid and coherent commentary than the original often resulted.

Third, an index card based analysis of structural elements has an extremely good pedigree. The technique used here, though very different, is genealogically related to that applied by Lévi-Strauss (1963) to the structural analysis of myth. This is encouraging, not simply because it might confer respectability on the method, but also for the deeper reason

that both methods have similar aims - allowing order to be seen in the apparent fragmentary chaos of human discourse.

This leads to the final comment - the nature of the data itself is important. The analysis method described in this section is structural only insofar as it orders 'data' - it does not constrict its richness and diversity. The emergent categories are still of very different types - facts are separated from topics, and the topics themselves lie across a very wide epistemological 'spectrum'. Some suggest the metaphor of rules or social structures; in others a structural metaphor has no meaning, they suggest rather an ephemeral reality, fleetingly created. To use Conrad's terms, quoted at the beginning of this chapter, a "spectral illumination" is provided by these categories - some reveal meaning as "a kernel", others as a "haze" around the ethnographic tale, while yet others simply provide the "moonshine" that brings it out.

3.9 Writing Ethnography

The texts of interviews, and the boxes of index cards, are largely invisible in the presentation of this thesis, they are merely intermediate steps in preparing a picture of people's worlds, and of innovation. The next two chapters present ethnographic descriptions of different worlds. Van Maanen says that ethnography:

> rests on the peculiar practice of representing the social reality of others through the analysis of one's own experience in the world of these others. Ethnography is

> therefore highly particular and hauntingly personal. (Van Maanen 1988:ix)

Atkinson (1990) notes that, whilst the methodology of ethnographic research is well covered in the literature, that literature is biased towards the early stages - "as the research process develops, so the standard sources have less and less to say" (Atkinson 1990:177). When it comes to a consideration of writing ethnography, he sees only his own and Van Maanen's book (just cited).

There is a paradox here - methodological debate in section 3.3 had to fend off applied theories coming at it from all directions. A very conscious attempt was made not to indulge in the anti-interpretive habit of post-hoc rationalization, and a sense of guilt remains that theoretical justification for choice of method, however spurious, was thus shunned. It is therefore strange that even a positivistically inclined ethnographer seems to have no need to justify the way chosen to present research, provided it stays within the fairly broad canons of what a social science thesis should be. Van Maanen, and Atkinson, redress this balance by suggesting that the methods of social science should properly be used to ask what those canons are, and why they 'should be'.

Atkinson uses literary theory to demonstrate the rhetorical nature of <u>all</u> scientific writing:

> the Enlightenment's divorce of rhetoric from science should not be taken on board by those whose job it is to understand precisely how categories and contrasts like those are produced, shared and reproduced. (Atkinson 1990:175)

He focuses on sociological ethnography, and warns that although theoretical debate may appear to be absent, its invisible presence still stalks the prose. Atkinson's (1990:10) observation that: "Sociology is as inescapably rhetorical a discipline as it is a moral discipline", can be extended into other areas - many of those interviewed here clearly seemed to be using rhetoric in their own disciplines.

This section tries to give a flavour of Atkinson's and Van Maanen's critical way of looking at research texts. It carries forward the warning in section 3.3, that theory and method are separate philosophical categories, from the gathering of data to its presentation. The danger here is that the ethnographer re-incorporates fragments of theory into the text, and that instead of a thesis developing new theoretical standpoints, these old ones pop out unperceived. This is a danger only when it is unperceived, and the critical approaches to ethnography described here enforce its perception.

The primary point that both writers make is that researchers interpret field work in some way, and render it in textual form; ethnographies: "necessarily decode one culture while recording it for another" (Van Maanen 1988:4). An ethnography: "conveys to the reader a sense of place and of persons" (Atkinson 1990:62). To be successful, the ethnographer must:

> persuade his or her reader of the authenticity, plausibility and significance of representations of social scenes or settings (Atkinson 1990:57)

Atkinson describes this as a 'narrative contract' between reader and writer.

Both writers analyse how the text performs this rhetorical feat. Atkinson's thematic approach looks at textual figures, arrangements and devices. Van Maanen extracts three 'ideal types' of presentation which he terms 'realist', confessional' and 'impressionist' tales. Both writers show how texts are laden with hidden persuasive intent, both reveal their embedded rhetorical figures and features.

Two implications for data need to be borne in mind. First, if theory is implicit but left out, then so is most of the data - the narrative contract draws upon the cultural knowledge of the reader to fill in the gaps:

> The sociologist furnishes fragments of 'another text': these fragments are - as it were - glimpses, seen through the gaps in his or her sociological account. The reader draws on these fragments in order to reconstitute that 'other' text of everyday life in the setting in question. (Atkinson 1990:91)

This leads into the second point - the data presented, be it contemporary or historical, is incomplete precisely because it has to be - ethnography accumulates vast quantities of data:

> The historian and the ethnographer share very similar commitments and resources. Each is committed to a systematic and thorough account of cultural phenomena, yet neither can reproduce all the 'evidence' and detail available. There is an element of 'bricolage' in both types of writing. (Atkinson 1990:49)

The elements that the bricoleur uses include 'typical cases', and the whole range of Atkinson's 'exemplars'.

The fragmentary nature of a bricoleur's collage is a good metaphor for ethnography. It is a motif that keeps recurring,

and it is challenging to take it a little further by way of conclusion. Van Maanen (1988:101) carefully chooses the artistic metaphor of impressionism to illustrate ethnography; this thesis chooses collage. A particularly instructive example is David Hockney's remarkable photo-collage of a luncheon in the British embassy in Tokyo (Hockney 1988:126-127) presented in figure 3.1 below. Hockney presents multiple images of the same thing, he chronicles an event, distorts any reified 'objective reality' in order to present a truer interpretation, and leaves parts of the picture out (in the process thereby defining them, of course). A collage, just like the ethnographies of this thesis, must always remain partial and incomplete, as Hockney beautifully describes:

> I took the photographs over the period of the lunch. I tried to do it without intruding too much, so I never moved from my place at all.
>
> When I put the picture together, it really did look as though the viewer was in my place at the table, having conversations with the other people. .... I didn't make the collage until I returned home. I remember it took quite a while because I had to use the curves. Later on I could have got rid of those curves if I'd wanted to, but I would have needed more photographs, and I couldn't very well go back and re-create that scene .... (Hockney 1988:179)

Such a collage is a peculiarly apt metaphor for writing ethnography.

FIGURE 3.1 - LUNCHEON AT THE BRITISH EMBASSY, TOKYO, FEBRUARY 16TH, 1983. David Hockney



The importance of Atkinson and Van Maanen in this research, is the way they show how writers' theoretical predilections creep into their texts, whether they will or no. These ideas are close to Derrida's concerns with logocentrism, and the very fact that this thesis considers them here may well reveal a trace of its own latent theoretical concerns.

If all texts, at all times, contain theory, method and data, then consideration of any text might lead to fruitful understandings. An understanding of the way that this text is constructed may inform the reader's interpretation of how the people interviewed in this research construct their worlds. In the same way, entry into their worlds and discourses has informed the construction of this text.

3.10 A Data Primer - Definitions and Conventions Adopted

This section summarizes how the data were analyzed, and tidies up a few working definitions and conventions that are used throughout this thesis. Placement here, before the chapters of data and discussion, seemed convenient to attune the reading of data to the ways in which it will later be used.

An interview yields a transcript recording what is in fact a dialogue - an encounter between two people. The personal letters of Parsons and his contemporaries are very similar to these transcripts. All are texts through which various discourses can become evident. Discourse was

considered in some detail in the last chapter, but a working definition with which to enter the next two chapters is useful. O'Sullivan et.al. (1983) trace the usage of this complex term at some length, and (at the risk of over-simplification) a few extracts from their definition will suffice here:

> In its established usages, discourse referred both to the end result of thought and communication. Discourse is the social process of making and reproducing sense(s). .... Discourses are the product of social, historical and institutional formations, and meanings are produced by these institutionalized discourses. ....
>
> Once the general theoretical notion of discourse has been achieved, attention turns to <u>specific discourses</u> in which socially established sense is encountered and contested. .... Textual analysis can be employed to follow the moves in this struggle, by showing how particular texts take up elements of different discourses and <u>articulate</u> them (that is, 'knit them together').
>
> However, though discourses may be traced in texts, and though texts may be the means by which discursive knowledges are circulated, established or suppressed, discourses are not themselves textual. (O'Sullivan et.al. 1983:73-74)

This entire thesis constitutes an analysis of these texts, and this chapter has described some of the more procedural techniques developed along the way. It also introduced specific uses for certain terms which are worth repeating. The analysis grouped together words, phrases, and sentences, that seem to describe the same thing - whatever that thing is. These groupings have been termed <u>categories</u>; they ranged from the obvious and factual to much more vague discussions of a concept, idea or feelings. These vaguer categories were given labels that suggested, if only to the researcher, some sense of what they represented for him; they

have been termed topics. These topic categories, whilst largely hidden in the text of the thesis, have been central to the researcher's interpretation.

The reader, if served up with a diet of raw topics, would receive a thesis 'scarce half made up', nonetheless topics and their analysis do deserve to be recorded. Primarily for this purpose, an appendix is provided for each chapter. These appendices also contain some other relevant matters, too detailed or peripheral for inclusion in the main body of the thesis.

Finally, a set of textual conventions has been adopted for the quotation of interview data in this thesis. The quotations are presented in traditional fashion - short extracts are included in inverted-commas (""), longer extracts are simply left-indented and single line-spaced. Within the extracts three broad sub-divisions are distinguished. First - where a person's actual words were recorded verbatim, they are presented in additional inverted-commas, and gaps indicated by dots ("...."); this does lead, in a few short (i.e. non-indented) quotes to a rather inelegant use of double inverted-commas, but this has been accepted as inevitable. Second - when a person's words have been rendered in paraphrase no additional inverted-commas are used. Third - where amplification was provided by a person after an interview, this is included in square brackets ([]); in the few cases where a person interviewed rewrote their entire interview, square brackets have been omitted as inappropriate. Square

brackets have also been used occasionally to include minor additions that maintain the sense of an abbreviated extract. Abbreviations are indicated by dots (....), although in the case of verbatim quotes, no differentiation of excisions made at the time of interview from those made later, has been attempted.

These conventions appear irksome at times, but they do allow idiosyncratic idioms and words to be reliably imputed to a person and not the interviewer (although paraphrase has attempted to maintain sympathy with the usages of the person interviewed - for example US spellings are used in US interviews). This is considered important because the discussion of later chapters considers usages and words at various points.

CHAPTER 4 : THE PROVIDERS OF IDEAS - SOME CASE-STUDIES

> '.... It sounds rather ridiculous, but I was riding along on my bicycle past the Radcliffe Infirmary when I had one of those sudden clarities, the kind that sometimes come in dreams. I remember saying aloud with absolute conviction, 'But of course! Of course that's how things really do work.' But I couldn't reproduce the argument that had led to this, although the sensation was the same as having been convinced by reason (though without any reasoning). And I've since thought that one of the explanations as to why one can't recapture the wonderful argument or secret when one wakes up is simply that there wasn't one, but there was some kind of direct appreciation by the mind without any chain of argument as we know it in our time-serial life.'" (Tolkien, as interpreted and recreated by Carpenter 1978:144)

4.1 Introduction

This chapter and the next contain the 'data' derived from analysis of the interviews. The present chapter presents the two larger case-studies, and the set of smaller 'vignettes'. All of these use the words of the people themselves to describe the process whereby new technological ideas become innovations that affect the world at large. Some of these people have the ideas, some are merely commentators, others become involved in the diverse, and often strange, activities that allow ideas to flourish.

The first case-study is an historical one - the development of the marine steam-turbine. It is based upon first-hand, but nonetheless published, sources. Its material is different from, but comparable with, the other data in this thesis. A great benefit is the light it throws upon the machinations of innovators and government - matters hidden from the eye of the researcher in a modern case-study where

the actors are still alive and capable of being hurt. A happy coincidence here is that the technology of the marine steam-turbine in many ways led to that of the HOTOL engines of the contemporary case-study.

The HOTOL proposal is for a single stage re-usable space launcher, made possible by a radical new propulsion system. Its case-study is the most detailed of this thesis - most of those seen as instrumental in it by their fellows were interviewed. The story is unfinished - it was this that prompted conduct of the 'finished' historical case-study.

These two studies invite comparison, and later chapters will compare various elements, but it is useful to preface this with a comparison of their costs. Charles Parsons first step was to produce a prototype - the 'Turbinia'; this cost £16,000 in 1897 (Appleyard 1933:88), maybe three quarters of a million pounds today[1] (although such conversions are highly speculative). The first small turbine passenger steamer cost £34,000 in 1901 (Appleyard 1933:194), maybe one and a half million today; Brown (1983:87) gives the cost of the propelling machinery for 'Dreadnought', the turbine's first very large scale naval application, as £320,000 in 1905, maybe fourteen million today.

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[1] Based on 'all items' consumer prices index in Liesner (1985:23) from 1900 to 1983, and 'internal purchasing power of the pound' in "1991 Edition - UK in Figures" (Government Statistical Service leaflet) from 1983 to 1989 - £20,000 in 1900 would buy £880,000 worth of goods in 1989.

The interviews show that probably five million pounds has been spent on HOTOL so far, and that the whole programme would cost five or six billion. Germany has probably spent one order of magnitude more on their comparable launcher idea (see, for example 'The Independent', 3-4-1989), and the US two orders of magnitude more ('The Times', 23-3-1989). Thus the cost of high technology appears to have increased over the last century, and the cost of realizing HOTOL is now more than any single investor or company could bear.

Of course, both HOTOL and steam-turbines were potentially very large innovations, but their early stages were small, and these can give insights into much smaller-scale ideas. Their study can thus offer the best of both worlds, given a methodological stance that is sensitive to the possibility of general elements within the specific. Such an approach has been adopted here (the appendix to this chapter describes the analysis in greater detail). It has been further enhanced by the limited smaller-scale studies at the end of this chapter.

Case-studies are usually set within a clear organizational context. This case-study is not, because the interviews showed that innovation depended on a great many extra-organizational contacts, and that many of its problems were not primarily organizational. In some, perhaps most, cases the organizational context was all but irrelevant. The method chosen here has tried, therefore, to maintain a broad

contextual focus. The result may look unconventional, but the touchstone must be its utility.

4.2 An Historical Case-Study - Parsons and the Marine Steam-Turbine

4.2.1 INTRODUCTION - THE MATERIAL

The problem in looking at innovation is that it takes time - any one set of interviews is only a snapshot, the process of innovation (if a process exists) has to be inferred. Even the expansive timescale of most theses cannot capture a 'whole' innovation, so injection of an historical perspective is called for. This has the additional benefit of introducing a sort of serum of radical doubt to inoculate the researcher against modernist notions of the universality of progress - things might be different in another time and another place.

Chapter three outlined the two-fold requirements of this historical perspective - a technological innovation similar to HOTOL, with material similar to interview data, was needed. Steam-turbines clearly answered the first need, indeed, they were initially chosen because of the similarity between HOTOL's unplanned début on television news, and Parsons's publicity 'coup' when 'Turbinia' appeared unannounced at the Spithead Review. It made sense to base a study around Parsons, and to include Fisher who was clearly associated in

the public mind (rightly or wrongly) with the development of a turbine navy.

Finding the right data demanded more care; here the case has been made for using published letters and papers as surrogates for interviews. Suitable material is available, as fully discussed in the appendix to chapter three (its analysis is detailed in the appendix to this chapter), but the possibility remains that those who have collected it for publication may have introduced bias. Marder's selection of Fisher's letters is so comprehensive as to allay such fears (Marder 1952, 1956, 1959), but Appleyard's (1933) biography of Parsons - a curious book, hagiographic, florid in style, yet imbued with a certain integrity - raises more doubts. Appleyard is uncritical, and certainly might have suppressed material detrimental to Parsons, but he argues no strong case, and his lengthy selections from Parsons's letters seem more random than manipulative. In short, it seems reasonable to use his book, with circumspection.

Selectivity in what is preserved might also introduce bias - Mackay, in his biography of Fisher, says that Fisher "destroyed most of the letters from those distinguishable as enemies" (Mackay 1973:212). Such problems can only be circumvented by a critical review of the literature which describes the context in which Fisher and Parsons worked, and the men themselves; the next sub-section provides this.

Throughout this section the main sources for Parsons's and Fisher's letters are Appleyard (1933), and Marder's three volumes (1952, 1956, 1959). For convenience, these books will

be referred to simply as 'A', 'MI', 'MII' and 'MIII' respectively.

4.2.2 PARSONS, FISHER, AND THE ARMAMENT BUSINESS

PARSONS:

Appleyard outlines Charles Parsons early life - born in 1854, the sixth son of the 3rd Earl of Rosse, he grew up in Ireland, was educated at home in a free and practical way, then Trinity College Dublin, followed by mathematics at Cambridge. He then spent three years at Armstrong's Elswick works as an apprentice, followed by partnerships, first with Kitsons of Leeds and then with Clarke, Chapman & Co at Gateshead, where his work on steam-turbines began.

Various contemporary reminiscences of Parsons exist, Clarke's (1984) sensitive monograph presents an excellent selection, and Lord Rayleigh writes a memoir in Parsons's nephew's collection of his papers (Parsons 1934:xv-xxvii). Rayleigh is a token of Parsons's circle of acquaintance - a notable scientist, Balfour's brother-in-law, and one of Fisher's sponsors on entering the Lords in 1910; his niece also married Parson's nephew in 1915 (Marder 1956:313, Appleyard 1933:304insert).

Rayleigh describes Parsons as: "a singularly modest man", whose: "want of self-assertion was at times almost comic", but also: "naturally temperamental", and: "not always reasonably patient" with his craftsmen. He notes

Parsons's delight in the more physical manifestations of engineering: "He had a boyish pleasure in any experiment which ended in a big bang or flare up", adding Parsons's comment: "'Well, we have the satisfaction of having bust it.'" In all this Parsons seems a fairly typical engineer.

Parsons's methods of enquiry also seem fairly typical of today, even though they seemed to excite his friends' and biographers' comment as being unusual. Rayleigh, for example, observes that Parsons himself affected never to use mathematics. The impression emerges of some mysterious creative process, allied to experimentation and model-making, but devoid of rational scientific calculation - an impression refuted by Parsons own letters:

> 30-01-1894: In regard to the boiler, this has been very carefully considered in conjunction with all the available data I can find, and I have carefully calculated the proportions of steam and water ..... I tried the model in a quarry close by, with comparatively big waves (A:96-97)

Parsons also makes frequent reference to consulting his peers upon technical matters. The fact that Parsons's contemporaries needed to see his 'real-life' methods as odd, is perhaps evidence of the scientific rhetoric surrounding innovation that later chapters will seek to explain.

As to Parson's writing and speech, Rayleigh says he wrote letters: "not very systematically and for the most part late at night. He never dictated letters", whilst Clarke quotes Rayleigh's observation that: "His want of readiness and lucidity of speech hampered his usefulness .... He was painfully conscious of this". A useful chronology of

Parsons's life is provided by Clarke (1984:79-82).

FISHER:

Fisher was a man very different from Parsons, and a copious letter writer:

> He rarely used the telephone in his work, preferring to transact business in person; nor did he do much dictating. Nearly all his correspondence, official as well as personal, was in his own writing (Marder 1956:18)

The two scholars who have published his letters and official papers - Marder and Kemp - introduce him:

> Regarded by the Royal Navy in his own lifetime either as a saint or a devil .... 'Jacky' Fisher has remained in death a subject of controversy. (Marder 1952:9)

> A very close friend wrote of him: 'He was a mixture of Machiavelli and a child, which must have been extraordinarily baffling to politicians and men of the world'. It is a profound summing up of his character (Kemp 1960:xxvi)

Fisher, incontestably, had very great influence, it is also likely that the marine steam-turbine benefitted from this influence, but whether, without Fisher, it would have found another, perhaps better, champion, is a problematical question. Fisher is worthy of study because his letters lay bare the fine detail of his world and his influence, in a way that no contemporary study could.

Fisher's world is also made accessible through biography and scholarly comment. There are the two quite remarkable volumes of his memoirs ("Memories" and "Records" - Fisher 1919a&b), and a biography by one of his protegés - Bacon. Other biographies have followed, Hough's 'authorized

biography' in 1969, Mackay's in 1973, together with Marder's biographical comments. Fisher's biographers have, for the most part, treated him kindly. The story of his life is too well documented in these books[2] and elsewhere to need repeating here. All that this section tries to do is sound a very brief note of caution against uncritical acceptance of Fisher's, and his biographers', words.

Between 1892 and 1897, Fisher was Controller of the Navy - he ran Naval procurement, and was a primary influence upon naval engineering and architecture. Fisher used alliances with politicians, armaments suppliers, fellow officers, and above all the press, to push the schemes in which he believed. Important here was Sir William White - Director of the Admiralty's Royal Corps of Naval Constructors, and a key figure alongside Fisher. Brown (1983), and White's biographer, Manning (1923), consider this period:

> The Controller's Department was scarcely large enough to contain two stars; and it should be remembered that White had attained the end of his ambition, while to Sir John Fisher, the Controller's office was but a step towards greater things.
> (Manning 1923:379, also quoted by Brown 1983:80)

Equally illustrative of Fisher's character are his own comments about White:

> 03-12-1894: I've heard there is a civil K.C.B. now vacant .... and thought possibly it might be obtained for White. (MI:123)
> 04-01-1902: I heard someone say of White when converted against his will that 'he would talk a bird out of a tree'! (MI:357)

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[2] Bacon (1929 vol.I:xxiii-xxv & vol.II:v-vi) provides a succinct chronology of Fisher's life.

His relations with White were later to change radically, but at this period they illustrate the many similar relationships through which Fisher worked, their import is well summed up by David Brown's comment: "Fisher's enthusiasm may have helped some but his enmity put off a lot of others" (D.K. Brown: private communication). Fisher's enthusiasm is neatly encapsulated by his belief in speed, or as he would have put it SPEED! - Bacon quotes one of Fisher's lieutenants:

> 'Jacky' was never satisfied with anything but 'Full Speed.' .... He loved dash and making a fine effect. (Bacon 1929,vol.I:116, part quoted by Marder 1952:102)

Fisher often secretly used the press to push his schemes. Bacon and Marder defend him in this, but Mackay's biography is less partisan - he fleshes out Fisher's contacts with the press, quoting Stead, a sensational journalist of the time: "I used to meet him, as he said, like Nicodemus, at night, in all sorts of out-of-the-way places" (Mackay 1973:179). Fisher, was subject to a 'bad press' too. Some of this came, in later years, from none other than Sir William White writing as 'Civis' in the Spectator (e.g. 'Civis' (1907)). Fisher's views of White seem to change in this period:

> 23-02-1907: I am anxious to expose Sir W. White and shatter his bubble reputation. (MII:119)

> 12-1910: A Navy scare is the Tory sheet-anchor! at judicious intervals. That pestilent cad, Sir William White, is, I fancy, Providing Winston with ammunition against the Navy Estimates. (MII:345)

In summary, then, Fisher was a character, like much of his writing, writ large on the page of history. Whether his

influence was malign or beneficial is a question that this thesis does not need to answer, what matters here is that it was effective. When an idea fits in with the views of a man like Fisher it is likely to get the support it needs to grow - Fisher provides a fine study in influence.

THE FIN DE SIÈCLE ARMAMENT BUSINESS:

Chapter one gave some indication that, at the turn of the century, change was in the air in British industry. Warren (1989) expands upon this in his 'business history' of Armstrongs. Armstrongs keep cropping up in the steam-turbine story - Parsons was an apprentice with them, Fisher was nearly seduced into working for them, White did for two years, after which Watts replaced him. They built 'Cobra' - one of the first two steam-turbines destroyers. Brief study of Armstrong's is a good introduction to Parsons's industrial world.

The 1880s and 1890s were decades of change, armaments firms were becoming larger and more integrated. Stuart Rendel, a key director of Armstrongs, observed in 1884: "all the charm of the work has gone out of it for me now that we are no longer a family firm" (Warren 1989:25). These were decades of rapid technological change, vehement political argument and 'Navy scares' (Marder 1938). Industry, the Admiralty, and Fisher did not control these waves of

imperialist hysteria, but they certainly used them if they could, as Fisher's letters to the first Lord illustrate:

> 22-10-1894: Lord Rendel spent over an hour with me on Friday last .... What he wanted was to get the order for 7 ships for Elswick instead of only 5, and he frankly told me Noble had sent him for that purpose. (MI:123, also Marder 1940:34, note 16)

It is plain that a web of links between industry, government and finance existed. Marder (1940:27) notes: "Numerous banks invested large amounts in armament stock, as did prominent bankers like the Rothschilds and Sir Ernest Cassell". Marder (1938:247) notes the intimate social and family ties here as well, and in both references discusses allegations of politicians exerting undue influence in favour of their armament investments.

Marder also illustrates the intimate contacts and exchanges between people in the Admiralty and industry:

> Another practice of the armament magnates was their eagerness to avail themselves of the services of responsible officials at the Admiralty and War Office, for the knowledge and 'contacts' possessed by these men was of great use to them. (Marder 1938:246)

Marder sees this as rather more sinister than does Brown (1983), who, citing similar examples, presents a more rounded and convincing picture of such contacts. Nonetheless, industry's use of technical skills can be criticized - Warren quotes one commentator's view of Armstrongs, that: "'brilliant technical expertise was wasted or suppressed or shunted into obscurity'" (Warren 1989:32). Of special interest here are steam-turbines; Armstrongs were interested, but dithered interminably - thus in 1912:

> arrangements suggested by Sir Charles Parsons - apparently involving sale of this section of his business to Armstrongs - were reckoned unsatisfactory. (Warren 1989:139)

All this has a remarkably contemporary ring, as does Armstrongs' 'salaries crisis' - in 1908 their net profits were only £277,011, but the five Executive Directors received between them £44,938 (Warren 1989:ch.21). Fisher was tempted with large salaries to join industry (including Armstrongs) on various occasions, but his letters show his strong purpose and integrity here, not to mention his political radicalism (even if he was not averse to mentioning these qualities):

> 21/06/1910 25 years ago. I was offered practically £10,000 a year .... and the certainty almost of being a millionaire. I declined, because I wanted to be Commander-in-Chief in the Mediterranean, Commander-in-Chief at Portsmouth, and First Sea Lord, and work a revolution! (MII:328-329)

Much of today is mirrored in this brief survey of industry a century ago, and this gives courage to develop the story of the marine steam-turbine. Then, as now, industry and Admiralty shared the same language; interchange of people is more to be expected than to be seen as sinister. Then, as now, the press took great interest in things military and technological; such things tend to go fast, make loud bangs, and supply exciting copy - it is hardly surprising if those involved try to manipulate such interest. Then, as now, aberrations in industry exercised both popular and scholarly debate - high prices, exorbitant directorial salaries, and perceptions of inefficiency and gross organizational pathologies.

4.2.3 THE MARINE STEAM-TURBINE - Early Days, Steam-Turbines and Patents - the 1880s.

Parsons first steam-turbine ran a mere three and a half years after he finished his apprenticeship at Armstrongs, by which time he was working at Clarke Chapman:

> About the year 1884, circumstances being favourable, I determined to attack the problem of the steam turbine (Parsons, quoted by Appleyard 1933:29)

Parsons's earliest provisional patents (nos.6734 and 6735) were dated 23rd April 1884, and many more followed. The design was an axial-flow compound steam-turbine engine. In 1898, Lord Macnaughten well summed up Parsons's achievement, in his judgement on Parson's petition to extend patent 6735:

> Many persons have endeavoured to employ the velocity of steam for the purpose of causing rotary motion without the intervention of any reciprocating apparatus. But no one before Mr. Parsons ever succeeded in producing a steam turbine of practical utility.
> (quoted by Appleyard 1933:86)

Parsons initially applied his turbine to electrical generation, but, Appleyard (1933:87) says, by 1898: "From a pecuniary point of view the result so far as land turbines were concerned had been disappointing". Electrical generating sets had been sold, demand had grown, but the general tenor of Appleyard's (1933:ch.3) review is that until Continental exports started around 1900, the market was far from assured.

The question facing a study of innovation is - how, if profits were low or absent, was the development financed? A

simple answer is that Parsons put £20,000[3] into his 1884, one-eighth, partnership in Clarke Chapman:

> this had the advantage for him that he at once began to take his one-eighth share of the profits .... Consequently he did not have to wait for his own patents to produce income. (Appleyard 1933:74)

However, in 1889 Parsons and Clarke Chapman dissolved their partnership, and Parsons (together with twelve of his colleagues) left to found C.A. Parsons & Co. at Heaton, with a staff of forty eight (Appleyard 1933:48, Clarke 1984:13). Clarke suggests:

> Most likely the root cause was the wish of his business partners for profitable output of at least a relatively stable design, whereas Parsons wanted both money, time and people for research and development .... however much a temperamental outburst was the immediate circumstances of the break with his partners. I doubt very much if Parsons realised he would lose the use of his patents (Clarke 1984:9)

> The loss of the axial-flow patents was a considerable blow, to get around them Parsons had to develop a new radial-flow design; Parsons did not re-purchase them until the end of 1893, for £1,500. These patents were evidently licensed in many different countries, which provides a useful commercial insight into Parsons's business.

Very much later Parsons summarized the achievements of this period, and showed that his thoughts were also turning to the sea:

> 09-07-1914: I thought it worth trying. Encouraging results followed, one improvement led to another, and it gradually became an efficient motor. When it had beaten a compound engine driving a dynamo, my old friend Dr. John Bell Simpson said to me one day when we were out

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[3] Probably something over three quarters of a million pounds today - see discussion and footnote in section 4.1 above.

> shooting: 'Why not try it at driving a ship?' To which I replied that I thought the time was ripe for the attempt. (A:47)

This perhaps provides a creation myth for the marine steam-turbine, but Parsons's own words describe the reality of such an invention:

> .... let us for a moment consider in what invention really consists, and let us dismiss from our minds the very common conception .... that invention is a happy thought occurring to the inventive mind. .... Generally, what is called an invention is the work of many individuals, each one adding something to the work of his predecessors, each one suggesting something to overcome some difficulty, trying many things, testing them where possible, rejecting the failures, retaining the best, and by a process of gradual selection arriving at the most perfect method of accomplishing the end in view. (Parsons's Address to the British Association - 1904 - reproduced in Parsons 1934:42)

Parsons goes on to describe its subsequent commercial aspects:

> this afterwork often involves as great difficulties and requires for its accomplishment as great a measure of skill as the invention itself

4.2.4 THE MARINE STEAM-TURBINE - Small Ships - the 1890s.

'TURBINIA':

Parsons, to demonstrate the marine steam-turbine, looked for financial support for the construction of a boat:

> 03-1893: I have in all cases clearly pointed out that there is considerable risk, but that I consider the matter as sound as can be seen without trial on full-sized scale, so that no one can blame should anything happen. They are all apparently very keen and think the venture at the present time, when fast speed is the rage, very opportune. (A:101)

A company was set up, as Parsons records in his letters to his brother at the time, also describing the technical progress:

> 22-12-1893: Enclosed is a revised prospectus of the boat company with the directors' names, etc. .... We are just

> starting preparations to build the hull and shall presently have about twenty men on this part of the work (A:90)
>
> 25/01/1894: There are now 244 shares taken up .... this only leaves 79 shares remaining, which is very satisfactory. I am arranging to try the 6-foot model in rough weather to see how it behaves. (A:93-94)
>
> 26/01/1894: The company was registered yesterday. I enclose several copies of the circular in case you may want some. The plates and angles are all in now and the building operations will soon commence. (A:95)

The Patent extension judgement of 1898 includes a summary of the arrangements:

> 19-04-1898: In January 1894 he granted to a company called the Marine Steam Turbine Company, Limited, an exclusive licence to use for the purpose of marine propulsion, and for that purpose only, the Patent No. 6735 of 1884, together with a large number of other patents belonging to him .... In consideration for this licence he received £9,000 in fully-paid-up shares in the company, which had an issued capital of £24,000 out of a nominal capital of £25,000 .... The company built and equipped the 'Turbinia' at a cost of about £16,000. Mr. Parsons executed all the work for the company at net cost, and without making any charge for his services. (A:88)

A lot of interesting points emerge here. Money for the new company was raised, it appears, without too much difficulty. The company was registered a full month after Parsons was 'just starting preparations to build the hull' - his old company presumably financing these operations. This is also suggested by the patent ruling's mention of the apparent no-profit transfer price arrangement between the two companies, and Parsons himself eschewing a salary.

Even at this stage, Parsons's long-term aim was large ships, as a letter makes clear:

> 28-01-1894?: These arrangements appear more suitable to a large ship than a small boat, and when we come to large ships, as we hope to do, they should apply largely.

(A:91)

The 'Turbinia', however, was small: "In view of the large amount of alteration that would probably be required" as Parsons said (Parsons 1897:8). She was started in 1894, but as Parsons reviewed in a contemporary lecture:

> 11-10-1897: The fulfillment of these anticipations was, however, much delayed, and almost frustrated, by .... "cavitation". (Parsons 1897:8)

She took three years to develop, Appleyard is rich in letters describing 'the large amount of alteration' that <u>was</u> required to beat cavitation. A few examples from Parsons's 1895 letters to his brother catch something of the flavour:

> 04-02-1895: The weather is bad, but we are getting on a little with painting 'Turbenòce'. We may be ready to run on the 16th if weather allows painting between now and then. (A:100)

> 03-10-1895: We had a preliminary run with the boat yesterday to Tynemouth and back. .... I think, on the whole, the first run is quite satisfactory. .... We shall now work things gradually up, and so far there appears no reason to anticipate much further expenditure in the experiment stage, though it may be too soon to give a reliable opinion. (A:121-122)

> 26-11-1895 We propose to have a Directors' meeting to consider the Foreign Patents for the boat and some other matters of minor importance, on December 4, and I hope all present alterations completed by then, and to have a run at over 20 knots. If all works well, we may get near 30 knots, or over, on 1,000 to 1,200 H.P. (A:124)

By 1897 Parsons's letters begin to have a scent of victory - the following long quote demonstrates:

> 20-02-1897: It has been blowing a whole gale for the last ten days. We went out on Friday morning in the midst of it. .... We got drenched and had to turn for home on account of a blowing joint and losing our fresh water, and also to get back in time for the meeting at 2.
> We talked matters in general, and the impression now is not to build hulls at all but to sub-contract for them and fit them up. We could also approach Ewing (professor of engineering at Cambridge), who made the best report we have had on the turbine, to ask him to test the

> 'Turbinia' in conjunction with Professor Weighton, professor of engineering at the College in Newcastle, the latter to do the detail of the trials. We shall be out again on Wednesday, weather permitting, and hope then to get some further results of a favourable kind. (A:132-133)

This also shows something of Parsons's commercial plans, as well as the desirability of unimpeachable technical publicity.

A final stirring letter sets the seal on their success:

> 07-03-1897 We went out in the 'Turbinia' on Thursday, but it was blowing hard with sleet. The wind had shifted from W. to S.S.W., so it was too rough outside. We got a run in the river .... We estimate that we reached 31 knots one part of the run. Today we luckily got a smooth sea with a long swell .... Two runs on the mile North and South gave a mean 28.12 knots .... Prof. Ewing of Cambridge and Prof. Weighton of the Newcastle College are willing to come and make a joint report. .... I hope you will excuse the scrawl. Fingers are cold and raw with runs. (A:133-135)

It only remained for 'Turbinia' to make a public appearance.

Parsons recalled the occasion of this in the lecture quoted above:

> The 'Turbinia' steamed from the Tyne to Harwich at the average speed of 12 knots, and from Harwich to Cowes at the average speed of 16 knots. There she was run at speeds up to 34½ knots, estimated from the curve of steam pressure and speed
> (Quoted in Appleyard 1933:182, also Parsons 1897)

THE SPITHEAD REVIEW:

The 'Turbinia' steamed to the Solent for Queen Victoria's Diamond Jubilee Naval Review at Spithead on 26th June 1897. Parsons's laconic words above belie the legend that has grown up around this event - an undoubted 'publicity coup'. At the lecture Parsons was thanked by J. Fortescue Fleming M.P., who said:

> I do not think that anyone who was present at the Naval Review in June last, and who saw the 'Turbinia', as I did, ... could doubt for a moment that something entirely novel, both in speed and in manoeuvering ability had been disclosed to the world. (Parsons 1897:26)

Appleyard (1933:104) fuels the legend by including the colourful recollections of Parsons's friend and colleague Gerald Stoney, who was on board the 'Turbinia':

> We made a bolt and just scraped past the bows of the French yacht .... I remember well the flow of language from the skippers, French and English.
> (quoted in Appleyard 1933:104)

It all makes a very engaging story. Even Manning, biographer of White - then Director of Naval Construction (DNC) - records the event (Manning 1923:374). Sims (1976) provides an account by no less than T.E. Lawrence:

> 'I was once taken by my parents to see a Review of the Fleet at Spithead ....
>
> 'the Queen was due to make an inspection of her fleet .... About half an hour before the time, a certain commotion was observed, and the Senior-Officer-in-charge-of-keeping-the-course-clear .... was horrified to see, making straight for the Fleet from the shadow of a nearby headland, a small, evil-looking, squat, ugly little steamer' (Lawrence, reported by Sims 1976:40-41)

And so on - beyond its self acknowledged invention, this strange confection, half way between 'Alice-in-Wonderland' and Gilbert and Sullivan, is filled with inaccuracy - the Queen was not there - she sent the Prince of Wales[4], 'Turbinia' was neither squat nor ugly, and so on[5]. The point of dwelling on

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[4] Dictionary of National Biography, Vol.XXII Supplement, Oxford: Oxford University Press, 1917:Queen Victoria.

[5] Sims (1976:41) adds a footnote: "Lawrence was taken to see the Review by his parents, but how much he saw at the time is uncertain, since his mother wrote later that instead of looking at the Fleet he was found curled up in the saloon reading Macaulay's 'History of England'".

these accounts is to show how popular myths are made, and how little they are factually based.

Parsons's part in Spithead remains unclear - he appears to be the originator, he clearly knew the value of publicity, and of the popular persona Spithead must have lent him. Indeed, the presence of a "cinematograph photographer on the 'Turbinia' at Spithead", reported by Clarke (1984:72), confirms this. However, Parsons's letters and the other evidence do not seem to provide a totally convincing need for such publicity.

Manning (1923:375) suggests that propaganda was needed: "the official mind is slow to move, and a public demonstration was the most fertile means of propaganda". But he also points out that the Navy, and White, were very well acquainted with 'Turbinia' before Spithead; indeed, he earlier quotes White's record in his diary, as early as 6-2-1894, of his inspection of "'Parsons' new boat with turbine motors'" (Manning 1923:338). Parsons's own letters record this acquaintance in much greater detail:

> 12-1896: In correspondence with W.H. White with the object of endeavouring to get their 2/3 full speed of revolutions astern modified. (A:127)
>
> 05-01-1897: We are testing some reversing methods - one suggested by Swinton looks very promising. .... I have sent a long very buttery letter to W.H. White (which will satisfy Swinton as to its tone). (A:130)
>
> 05-02-1897: W.H. White is coming on Monday to look over her. I think there is business coming in this quarter. (A:130)
>
> 16-02-1897: Sir W.H. White turned up unexpectedly last week and spent all one morning till 1.30 looking at the

> machinery and the boat on the slip. He examined everything closely and expressed himself pleased (A:131)

Parsons was consulting White, even being 'buttery' to him; they were actively involved in answering the Naval requirements for high speeds astern. Most important, Parsons saw 'business coming in this quarter'. Clarke adds:

> It is still suggested that Parsons without authority took his little craft to the Spithead naval review of 1897 and that the Admiralty was slow to respond to the new prime mover. An examination of the publicly available evidence can hardly sustain the second point (Clarke 1984:62-64)

If Parsons, alone, chose to go to Spithead, it must have been a fine judgement whether such a cheeky exercise would please or offend his prospective customers. Brown takes up this point:

> Both Durston and White were impressed by their visits to her. It seems likely that it was Durston who suggested and obtained permission for 'Turbinia's' famous visit to the Jubilee Review of 1897.
> (Brown 1983:75, also quoted by Clarke)

His suggestion fits the evidence well; Vice Admiral Durston, the Navy's Engineer-in-Chief, may well have needed such propaganda. Fisher's letters certainly paint a picture of intrigue in the Admiralty, into which such propaganda would fit well - possibly to obtain internal acceptance of turbine propulsion.

This does raise one last small question - Fisher was still Controller to the Navy in June 1897, White and Durston were his right-hand-men, he was a great advocate of <u>speed!</u> - was he involved? His silence is curious, but perhaps the curiosity lies in his own pernicious legacy - the researcher, habituated to his intrigues, seeks them even where none exist.

TORPEDO-BOAT DESTROYERS:

Fisher, from everything he wrote, emerges as an impassioned supporter of the Navy, his support of innovation and innovators was secondary to this. The Navy needed fast destroyers; Parsons saw that 'fast speed is the rage' in deciding to build 'Turbinia'; so the marriage of the two was an obvious one. Fisher no doubt agreed, he was intimately involved in the turbine's general adoption a few years later, he often cited it as a shining example of the way to proceed:

> 08-08-1910: I have helped in lots of 'shoves'! The water tube boiler - the turbine - the 'Dreadnought' - scrapping - Navigation School - feeding the sailors - etc., etc., etc. (MII:336)

But at the time Fisher was more circumspect:

> 27-04-1900: Importunity and strong language has given me 8 more destroyers, but as sure as fate, if more destroyers are not obtained .... we shall have the Boer War played over again at sea! .... They are waiting for the development of the turbine, but half a loaf is better than no bread, and while straining at the 'gnat of perfection,' the 'camel of unreadiness' is swallowed. (MI:156)

After Spithead both the Admiralty and Parsons had acted. As early as 1894 Parsons had foreseen going "to a big company" (A:97-98), and in 1897 'The Parsons Marine Steam Turbine Company' was formed with a capital of £500,000 (£240,000 issued). The Admiralty ordered a turbine powered destroyer, 'Viper', and Armstrongs built another, 'Cobra'. Parsons engined both, and saw more business here, he kept his brother informed:

> 21-11-1899 The 'Viper' had her first preliminary trial today and reached 32 knots. .... The inspectors seemed

> greatly pleased and said they considered the performance most satisfactory in every way .... I have no doubt, after a few trials, the Government will order some boats. (A:141-142)
>
> 22-06-1900: The 'Cobra had a semi-official trial to-day. The mean speed for six consecutive runs was 34.89 knots .... The Admiralty and Armstrong's officials expressed themselves very pleased and reported the trial as entirely satisfactory. I suppose she is now, therefore, as good as sold to the Admiralty. .... I think the trial of the 'Cobra' is the most important event for the marine Company since the trials of the 'Turbinia'. (A:142-143)
>
> 27-08-1900: The 'Viper' has thus two more, and the 'Cobra' one more trial before being handed over. .... It looks as if Wallsend would become busy this autumn and begin to earn good dividends. (A:144)

However, all these plans received a severe setback in 1901, when both ships were lost. Parsons gave evidence at the 'Cobra' Court-Martial; in November the Admiralty established 'the Committee on Torpedo-Boat Destroyers' (Brown (1987 & 1988)). The turbine engines were cleared, but it is evident that it must have been a blow to Parsons - Rayleigh (in Parsons 1934:xx) says: "he was living and working at that time under conditions of great strain, financial and otherwise", which he uses to excuse Parsons lack of patience with his craftsmen. Rayleigh also noted that, in 1904: "he had come through into smooth water, but occasional remarks showed how much he had endured" (p.xv), as well as describing the earlier patent litigation as: "a time of acute financial anxiety" (p.xv).

4.2.5 THE MARINE STEAM-TURBINE - Large Ships - the 1900s.

MERCHANT SHIPS:

Parsons described what happened next in a lecture to the Institution of Naval Architects:

> 26-06-1903: Both the 'Viper' and 'Cobra' were unfortunately lost before substantial practical experience in commission had been obtained. During and after the occurrence of these events, the Turbine Company directed the attention to the important application of their system to vessels of moderate speeds .... The vessels 'King Edward', 'Queen Alexandra', 'The Queen', and the yachts 'Lorena', 'Emerald', 'Tarantula', are now in commission and have given entire satisfaction to the owners and public.
>
> The engining of larger vessels and liners is not a very long step beyond what has already been proved to be successful. (Parsons 1934:40)

The 'King Edward' resulted from a syndicate of Parsons, Denny the shipbuilder, and Williamson the ship-owner and operator. Appleyard retails the reminiscences of Sir Archibald Denny:

> We, the three of us, shared the cost, which was about £34,000 I think. She was, from the first, a great success technically and financially. (A:194)

Large ships were the next step - another lecture to the Institution of Naval Architects, in 1911, picked up the story:

> 05-07-1911: The first vessels to be fitted with turbines for Transatlantic service were ordered by the Allan Line, viz. the 'Victorian' and 'Virginian'. .... At this juncture the late Lord Inverclyde appointed a commission of experts to investigate the suitability of the turbine for two express Cunarders for the New York route. After most careful consideration of all data then available .... the Committee unanimously recommended turbines in preference to reciprocating engines for the 'Mauretania' and 'Lusitania' of 70,000 horse-power and 24½ knots sea speed. (Parsons 1934:114)

According to Appleyard (1933:190), Cunard so decided late in 1904, but in 1905 conducted comparative trial of turbine versus piston propulsion, which Parsons related to his brother:

> 23-11-1905: The builders and the Cunard Company kept things very much to themselves for reasons of their own, but we shall sooner or later get the exact figures so far as they go (A:191)

This picture of the relationship between Parsons and Cunard is a little more convincing than the picture Parsons's lecture describes. Nonetheless, turbines had clearly come of age in merchant ships.

'DREADNOUGHT':

'Dreadnought' was, for the Navy, what 'Mauretania' and 'Lusitania' were to the trans-Atlantic passenger business. Fisher's letters allow close scrutiny of how this innovative step was taken - a study in influence. Fisher knew, and influenced, many people, including the King himself. He obviously worked very closely with successive Civil Lords of the Admiralty, notably McKenna and Churchill, and with other politicians. His relationships outside of politics were also central to his influence, particularly with the journalists James Thursfield and Arnold White, and the shadowy figure of Lord Esher.

Thursfield was a leader-writer on 'the Times' (Marder 1956:64), White wrote for various papers. Extracts from a few letters will illustrate how Fisher liberally gave these men information that served his and their ends. Letters to Thursfield first:

> 08-01-1901: Lord Selborne writes me very excellent and clear-headed letters, and he seems to be bent on obtaining all we require; but I have no doubt a little 'stiffening' from outside in the shape of one of those unmistakable 'do-your-duty-or-you'll-catch-it' leading articles in 'The Times' will help him (MI:179)

> 13-07-1901: you may imagine my disgust at being trotted out in the 'Daily Mail'.
>
> However, and THIS IS ABSOLUTELY BETWEEN OURSELVES, I have received a secret letter cancelling all previous letters and substituting a fresh set of instructions in preparation for war (MI:198)

There were many other such letters.

Arnold White, notwithstanding having 'trotted out' Fisher in the 'Daily Mail', was one of his main journalistic contacts. Often exhorted by Fisher to burn letters once read, White opined to Fisher's son in 1923: "His 'burn and destroy' meant 'publish as widely as possible, but don't give me away'" (MI:355). The following give a flavour of their correspondence:

> 17-07-1900: The intense ignorance of the men at the head of affairs is what frightens and appalls me! .... Our curse is the parochial politician in Parliament and the ineptitude of our Foreign Office .... and ill-informed democracy! Can you not reach them with this splendid article you have sent me? Is not a league of preaching friars required for a new crusade! (MI:157)

> 10-02-1910: I can truthfully say I never sought the Press, but I recognized it as the one and only engine that could effect the vast revolution from shipbuilding to bread-baking - from kettles to turbines - from fossils to Nelsons that the whole scheme of October 21, 1904, with its 21 points you so admirably summarized in the 'Daily Despatch' of January 26 meant. <u>Without the Press it couldn't all have been done!</u> It may not be politic to say this, but it's true. (MII:305-306)

Fisher met Esher around 1903 when he sat with him on a committee of three set up to re-organize the War Office. Esher was a man at the centre of affairs - never aspiring to high office, he was an éminence grise behind the high politics of the day. Fisher described Esher to Thursfield thus:

> 17-12-1906: You know he is a 'man of affairs' and is more in the hidden counsels of the King and his Ministers (and '<u>His Majesty's Opposition</u>' also!) than perhaps any man living! He is my most intimate personal friend!

(MII:107-108)

The way in which Fisher sold the 'Dreadnought' idea to the Admiralty, and to public opinion, brings all the threads of his influence together. A collage of Brown's words sums up 'Dreadnought's' genesis:

> Fisher and Watts conceived their first 'all big gun' ship during the trials of 'Inflexible' in 1882. .... While C-in-C Mediterranean in 1902, Fisher became friendly with .... W H Gard, and they produced further BIG .... battleship studies. Another Fisher inspired study was by Sir Andrew Noble of Armstrongs. .... Other people's ideas had been moving in the same direction (Brown 1983:85)

In 1901 Watts was working for Armstrongs, but at the end of that year he wrote to Fisher:

> I remember quite well when I was in 'Inflexible' your promising to make me DNC when you became Controller .... I suppose it may come yet .... if you should come back there in another capacity. I trust it may be so! (Brown 1983:82)

This is a fascinating letter, and shows some prescience given that it was not until 9-2-1902 that the First Lord wrote to offer Fisher the post of Second Sea Lord (MI:222). Fisher's response is shown in a letter to the First Lord, dated 4-1-1902, where he says Watts "has enterprise and audacity in excess of White" (MI:357). Watts became Director of Naval Construction in 1902. A strong sense emerges of a community, encompassing Watts, Armstrongs, the Admiralty, and Fisher.

Fisher became Second Sea Lord on 5-6-1902, stayed for a year, and then went to Portsmouth prior to returning as First Sea Lord on 21-10-1904. While at Portsmouth his ideas about new designs of warship further developed, although he kept his council on them, as he observed to his Flag Captain:

> 'I'm not such a born idiot as to tell all those chaps at the Admiralty what I'm going to do before I go there.' (recorded by Arbuthnot, quoted by Bacon 1929,vol.I:249)

Fisher wrote much the same to Esher (MI:320). It was at this time that Fisher laid plans for his Design Committee (which first met early in 1905) to look at 'Dreadnought' and other new types of warship. As Brown points out, Fisher's 'Dreadnought' ideas predate the Committee by several years, even decades, and the idea for the Committee predates his becoming First Sea Lord, as his letters show - to the First Lord:

> 02-08-1904: Dear Lord Selborne .... About the Committee on New Designs, I am also delighted with your approval! One thing I VERY SPECIALLY <u>wish you to do</u>: Not to take any step to lay down any fresh battleships, or that will in any way bind you to do so, until you have allowed me to set before you in detail in October next why we should hold our hands in this matter! (MI:321)

To Esher he explained his calculation succinctly:

> 21-08-1904: Selborne has also agreed to my being President of a Committee to devise new types of fighting ships. I explained to him that I had got the designs out of what had to be; but it was the politic thing to have a committee of good names, and then Tommy Bowles and others will fire away at them and leave me alone! (MI:325)

Other letters clearly show he was active in soliciting influential help, even from the King and Prince of Wales (MI:324,326).

Bacon (1929,vol.I:250-251) notes Fisher's association with Gard, Gracie and "various naval officers" (including of course Bacon himself), such that:

> By the 21st October the sketch designs .... were practically complete, and it is interesting to note that these designs subsequently underwent very little alteration. (Bacon 1929,vol.I:251)

On 21st October, Trafalgar Day, Fisher became First Sea Lord. A letter to the First Lord two days before this, describing the 'seven brains' of his committee, and their designs; is a little more muted than the completeness of those designs might have seemed to warrant:

> 19-10-1904: This is the 'modus operandi' I suggest to you. If these proposals in their rough outline commend themselves to you and our colleagues on the Board, then let me have these seven, assisted by Mr. Boar (who is a mole in the Accountant-General's Department - you know of him only by upheavals of facts and figures!), and secretly these eight will get out a detailed statement supported by facts and figures for consideration before we take a step further! (MI:330)

Fisher's desire for a financially astute representative is worthy of note.

The formal committee, including most of the same names was set up on 22-12-1904 (Bacon 1929,vol.1:257). It first met on 3rd January 1905, and Parsons gave evidence twice that January (Kemp 1960:267). Parsons must have sensed his ideas coming to some sort of fruition - Fisher's advocacy of 'Dreadnought' was successful, as was the ship herself. Much more could be added, but this large-scale success is a suitable place at which to leave the story.

4.2.6 A FEW BRIEF OBSERVATIONS ON THE USE OF HISTORY

The way in which historical material was analysed showed it to be remarkably similar to texts of interviews. However, the way in which it was used to write this section was different - the biographical sources have been used more than was expected. The story has not been told purely through the

lips, or rather pens, of Parsons and Fisher; the question is, does this matter?.

This section can fairly claim to have provided the complete story of an innovation, but the real problem is whether the introduction of secondary material corrupts the intention of comparing cases across time. In practice, the solution to this problem emerges in the way secondary material has been recursively woven into the narrative text of the primary material. The emergent categories provided the initial sense of direction as to what should be included, but the real business of writing demanded a chronological layout of the letters used. The same events, the same people, the same questions, then all seemed to emerge as associations clumped together in time. These associations provided a tool of remarkable power with which to cross-check and assess individual items of secondary material, and this, in short, licences the use of this historical case-study in later chapters.

This section has shown that the interpretive analysis seems to have worked. The personal letters of Parsons and Fisher have allowed vicarious participation in their world just as contemporary interviews allow a researcher into unfamiliar worlds of today. The use of historical material in this thesis has demonstrated its utility as a very rich (and untapped) source of data for organizational and management research.

4.3 A Contemporary Case-Study - HOTOL

> .... only the unimaginative can believe that mankind's destiny lies on a small overstrained planet in a galaxy of plenty. If we are to leave our 'cradle' let's do it now, and let Europe lead the way and reap the rewards in the process. The meek shall inherit the Earth, the rest will inherit a Universe. (Bond 1988:15)

4.3.1 INTRODUCTION

The HOTOL concept foresees a spacecraft designed to place satellites in orbit far more economically than presently available launchers. The name HOTOL is a good place to start, it has become lodged in the popular imagination, and this public 'persona' has been very important. The changes of name encapsulate many of the twists and turns of the story that emerged from the interviews.

The acronym HOTOL stands for the unhandy phrase 'HOrizontal Take-Off and Landing', which gives some indication of its mode of launch and return to earth. The name emerged from the centre of British Aerospace's space activities at Stevenage; the concept itself emerged elsewhere. The interviews showed that naming things was important to people:

> I had said "I want an awkward name, one people wouldn't know what it is". We nearly persuaded the BBC to run a children's competition to name it (Conchie)

> [Larry Blonstein] named all the BAe satellites, like Giotto; he named HOTOL. I had mispronounced HOTOL - Peter Conchie later said this at a press conference: 'it's HOTOL, since ITN mispronounced it' - that gives you a nice feeling. (Miles)

HOTOL was not the only early name - 'Swallow' was an earlier one.

The engine design, which later fell under the Rolls-Royce designation RB545, is the most radical part of the innovation. Shortly after being patented, the Ministry of Defence (MoD) classified the engine patent. Particularly radical is the fact that, unlike older rocket engines, it breathes atmospheric air in the earlier stages of the ascent to orbit. This greatly reduces the supply of liquid oxygen that has to be carried, and allows a single-stage vehicle design, without the need for separate booster stages or secondary fuel/oxygen tanks. This has all allowed significant savings in launch costs.

This case-study chronicles the immense problems the concept has faced - not predominantly technical, but rather financial, organizational and political. Up to now the concept has not been realized in concrete form. Moreover, what BAe now call HOTOL is no longer a single-stage vehicle, taking off on its own from a runway. They now propose to hitch an initial ride on the back of a large Russian transport aircraft, so HOTOL becomes a two-stage launcher in fact.

The original engine was de-classified in 1991, but its inventor had already produced a different and improved design, unpatented to frustrate any new attempt at classification, which he originally called 'SATAN' (Swallow with Alternative Thermodynamics and Adapting Nozzles), and now calls (following some resistance to so diabolical a name) 'SABRE' (Synergetic Air-Breathing and Rocket Engine). SABRE is proposed to power a new single-stage vehicle called 'Skylon' - a direct descendent of the original HOTOL.

This section will use the name HOTOL for convenience, since it is the most persistent name for the concept. The story is told broadly chronologically, and is based upon the interviews with those involved. The conduct and analysis of these interviews was described in the last chapter, and amplified in the appendix to this chapter, they were largely carried out during 1988 and 1989, with a final one in October 1991. The appendix also includes a full list of those interviewed, and a glossary of aerospace industry acronyms and slang.

All the people were sent transcripts of their interviews, and many replies have augmented and aided interpretation of the original material. Quotations are only attributed where the person has replied and indicated they have no objection. For conciseness, these attributions are generally made by surname only; this is a scholarly convention and is in no way intended to give offence to those who have so generously helped in this case-study.

The story is told, as far as possible, through the words of the participants. It is very much a re-assembly of the fragmentary elements of their discourse, augmented with evidence a few of them gave to the House of Lords, and the odd newspaper article. Many of those involved in HOTOL certainly make use of the press, particularly the more technical periodicals - a useful example is provided by their articles in the pages of the Royal Aeronautical Society's general news

magazine - 'Aerospace' (Parkinson & Hempsell (1985), Burns (1987), Bond (1988), Gibson (1989), Conchie (1989) and Pattie (1990)).

This thesis can attempt neither a technical description of HOTOL, nor discussions of the science, economics, or indeed politics, of space transport; the articles just cited are a good introduction to these subjects (together with Postlethwaite (1989)). What this thesis does attempt, is to tease out how the people and the organizations who helped to create HOTOL did so. What follows is an interpretive ethnographic survey of a community of people - the HOTOL community.

4.3.2 THE ORIGINATORS

HOTOL started with three people - Alan Bond, Bob Parkinson and John Scott-Scott. They all belong to different organizations, although all had worked on rocket projects (such as Blue Streak) in the past. Alan Bond had worked at Rolls-Royce, he left there to work at Culham, and has now set up a small company to exploit his ideas; John Scott-Scott still works at Rolls-Royce:

> I have had a burning drive towards spaceflight since childhood. (Bond)
>
> "The whole thing ought to be blamed on Alan and Bob", both "long term and very staunch members of the British Interplanetary Society"; the main driver is getting off the planet. "Alan's metier is dreaming up exotic propulsion systems". (Scott)

Bob Parkinson went from Royal Ordnance to work at British Aerospace early in the history of HOTOL:

> It started before I joined BAe - .... I "met some of my old friends from Hawker Siddeley", from Blue Streak days - "a bit of an old boy's club". I "had a little tour of Stevenage", and came in November 1982. (Parkinson)

These three people tie the conception of the idea to a specific event:

> At Easter 1982 there was a European Space Conference in London. A CNES guy gave an Ariane 5/Hermes talk; Alan Bond and I were sitting at the back. .... "Alan Bond muttered 'there must be a better way than this'". (Parkinson)
>
> In 1982, at a British Interplanetary society meeting, a French guy said that ESA would use '50s technology for such systems. .... I "perceived what was being stated was fallacious". I saw an engine was needed (Bond)
>
> "The genesis of the idea was really a long term interest in interplanetary travel" .... Alan was looking at a bus to the stars, "Bob came up with a vehicle", ("I can't prove this at all") they cooperated. (Scott-Scott)

One further member of the British Interplanetary Society, later to play a major part in HOTOL, was Peter Conchie - he remembers taking a key rôle in setting up their series of launcher meetings:

> I said "we're going to make this the theme of our committee" - new launchers. "We're going to have a symposium once a year". .... "Then I moved to Hatfield, late '78", handed over the committee (Conchie)

The three originators clearly site their 'creation myth' for HOTOL at the 1982 meeting.

By October of 1982 Parkinson and Bond had involved Scott-Scott - they saw they would:

> "need a guy who had more than a theoretical knowledge of turbomachinery" - John Scott-Scott.

> 11-10-82 - we had an informally arranged meeting. .... John Scott-Scott tried to find some money from RR. (Bond)

John Scott-Scott takes up the story:

> "An awful lot of HOTOL work has been done at home". .... We had a "discussion in Alan's office (where we shouldn't have done it, but I've lots of reasons to go to Culham)" - "all the old Armstrong Siddeley rocket team". .... "Alan looked at what was in it" ..... I am good at working through it to "see if there's any mileage in it. .... He brought to my house .... a blue book .... (he's very good at tidying things up); .... when I looked into it, ... as an engineer it took me a month to understand the thermodynamics behind it". "What he has done ..... would allow British industry to take the same step forward as Whittle", but Alan's ideas were much more complicated. "I telephoned - .... 'this is a goer'". The question was "could you turn it into anything workable?" I "Talked to our then head of engineering, ... wanted something very small, ... eight or ten man weeks." (Scott-Scott)

So the idea entered the Ansty bit of Rolls-Royce, informally.

Bob Parkinson introduced the idea to British Aerospace

Stevenage via the newly returned Peter Conchie:

> in April 1983 Peter Conchie came as Director of Business Development, I already knew him. .... we "leapt upon him" and showed him - he liked it - he said money for a study may be possible. (Parkinson)
>
> I went out to Hatfield in 1978, and came back in 1983. .... Alan Bond had his idea sometime in this interval, "it was moving forward at that chatting in pubs pace". .... We called it Swallow. (Conchie)
>
> 3-5-'83 - the first Stevenage meeting - John Scott-Scott had levered some local money out of RR - this was very important .... It allowed the calculations done in my lounge to be tested. (Bond)

The studies started to move out, highly informally, into other company areas - to Rolls-Royce Bristol, and to Clive Leyman at the neighbouring British Aerospace Filton:

> "The people didn't exist at Ansty .... was given a wandering brief to look around the company, ... found another old diehard at Bristol .... - 'you seem to believe in it, I suppose I'd better help you'". So with

> director's support I made contacts - "'don't worry about the bookings'". (Scott-Scott)
>
> We "acquired Bristol [help] almost by accident because of the costing thing" - "someone at Bristol wanted to know what a rocket engine cost". "I had no idea of the aerodynamic aspects", I needed someone who did, so I went there and "discovered there were people in there who know about supersonic aircraft who were quite willing to accept these strange ideas". (Parkinson)
>
> "If I'm horribly blunt about it .... [initially] it was the experience and personality of two people .... [It] was really done by a guy called Frank Crowfoot, ... and myself. .... We were able to throw ideas around." (Leyman)

This small group of people produced the initial HOTOL design,

as Scott-Scott and Conchie describe:

> The "airframe was examined by Filton - created some of the best synergies, ... because they were all a bit daft. ..... Frank Crowfoot - a nutter - he made the first model of HOTOL", "he made a little man, said 'don't forget this is a big machine'", it is to his "eternal credit". (Scott-Scott)
>
> We had "a meeting in this office", "me at the end, Bob, Alan, John Scott-Scott, Clive Leyman". .... "I asked John Scott-Scott 'will it work?'". In his bruff Derbyshire accent, "John Scott .... said 'I don't think it has much chance of working'". But then he came back and said "'much to my amazement, I think it will bloody well work'". (Conchie)

The story continued through 1983:

> We went to the DTI and "after some considerable time, liberated forty thousand pounds" to study launch systems (Parkinson)

In British Aerospace, HOTOL was presented at main board level, in Rolls-Royce it was not. Then, in December 1983 two things happened that were to be of critical importance; Alan Bond patented his engine (which MoD promptly classified), and the journalistic interest of the Independent Television News (ITN) Head of Science, Frank Miles, was awakened:

> I worked on it by getting up in the early hours, in holidays etc.. In December 1983, with the help of the

Rolls-Royce patent office I registered a patent - a "Christmas present". Then the "silly" classification happened. (Bond)

The first I ever heard about HOTOL was at a Royal Society meeting. .... Someone .... came up and mentioned a British space launcher
[I told the contact I was sorry but I had something else I was working on .... could I come back to him later. This I did - at the end of January or early February 1984.] (Miles)

4.3.3 1984 - HOTOL BECOMES PUBLIC

In the months up to August 1984 Frank Miles found out more about HOTOL, the support of Admiral Sir Raymond Lygo, British Aerospace Chief Executive, was gained, and the companies approached Government. This set the scene for many later developments.

Frank Miles recalled this time:

I got back to the man who had mentioned the space launcher .... I had a group of informants - they came to my office, we sat and talked and drank cans of beer from my fridge; I remember it got quite dark. ....
[Contacts, not BAe people, came to see me ....]
Later I went to Stevenage .... as journalists do, I read a memo upside down on someone's desk which seemed to confirm it. (Miles)

The BAe Chief Executive became involved:

"Peter had to come clean with his bosses", he "had to go to corporate level". "Just as I found a friend in Peter, Peter found a friend in the Admiral" "If we ignore it our children won't forgive us"; the Admiral supported it. (Parkinson)

"we had to go through the process - I had to let my boss know about it, ... no one at corporate knew about it" yet. .... It would not have got beyond 1985 without Lygo. (Conchie)

Many interviews noted Lygo's support for HOTOL. His Deputy Chief Executive said:

> "I think Ray Lygo fell in love with the project, ... it had an emotional appeal to him". "A project needs a champion". But on a cold blooded basis people would not have done it, and this may have been the right thing, he had doubts later too. (Yates)

Early in 1984 a presentation was made to the DTI by British Aerospace Stevenage and Filton, together with Rolls-Royce. DTI wanted the technical opinion of the Farnborough Royal Aircraft Establishment[6] on the vehicle, and of the nearby Pyestock National Gas Turbine Establishment on the engine:

> "It wasn't respectable in the early days, ... had to have the blessing of Pyestock .... - very old friends. .... It stopped them dead, ... floored them completely" ..... "You've got to work this up to a fairly high level before the officialdom of the world allow it to become respectable". .... usually "Pyestock just figure high. .... For once they couldn't". It was similar with BAe and Farnborough - "how could this have grown up with two blokes at home?". It "generated a lot of resentment". (Scott-Scott)

The DTI were convinced, and provided some funds:

> "It was to equip ourselves with answers to get the strategy right", not entry into launchers. .... "The first step was to run the rule over it by Farnborough - we came to the conclusion, 'why not'".

In August 1984 Rolls-Royce made their own presentation to MoD:

> I contacted D.G.Eng., and while not immediately seeing it as a candidate programme for MoD support his advice was to acquaint the MoD Chief Scientist with the work being done and to offer a briefing. Consequently a letter was sent to Professor Norman in August 1984

They showed polite interest:

> HOTOL "offered us a launch platform" .... there was a briefing meeting .... It would be "out of the question to

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[6] Since re-named - twice; see glossary.

> enter R&D costs", "unless DTI .... were going to put up most of the money .... it was a non-starter". (Norman)

August 1984 was a momentous month, it excited a great amount of extremely entertaining comment in the interviews, the researcher's happy problem is condensing it. August was the month before the biennial Farnborough Air Show - an event the elders of the industry sometimes use as an embryonic project's 'rite-de-passage' between secret adolescence and public maturity:

> "I was concerned in getting space a higher profile in the UK" .... "It was obvious we would have to release this at Farnborough '84" (Conchie)

The true shaman at HOTOL's 'rite-de-passage' was Frank Miles; he continues the story - BAe Stevenage, the 9th August:

> So, after lunch, all sitting together, I asked them 'what about HOTOL?' - I pronounced it (as in hotel, not hot) that way - their faces all went blank - a clear reaction - they refused to say anything about it. Larry Blonstein was there - the chief salesman .... he died about eighteen months later .... he showed me down to my car; just as I was about to drive away, he tapped on the window, and then said 'I bet you don't know this, it's got two fucking great fins' - he thus confirmed it existed - he obviously wanted me to use it - I would not say this, except he is dead now, and he deserves the recognition. (Miles)

Miles deduced further confirmatory evidence from conversation with David Scott (an Apollo 15 astronaut involved with BAe) which satisfied David Nicholas - ITN's Chief Editor:

> After that the HOTOL story all really happened in the last two weeks before we ran it. .... It all had to be secret - David Nicholas said that even though the engine was covered by the classified patent, the airframe was not, and it was clearly in the public interest to publicize a new British first - he is very patriotic. He said he could ignore a 'D' Notice, but if Rolls-Royce or British Aerospace heard, they could get an injunction within an hour, and he could not ignore that. So it was kept secret. I made a lot of the phone calls from home -

> all ITN calls are logged - I had to protect my sources. The two guys doing the graphics were .... sworn to secrecy - they did a superb job - worked all night .... they did a computer simulation, new in those days, and a picture. I told them they had to design a space plane. I knew it had two fins, ... - I was told to look at the film '2001', but ITN could not get a copy, just a bad still from it. Bob Parkinson later told me we had produced yet another of the many preliminary designs for HOTOL! I prepared a script for David Chater to present. (Miles)

On the 23rd of August the ITN News-at-Ten began: "Good evening. Secret plans have been drawn up for Britain to build a revolutionary spaceplane .... It would be the most advanced spacecraft ever constructed ....". Frank Miles again:

> We went on the air. I remember sitting in the control room - I felt dreadful - I worried if any of the people I had spoken to, and any of my informants, would get hurt - you run it because it is in the public interest, but you worry. After it went out I got a call from David Nicholas to go up and see him - I thought Oh No, but he actually hugged me - the greatest scoop ITN had had. (Miles)

The men behind HOTOL told of their reactions, and Rolls-Royce's:

> ITN blew it. No-one at Rolls-Royce knew -"John Scott-Scott was kicked all over the universe". Tombs made some very silly statements .... There was a serious RR furore - "only the public embarrassment kept them in the show", they "viewed it as a pain in the backside", they still do. (Bond)
>
> I was at Little Rissington, "sunset, ... the phone rang, ... 'you'd better watch the ten o'clock news'. .... You couldn't believe it .... - everything we'd been talking about for weeks, ... blew the lot; .... that was the end of my holiday." "Next morning that phone went wild. .... The chairman said 'what the hell is all this about?'..... My only friendly director ..... summoned to Buck Gate". "Although I'd fixed it with the head of engineering at Ansty, ... it was all done without any bookings, nothing had ever shown. .... The Chairman said we're not working on this; I became the Chairman's number one hate for weeks." Still, I get sacked every 18 months or so. (Scott)

The comments of others in Rolls-Royce amplified this:

> The structured approach to the study was to a degree pre-empted by the extraordinary Frank Miles television programme .... Clearly there had been a major leakage of information from BAe, and some questions had been asked of Scott-Scott, but no specific details of the propulsion system were exposed. The next day I was attending the Company pre Farnborough press conference, and the Chairman was not prepared to respond to questions on a project of which he was unaware

Peter Conchie at British Aerospace was more sanguine:

> "Unfortunately ITN jumped the gun. .... Frank Miles" .... had an embargoed press release, but he also had an informal source tell him about it, so he felt it was legitimate to use the story early. John Scott-Scott had more problems communicating it up the Rolls-Royce Hierarchy, Francis Tombs went to a press conference knowing nothing about HOTOL. "Francis Tombs has never really forgiven HOTOL for putting him in that embarrassing position". (Conchie)

Frank Miles hotly denies Peter Conchie's interpretation here -

BAe people were not his sources:

> Peter Conchie has claimed that the information I had on HOTOL was embargoed - I would like to make it very plain that it was not - British Aerospace had not given us any information on HOTOL. (Miles)

HOTOL went on to generate considerable interest at the Farnborough Air Show in September 1984. In that month, Sir Geoffrey Pattie became Minister of State for Trade and Industry:

> A Farnborough model in September 1984 "started to give us the shape". .... "HOTOL eclipses everything else" .... It was "the only thing that generated any attention". The "second thing was the advent of Geoffrey Pattie within DTI". .... "Pattie was taking a positive attitude to space". (Parkinson)

Parkinson goes on to describe an Autumn 1984 BAe Stevenage briefing, on their other space programs, to a responsive Pattie, who was preparing for the January 1985 European Space

Agency Ministerial meeting in Rome. At ITN, Frank Miles's interest continued:

> I got to know Geoffrey Pattie too. He gave me and three other journalists a briefing before the European Community Ministerial meeting - he talked about Columbus and Space Station, but never mentioned HOTOL - I said 'what about HOTOL?', and went on to say that if he did not raise it I would be sure to interview him for the News-at-Ten when he got back, and ask him why not? (Miles)

4.3.4 HOTOL - AS A GOVERNMENT ASSISTED COMPANY PROJECT 1985-1987

A further BAe meeting to brief Pattie on HOTOL seems to mark, in people's minds, its transition from an informal, extra-organizational, activity to a regular company project:

> In January 1985 Pattie asked for a platform/HOTOL brief - it was planned for one hour, but lasted three. "I was the least of the people present" .... "Pattie said 'I will fight for our corner at Rome'", the "Admiral was present, he turned to Pattie and said 'this is important .... worth a million of my money, how much is it to you?'"; "that is literally how it was done". (Parkinson)
>
> There was a meeting with Pattie. Lygo and Pattie each said they would put in a million pounds. (Conchie)
>
> There was a "promise of British Aerospace money, ... the reputed statement of [Sir Raymond] Lygo: 'if you put in a million quid, ... I'll put in a million quid'." (Leyman)
>
> "I recall .... in my DTI time going to BAe [for a presentation on HOTOL]..... that must have been in '85 ..... I have a lot of contact with Ray Lygo"; they were "doing their own funding". (Pattie)

If the British Interplanetary Society meeting provides a creation myth for the informal project, the meeting of Lygo and Pattie provides one for the formal project. Engineers

seem to parse their time with meetings. A 'proof-of-concept study' was proposed, however:

> for the "start of proof-of-concept study, it is not as simple as that, the Admiral doesn't have any money, neither does Pattie", it is all in budgets, it took nine months. (Parkinson)

Which neatly encapsulates the different discourse that the supporters of HOTOL now had to adopt - the technical concept had to be organizationally proven.

During 1985 HOTOL continued to attract publicity - ITN presented another news item on 23rd May; the Paris Air Show took place in July. BAe, after initial coldness, cooperated in Frank Miles second news item; this suggested HOTOL could "carry up to sixty passengers half way round the world in just 45 minutes", reiterating the statement in the first, of 45 minutes to Australia. These claims seem to have entered the public imagination, but as Frank Miles explained, their origin lay simply in the chance way chosen to illustrate HOTOL's speed, not in BAe policy (chapter six will take up this story).

The interest at Paris raised the question of the classified patent again:

> I had been to the Paris Air Show and was told by CNES and Aerospatiale that they <u>had</u> been interested in possible joining with the UK in developing Hotol commercially but that, because Britain refused to de-classify the engine, they could not evaluate it and so broke off negotiations. When I came back from Paris I contacted MoD about this (Miles - subsequent comment)

The issues surrounding the classified patent are shrouded in secrecy, and difficult to interpret. The implication is that

its effects were negative. It was not de-classified until Apri1 1991.

In Summer 1985 the Stevenage proof-of-concept proposal was considered:

> this "very interestingly went two courses [later comment: i.e. Warton and RAE independently reviewed my proposal]; the company decided it would go to Warton" .... On the Ministry side, "the basic success was that Farnborough, when they saw it, thought it was something we as a nation had to do". (Conchie)

The interviews discussed establishment of the Warton project team - Parkinson summarizes:

> "To get DTI money" a proof-of-concept study proposal was prepared, "which we did at Stevenage, even though we knew we were losing management". Warton made comments. .... Stevenage's role was "understanding the need for such a vehicle", we "retained responsibility for the business development, ... the costing, ... the operational definition", "all areas I would have expected the project manager to be concerned with"; we also covered the space technology aspects. "The nett result was .... something like 25% of the job was done at Stevenage .... 10% at Filton .... the remainder was done at Warton". (Parkinson)

By 1985 Rolls-Royce had formalised their activities, although they never set up a dedicated project team:

> The proposal for the study programme was submitted to D.G. Eng., following normal practice, for consideration by MoD or DTI. It was included for information in the corresponding BAe submission, and noted that Rolls Royce has not allocated Company funds in the forward programme and would expect our part of any HOTOL feasibility programme to be fully funded.

The Rolls-Royce insistence on full, and separate, funding is significant, as will appear later.

The renaissance of British Government interest in space led to establishment of the British National Space Centre

(BNSC), with the highly respected figure of Roy Gibson as Director-General:

> HOTOL "came in the very first day I was appointed". Rolls-Royce and BAe "wanted to give a briefing to the Minister"; "when I looked in the till .... it seemed to be the sort of thing we should fund .... - 50/50 - .... Geoffrey Pattie was very keen on it". (Gibson)

BNSC funded it:

> "It wasn't a case of throwing much money at it .... proof-of-concept chosen" .... "led to the requirement for co-funding". "BAe clearly got more from it", "Rolls-Royce could not get the same driver".

The two year proof-of-concept study, started in BAe in October 1985, was now funded. HOTOL became a more orthodox, though still radical, BAe company project:

> One of the problems of moving was having "people who had not been there from the beginning, ... who had not been through the positive phase of the programme". .... It takes "about three months to get a new person indoctrinated into the system". (Parkinson)
>
> "The traditional engineers come to meetings with books", try to write it all down; I remember one from Warton in one of the early meetings, the "poor guy got lost" - Bob Parkinson would say one thing, Alan Bond would say 'yes, but', John Scott-Scott would throw his ideas in - "they talked a different language, ... ideas could be bounced off with those four". The "Warton guys took a long time to get up to speed; .... now they are different from the other guys there". (Conchie)

Other BAe interviews tended to confirm these observations:

> You "have to clearly see actions at meetings - need your own notes", "what you'd like to do is different from what the chairman wants". "I go into all these other offices .... - I've got to do all the basic programmes - we've identified maybe fifty-five different problems", "some of the queer things we've never had to do with aircraft" before.

In Rolls-Royce HOTOL was just one concern among many:

> Brian Lowrie was appointed to lead all work in the Company on high speed propulsion.

> "I picked it up in a fragmentary fashion. .... It came into the company by a completely unorthodox route"

Alan Bond summed up the differences between BAe and Rolls-Royce:

> BAe management is straightforward; Lygo supports HOTOL. .... They are "training a British team again" for space, they have learned a tremendous lot. .... Rolls-Royce is quite different, there is no emphasis, everyone is part time. (Bond)

The issue of the patent rumbled on; in April 1986 Rolls-Royce agreed a two-year option to buy it from Alan Bond. Its classification was implicitly mentioned in comments in May, when Pattie toured Europe: ""giving short unclassified presentations to our ESA partners"" (Pattie). Gibson adds: ""We gave them a confidential briefing on HOTOL"". Conchie mentioned its effect in a newspaper report nine months later:

> Mr. Conchie said that, under special procedures authorised by the UK Government to try to win European support for Hotol, about 20 people in the rest of Europe .... have been told details about the engine. (Financial Times 23-2-1987)

With 'proof-of-concept' work due to end in September 1987, attempting international collaboration clearly made sense for BAe. Whether these attempts foundered because of the classified patent is very unclear, but, despite Pattie's belief in HOTOL, the Government's continuing unwillingness to de-classify did presage its future withdrawal of support. Nor was support in industry universal:

> There was ill feeling between BAe and Rolls-Royce, it was the "start of the rot" .... The Ministry asked for the next two year proposals - BAe matched the government, the Ministry said OK - RR put in nothing, they were asked at a high level. The "Ministry mandate is 50%, and we renegued on it; .... an edict from the Chairman, the wretched Sir Francis." "A superb dichotomy - .... one

side showing goodwill, ... engine man got sourer and sourer". (Scott-Scott)

As Pattie observed in his interview:

"There are other problems .... politics isn't to do with Whitehall, ... it's just a Punch and Judy show here - the real politics is in every organization in the land" (Pattie)

Bob Parkinson introduces the next stage in HOTOL's progress:

We come to "now define the decay of the programme". .... "Pattie lost his job - probably not because of his space interest, ... but it didn't help", and "this Clarke fellah appeared, who then proceeded to reverse everything". Saying 'no' to space appeared to suit Clarke's career. (Parkinson)

4.3.5 "THE DECAY OF THE PROGRAMME"

1987: General Election; then Mrs. Thatcher said no more money for space, BNSC folded, Pattie went - it all "crashed in the space of a few days". The "DTI ratted out" on BAe and RR funding, it had been supposed to last for years. (Bond)

This was in July of 1987; Kenneth Clarke replaced Sir Geoffrey Pattie, the UK space budget was capped - as Roy Gibson observed:

"So it came in straight from BAe, ... we ran with it, ... then the Minister changed - this shows the fragility of it". (Gibson)

In August Gibson resigned; in September the 'Proof-of-Concept' study ended:

It finished in October 1987, by which time we "had already spent very considerably more than the one million, ... by a factor of 100%, ... just BAe". (Parkinson)

In Rolls-Royce as well

We "have not proved proof-of-concept. .... Stopped last summer". "The way ahead is extraordinarily clear; ....

> the carrying through of that message beyond the immediate team has not yet happened". .... "we were pretty naïve".

A meeting in October seems etched in people's memories. The surrounding events show something of the centrifugal forces that can tear a new idea apart, as well as providing a rather sad swan song for Frank Miles - the 9th October, 1987:

> It was a month before leaving ITN that Kenneth Clarke gave the interview saying 'No more money for the European Space Agency .... I urged immediate use, that night, (a FRIDAY night) .... In the end ITN decided to run it on Sunday night, the eve of the Brighton conference. It caused a furore there. Clarke cancelled and sent John Butcher. (Miles)

Press reports show the impact upon those interested in HOTOL:

> Britain's top space scientist threatened to resign last night ....Mr Alan Bond, the creator of Hotol, said: 'If there is no more government funding, and if industry is unable to support the project, I will take my expertise elsewhere.' .... He said he was determined to ensure that the Hotol project went ahead even if it meant his risking imprisonment for breaching the Official Secrets Act. 'I have been in prison for the past five years with this project', he told ITN news last night. .... I am not going to stand by and watch the Government sit on something which is going to introduce a modern industrial revolution comparable to that of 150 years ago' ('The Times' 13-10-1987)
>
> Peter Conchie .... talks about the project the way a long-serving prisoner talks about his sentence: 'The only way to cope is to look forward three months at a time. If we've got money for the next three months, we're okay.' ('The Independent' 14-10-1987 p.19)

The depth of feeling emerged very strongly in the interviews:

> I made some bitter statements, said I would go abroad if the UK did not follow it up. People were upset by my television appearances. Questions of official secrecy were raised, I said I would break the act to further spaceflight. (Bond)

Bond's words are in stark contrast with Clarke's airy espousal of rationality in his evidence to the House of Lords on 17th

November 1987 - the two men seem to speak different languages:

> I know that the chap who invented the propulsion system is getting very impatient, He is starting to say that he is going to take it abroad if it is not developed here. There are at the moment difficulties which I would not want to labour myself too much here, but it is classified at the moment because of its strategic significance, and so I hope he will not do anything foolish like going off and breaking the law while it remains classified! Obviously if he has a really good idea we all desire it should be kept here, and exploited here, but this Committee, more than most, will have come across really enthusiastic inventors .... I am afraid, as Ministers, officials, and even as Parliamentarians, we are lumbered with the boring task of finding out how we finance their enthusiasms, and how you appraise them at each stage in order to make sure they are going to work. (Clarke in Lords 1988:Para.725)

The interviews show that Clarke's predecessors at BNSC and the DTI spoke a language more similar to that of Alan Bond:

> "Kenneth Clarke pee-ed on the chips in a big way .... by swinging his broadsword around and effectively cutting the Europeans into little pieces. This, I suggested to him, was not the way. .... As a result the ESA door is closed" .... "The government attitude has been the worst" you could have. "He wrought havoc, not only in Europe, but also here. .... 'No more Government money, now is the time for private involvement, but we are not going to de-classify the engine!' .... Absolute nonsense, ... a complete disaster." (Gibson)
>
> it "depends on who the minister is - if he's someone who's a bit nutty about it like me" - yes; "if it's Ken Clarke" who wants to be Health Minister - no. (Pattie)

Clarke 'swang his broadsword' at the November 1987 ESA ministerial meeting. In December Rolls-Royce stopped test work for HOTOL, in April 1988 they exercised their option to buy Bond's patent:

> "in April '88 .... Rolls-Royce and myself finished our association". They purchased the patent and my rights. "What I did then was warn everybody in sight", Farnborough, Pyestock, BNSC, British Aerospace - I phoned everybody and warned them that "Rolls-Royce were getting the patent in order to do nothing with it". (Bond)

On 25th July 1988 Clarke finally announced that funding of HOTOL would be stopped. This provoked comment in the press, the House of Commons, and the interviews. Sir Geoffrey Pattie summed it up:

> It went on from 1986 to 1988, then came the "announcements in the not too distant past .... in July, by Ken Clarke - .... his last fling before leaving DTI was to can the project .... - in effect no programme". The "next stage was going to be a £22million stage" .... "What is really rather pathetic .... is the fact that Ken Clarke had to say what he did. .... It implied that it was already proven", since we were pulling out the funds - we had to throw it away for £6million. (Pattie)

Views varied as to the future. In British Aerospace they might be described as resigned patience:

> "Any judgement in the short-term is likely to be wrong in the long-term". It is worth keeping HOTOL going just to drive technology. In fact I do not think Europe will let it die. (Conchie)
>
> So it is "a flickering flame"; if we "keep it long enough" then it may take off "when the fog clears over the channel". (Yates)

In Rolls-Royce attitudes were much more mixed:

> "Perhaps it's been important that the .... Chairman hasn't been in favour of it". "In simple terms, from the Rolls-Royce angle .... we don't see a profit in it for us, ... therefore the customer has to start paying from the beginning".
>
> "We had all sat round a table and said 'do we run HOTOL?' .... - it was difficult to see how our company would make any money out of it" .... "which is why our Chairman is being very negative in public". "Even if BAe lose, they still make satellites".

These commercial judgements contrasted sharply with the originators' comments:

> "My experimental programme has been completely abandoned" .... I am seeking early retirement; it has been refused, but I will try again. (Scott-Scott)

> John Scott-Scott is still at Rolls, a couple of personally committed guys are carrying it. The project would die otherwise. All the figures are very much individuals. Rolls-Royce are not good at managing individuals, they appear "affronted by John Scott-Scott", they "do not like people saying balls", they "go off in a huff", they threatened sacking him. (Bond)

Over three years later, Bond reviewed the progress of Ivan Yates "flickering flame" of HOTOL in BAe:

> Ivan Yates was one of HOTOL's best friends in the end - he was sceptical at first, but became convinced. When Raymond Lygo left he took up the torch .... Bob Parkinson was the guy who introduced it into BAe - he is the sort of guy who, if you give him a meccano set, will go away and make the absolute best optimum job out of what you give him. If you then give him half a meccano set, he will still go away and make the best he possibly can out of it - even if you just give him one piece. That is what he, with Ivan Yates, did to carry on with HOTOL. (Bond)

4.3.6 EPILOGUE

The future of HOTOL remains uncertain. British Aerospace continue with it, Rolls-Royce do not. The British Government are neither funding it, nor actively pushing it as a contribution to the European Space effort. Yet the interviews agreed that international collaboration was ultimately needed. Meanwhile, other countries continue with different, but comparable, launcher schemes. In September 1990, British Aerospace announced Russian collaboration on what is effectively a two-stage HOTOL with conventional engines.

Yet, it was always Alan Bond's engine that gave HOTOL its innovative edge, and to keep his original vision alive he has continued to look for alternative finance. Bond describes one

potential approach back in 1987:

> he is a wealthy property developer - after the 1987 débâcle with Clarke "he contacted me and was very interested in funding the project". He wanted the patents and he could not have them - he does not understand I had had to sell the rights to keep it going. .... "He was proposing [his] Group would put up the cash" for development of the engine on a partnership basis. That did not seem practical to me then (Bond)

Instead, after the April 1988 patent buy-out, Bond sought more conventional project finance:

> I went to people like Rothschilds, very naïve. .... I set up the 'HOTOL Development Corporation'. That seemed straightforward, but I was very naïve - personalities got in the way, especially Tombs. (Bond)

After the July 1988 cessation of funding, the possibilities offered by Bond's aspiring patron reappeared:

> a message was sent to BAe via [his] aides - he wanted to keep his own name quiet - would they respond to the 'HOTOL Development Corporation' idea. BAe and Rolls-Royce said yes to a meeting over dinner - BAe, Pattie, Robbins - not me, it was deemed best to focus on the commercial, not the "boy wonder aspects". (Bond)

Once again, the plans were frustrated:

> Rolls-Royce would not reply, the dinner dates got changed, and so on. [He] got uneasy .... It was an amazing thing that happened - this Rolls-Royce pussy-footing around; [He] said he could not do any more until they did; I contacted Rolls-Royce - they said in the end they had 'changed their mind' - I still cannot believe it. (Bond)

Without Rolls-Royce, the only alternative for Bond and his sponsor was a new engine design:

> he wanted to get around the patents, he offered me facilities .... Now that looked difficult to do, but in November I did make a breakthrough - it may have been the shot of adrenalin from the loss of the patents to Rolls-Royce - both [he] and I were hopping mad about that. "At that stage we managed to" get around the patent. (Bond)

The next year, 1989, two threads developed. Bond set up a technical company - 'Reaction Engines'; he also acquired a

financial intermediary company - 'Enterprise Corporate Finance'. 'Reaction Engines' produced a computer model of the new SATAN design, which BAe tried with their vehicle model, but again large company politics intervened:

> At that time Roland Smith separated the BAe plants into separate plcs, and civil war broke out - Warton throttled off Filton and Stevenage .... it meant Filton would not run the intake matching after April 1989 (Bond)

The two originators continued on their own with the new engine, now renamed SABRE:

> I had set up 'Reaction Engines Ltd.' to exploit the SABRE engine .... - John Scott-Scott and Richard Varvill too. .... John Scott-Scott is still with Rolls-Royce, he is ten years older than we are - we can afford to make errors, he cannot .... the first thing we did was look at the vehicle. A very very valuable exercise - out of that grew SKYLON - we are not in the business of designing vehicles, but you need one to work with. (Bond)

The finances were not so successful, Bond returned to the property developer who, in January 1989: "said look for other partners - he would put in one million pounds", but the property business was in decline, and by 1990 he had lost interest. At the same time:

> I engaged Richard Wilson and another guy .... corporate finance fixers. Wilson and I went ahead .... We trawled round merchant banks, venture capitalists .... What we were told - uniformly - was that the major part of the £1.7 billion was fundable by usual means, but the first research stages were not. Despite eloquent arguments - they could not fund it - "those were the rules of the game". "Some days we'd go off quite optimistic, ... but we'd still end up round the pub getting blitzed". (Bond)

The search failed the only gain was 'Enterprise Corporate Finance' itself:

> "That exercise went from April 1989 up until about February 1990". .... Wilson .... "was very very sold on what we were trying to do"; in 1990 his firm were in trouble, and his outfit was about to be sold off - we bought it .... So, most of my patent money had gone into

Enterprise Corporate Finance. (Bond)

Bond left Culham in August 1990 to work full-time for his 'Reaction Engines'. Saved from bankruptcy by various small bits of work, but still lacking serious funding for the new engine, Bond has continued to search for funds. He described his detailed financial analysis and modelling of possible schemes here:

> I realized there were lots of ways of getting money - financial engineering, and the arbitrage deal, entered my life .... The project could become a useful and valid basis for a legal arbitrage deal .... It turns out they attract a dubious set of characters .... most deals fall apart ....
>
> That was a good introduction. Another area is the foreign exchange market .... We set up a meeting with a group of wealthy investors .... no-one invested. (Bond)

Alan Bond summed up the October 1991 position of the three originators of HOTOL:

> Bob Parkinson is still committed to the stage-two concept - the full original HOTOL - the shape differs from SKYLON but the technology is the same except for the exotic materials. I have avoided commenting on HOTOL; if I am asked at lectures I just say where I know they have got to. My contract with BAe keeps them informed of SABRE developments. (Bond)

He described the current position of his own 'Reaction

Engines' - ""A hard tale of pursuing cash"":

> the "main thrust is still to get engine development under way". .... we now have good relations with ESA. .... We have offers of money from one fund. We are making the first moves to acquire a site ....
> The technical part "has been the pleasurable part of the last year". .... The great thing about the new design is that when you look at it, it seems to want to work. (Bond)

HOTOL started outside of any organization - with: "an inner circle of space enthusiasts who, as they disappeared under the table in the pub, jotted down some notes". It has come to entrain supporters and detractors within industrial, financial, and public life. Some of these continue to imbue it with the same sense of moral zeal as its originators - Sir Geoffrey Pattie, quoted at the beginning of this thesis, is one such - he continues:

> I would have thought the way technology is developing a horizontally launched reusable system is very likely to be what people will be using in the early part of the next century. Someone may then say in a small footnote, 'Yes, I think it was first actually developed in Britain' and it will all be done by Mitsubishi by then. Does it matter? I happen to think, my Lord Chairman, that it does.
>
> (Evidence of Sir Geoffrey Pattie to the House of Lords select committee, 25-11-87, para 764 - Lords (1988))

4.4 Other Cases - a Few Vignettes

4.4.1 INTRODUCTION

This section bridges technology and finance. Its four interviews provide a window on other case-studies that might have been pursued - four vignettes as it were. The first is in British Aerospace; the second looks at a brand-new company - a venture capital funded start-up. The third study considers the management buyout, on privatization, of Vosper Thornycroft (UK) Limited. The fourth interview was with a man who many of the other interviewees saw as something of a stereotype - Sir Clive Sinclair.

4.4.2 ANOTHER AEROSPACE INNOVATION

British Aerospace emerge from HOTOL as sensitive supporters of innovation. The brief study below is an antidote to any suggestion that it is the company involved that is the important factor in innovation, rather than the idea, or the people, or just chance. This study is also set in BAe.

Most of the people mentioned are not named since they were not interviewed, and therefore denied a right of reply. The few that also figured in the HOTOL study are named - their identities would have been fairly obvious, and their treatment in both studies allows a fairer treatment of them. People are referred to by approximate functional titles - strict accuracy has been sacrificed in the service of clarity. It should also be mentioned that the project involved was already known to the researcher (although he was only very peripherally involved in it). This allows the substance of a notice pinned to the man's office door, by one of his colleagues, to be recorded here - it bore the legend 'mad inventor' - he will, however, be referred to below as 'the originator'.

The originator, although closely associated with engineering testing, was not himself an engineer. He tied the beginning of his idea to a specific meeting:

> "If you go right back to the beginning, ... [the local MD] came down - .... said we're not selling the [product] - .... if anyone can suggest anything", then do.

The originator conceived a radical solution - a cut-down version of the existing product, with the addition of some new servicing equipment using technology similar to that in hydraulic cranes and fire-engine ladders. This allowed the way in which the product - a piece of military equipment - was used to be radically enhanced. He told his boss:

> "I went into [my boss].... and he said 'fucking hell, I think you've got something here'".

He also mentioned it to the Chief Designer, who seems to have been a little sceptical - perhaps not surprisingly, given the idea came from a friendly rival in a less technical department:

> "I explained it to him, he said '.... you're off your mind'" - 'you're mad' - "'leave the mad, hairbrained, schemes to us'" - "'it's not going to work!'"

Other engineers were more receptive:

> "I was working part time as a consultant .... to Dowty's". Now "Dowty's and hydraulics are synonymous"

Dowty's: ""could see nothing wrong with it. .... I thought - this is brilliant"":

> I went back to [the Chief Designer] - "'fuck off' he said". I said 'get everyone in a cinema - I'll do a presentation' .... "I went through it, half of them laughed, half saw something in it" .... [The Chief Designer] begrudgingly said .... 'look at it - patent it - it ought to be secret'.

The originator and two others did some more work. Then, still frustrated by people's scepticism, he set up a few tests of his idea:

> I borrowed the Simon Snorkel at [the local] Fire Station - I "drove up, said 'can I borrow your Simon Snorkel'" - they said 'No', then they got interested, so we did. .... - you remember that - tried it out .... [your boss] got enthused

The enthusiasm spread locally, but little further, the local main-board director:

> "took it higher up" - got a big NO - a tactical non starter they said. "So I said 'who are the people who know about tactics?'"

The answer was MoD, but approaches to them were: ""basically negative."" Other approaches were more successful:

> So I went to the Government scientists, and I found a champion there .... - I made a presentation in Paris - "so at that stage we had the scientists showing an interest, Dowty showing an interest, British Aerospace reluctantly showing an interest".

Local support continued to grow, the local main-board director:

> "got really quite keen. .... He said 'we'll talk to the Admiral'. .... He, unfortunately, was not prepared to think too sideways"; he hates hydraulics, some burst once and sprayed all over his uniform

The champion of HOTOL was not interested. The originator continued to advocate the benefits:

> "But again, nobody would believe my calculations because they were my calculations". .... I borrowed a crane for a week .... but still they would not believe us, "and then we got hit by the recession" .... Dowty spent three-quarters of a million, we did the same.

The idea has not been taken up - the last hope is a quite different civil application: ""that is still open ended"".

The interview covered one further aspect - the company claimed the proprietary rights to the idea in what the interview revealed as a rather heavy-handed way. The strength of feeling is obvious in the originator's words:

> "However the sad thing with anything like this, is you can make enemies". The 'why didn't I think of this' mentality. "The company tried to do the dirty". The company said I had to take out a patent and assign it to us - no one told me why. .... [The local MD] "insisted on

> seeing me - kept me waiting all afternoon - .... "said 'sign here, there will be five hundred quid in it for you' - I said 'you bastards! .... I'm not stupid, ... all I want is a fair deal'". [The local MD] said "'You want to go on working for us - you sign here'". I got a cheque for one thousand pounds and kept my job.

BAe was clearly not as supportive of this new idea as it had been of HOTOL. One last resonance with HOTOL underlines how people who aid one idea, harm another.

> "Pattie was the bugger that leaked the whole thing to the Sunday Times".

Or, maybe publicity could have saved the idea?

4.4.3 STARTING UP A TECHNOLOGICAL COMPANY

This sub-section looks at the creation of a new biotechnology company, funded by two of the venture capitalists in the next chapter. The founder and chief executive was interviewed, he freely admitted that he would not have succeeded without seedcorn venture capital support. He described the process:

> "What you have to do initially is get .... seedcorn money - I think that's probably the most difficult money to get" .... There [my backers] are unusual - they help you make that first full business plan that goes beyond the technology alone.

He described the need to adjust his own perceptions:

> "One of the challenges is breaking away from the 'them and us' idea - breaking away from the thought that it is your idea, your business". Do you want one hundred percent of the action, where in the end it is only worth one unit, or do you want to end up with ten percent of one hundred units? .... They have invested more than money - they risk their career and judgement on you.

The founder was candid about the way the venture capitalists monitored and controlled: "We have had run-ins, but we have talked them out", he continued:

> "That's where most of my frustrations came early on" - not enough incentive, too much control - but at the end of the day you know you can do what you think is right; you just have to satisfy them. They may want to approve capital items, employee salaries, but in the end they will forego that control as confidence grows.

Beyond seedcorn, the innovator needs:

> Big enough bank balances "to survive long enough for the products of their innovation to come through". It is often not the product they expect - it may sometimes be the fundamental process technology, not even the product it makes.

The original investor's funds may no longer be large enough here, the founder reviewed sources of new finance:

> In the US, people think of floating the company, an initial public offering (IPO) on the right market - whereas in the UK the USM is not an active market, and there are not enough individuals providing finance either. "So it's not a good place for a company to be"

The over-riding impression left by this interview is of the need for mutual respect and accommodation between financier and financed. The founder concluded by expressing his view that biotechnologists ought to get together more - he clearly sensed the value of community:

> We each have niches, we lose nothing, we can help each other with problems we have faced. It is not formal, it is something beyond that - trade associations can help, but there is no passion - they "do not feel as passionately as those in businesses".

4.4.4 A MANAGEMENT BUYOUT

Vosper Thornycroft (UK) Limited was bought by its management upon privatization of the conglomerate - British Shipbuilders. Peter Usher, Managing Director at the time of the buyout and afterwards Chairman, was interviewed. He saw the privatization as ""a very valuable fillip"", even though:

> "I knew full well that as the Managing Director I'd be the first to go, ... but that wasn't particularly worrying - .... we'd get the company going in a better way". An MBO looked very remote here, but I wrote to the Chairman and expressed an interest. .... He did not want old guys, but having said I wanted to do an MBO, they really could not sack me. .... "there was no general enthusiasm for MBOs in British Shipbuilders"

British Shipbuilders put the sale out to tender, the existing management decided to bid, but they still had to run the company at the same time - a difficult task: "my worst eighteen months".

If the parent company was less than friendly, potential financiers were much more receptive:

> "we did the conventional thing, we went to a venture capitalist. We were given a lot of good advice" .... "Full marks to County NatWest - and it was the individuals", not just the organization.

The management soon came to respect this new area:

> Most engineers do not know much about the City - it is a black art - "the people we met were charming, very fine people, they remain our friends"
>
> "They were in a teaching mode too" .... "the greatest difficulty was in reconciling attitudes" amongst people - the mixture has to gell.

The managers put up money: "half to three quarters of our houses - that would have had a pretty dramatic effect on us".

The venture capitalists acted as facilitators:

> "What they did was to open the door to people who would give a term loan. .... They gave the confidence to the people who would do that".

The venture capitalists also advised on the details of the buyout - such as whether to involve the employees or not - traditional values and perceptions were challenged:

> There was tooth-sucking among the management about how far to open the Pandora's Box of the MBO .... County NatWest advised no more than the seven - it was difficult to run it even with just seven .... a more democratic way would have sunk me. "I'm all for communication - I entered the industry" at a time when most people said 'never talk to the press, never talk to the men'; "I broke all that down", I spoke to the media on and off the record. I penetrated the union barrier too; that probably stopped resistance to it - the Trade Unions said 'better the devil you know' - you cannot ask for more - that is praise from them.

Usher's comments on the venture capitalists' conduct of the MBO gave a good insight into the way they work:

> "I'm full of admiration for them". They were not heavy handed, in fact they were reluctant to spend time on their prodigy - they do not want to run it .... "they put in much more than just management advice - they put real controls on". They were certainly on the end of the phone .... "They were like iron - .... they would go through those management accounts with a fine tooth comb" .... "their main interest was their reputation". It is a City characteristic, "they can afford to cover losses, they can't recover a tarnished reputation".

This last point is a perceptive insight into the City's way of doing business.

The buyout was a success, and exit no problem:

> "So, there's a satisfied group". It went like clockwork; "not only that, we also floated the company very quickly". We have made consistent profits ever since

The easiness of the relationship with County NatWest very clearly pleased, and surprised, Peter Usher.

4.4.5 INVENTION - THE LAST WORD

The interviews contained various stereotypes. The final interview in this section was with a man who was prominent, perhaps even pre-eminent, amongst such stereotypes - Sir Clive Sinclair. Scarcely a month after the interview described in this sub-section, another account of an interview with Sinclair, conducted in the same offices, appeared in 'The Sunday Telegraph' (29-7-1990) - it provides an antiphony to the present sub-section. The sense of déjà vu it evokes is extremely disconcerting for the researcher, but highly significant testimony to the reproducibility of interpretive research[7]. Two quotes from the article set the scene:

> over the two decades of his public life newspapers have worked hard to glamorise Clive Sinclair. Helpfully they have provided him with .... trappings of the whizz-kid tycoon they feel it his duty to be. .... He should be extraordinary, but sadly the trappings have remained largely imaginary and Sir Clive determinedly colourless.
>
> He listens carefully to questions, and answers them tersely, with a slight smirk, which can leave the questioner groping. ('The Sunday Telegraph', 29-7-1990)

In the interview, Sinclair reviewed how his own ideas had been financed:

> "From the early days it didn't arise", I did not need finance then. Lately the problem has been getting people to invest. One project follows on from another.
>
> "Historically, we've funded ourselves really". Around five to six years ago we did see some exciting projects that were too large to be self funded within Sinclair Research, so we set them up as individual companies.

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[7] It also re-emphasizes the direct comparability of some documentary sources and interviews.

Sir Clive's comments on inventors were quite revealing:

> People do find difficulty, but the ideas are risky - some of them should not be funded of course. "The impression I get is - good ideas do get backing". A large number of people have an idea, but no drive to push it all the way through. .... "No financier can do it without a champion for the project".

His comments on financing technology were muted:

> It is "very very much harder to get investment where there's technology involved". The investors have to ask someone else to assess it, and these people tend to be negative to protect themselves. "The banks .... ought to develop more imaginative routes to finance companies". Technical companies often need an equity route.
>
> Local banks could maybe act as intermediaries, sell shares locally. 'High nett worth individuals' are very important. "But I don't know how it's best done".

When asked about City involvement, Sir Clive seemed even more gloomy:

> "It seems to me .... that those people that have gone public have regretted it"; "they didn't enjoy the experience - .... there's something wrong there. .... It stems from a cultural difference - .... the City doesn't understand its customers".

Another issue raised was the differences between large and small firms:

> "I am worried about the attitude of people like 3is - .... generally they want to sell you to a big company, ... so you don't grow new big companies". .... "What I'm concerned about is we don't seem to be creating new companies as America does".

Sir Clive was forthright, even animated, in developing his criticism of large companies:

> You have got to have a competitive environment - large companies have failed to enter new markets. Electronics is dominated by GEC - they do not do it.

As to the rôle of government, Sir Clive was equivocal:

> "It may be one of these areas that may not be soluble" - you do not want monopoly, but you do not want market constraint either. [Does all this frustrate you?] No -

"I just get on with the job".

The Sunday Telegraph profile ends on a strangely similar note - Sir Clive's comments on a Yeats poem ('The years to come seemed waste of breath, / A waste of breath the years behind'):

> 'It's more a celebration. Accepting things are ultimately pointless, but doing them anyway'
> (Sinclair in 'The Sunday Telegraph - 29-7-1990)

CHAPTER 5 : THE PROVIDERS OF FINANCE - AN INTERPRETIVE SURVEY

> .... cultural types which occur in very similar forms in every society, because they are constructed around very simple polarities, are used to fulfil different social functions in different communities. .... No doubt relationships between any two societies would be made easier if, through the use of some kind of grid, it were possible to establish a pattern of equivalences between the ways in which each society uses analogous human types to perform different social functions. Instead of simply arranging meetings on a professional basis, doctors with doctors, teachers with teachers and industrialists with industrialists, we might perhaps be led to see that there are more subtle correspondences between individuals and the parts they play.
> (Claude Lévi-Strauss, Tristes Tropiques, pp 26-27)

5.1 Introduction

The investigations in the last chapter showed that one of the main problems with developing ideas into technological innovations was how to get finance. Inventors, given money, have time to develop an idea; without money any innovation recedes into an uncertain distant future. This chapter looks at finance.

The world of finance was investigated in just the same way as the story of HOTOL - with expansive informal interviews. In many ways, an ethnographic method is even more appropriate for finance than for HOTOL - finance is closer to the traditional anthropological idea of a community than the shifting and interacting communities that surround HOTOL.

The main part of this chapter tries to synthesize these multiple accounts into, it is hoped, a readable and informative description of the world of finance. However, the emphasis is still on the finance of technology - the people of the last chapter must not forgotten - it is well to remember

Cooper's (1989:490) caution that "every presence is a form of censorship, albeit a necessary one .... for its own realization". The innovator must still be the 'figure' within the broad 'ground' of finance that allows innovation, and that this chapter describes.

5.2 Historical Aspects

Understanding of HOTOL can be informed by looking at an historical project like Parsons's turbine; in a similar way, modern finance might be informed by historical finance. This chapter will therefore start with a few of the historical threads that led to the financial institutions of today.

The aim of the collage of examples that follows is most definitely not to be comprehensive, it is simply to allow limited allusions to be drawn over time, and to catch something of the sense of place that became very evident during the interviews in the City of London. Many of the tensions that excited comment in interviews have a timeless ring when set against this ground.

AN EXAMPLE FROM THE ANCIENT WORLD:

Innovations in trading patterns have been financed, since the earliest times by using other people's money. The introduction of coinage - tokens of value - allowed the abstract conception of 'money' to come into being in the ancient world. Abstract money could then be used to buy real

ships and commodities, in order to equip speculative adventures.

Such ventures go back at least to the days of the Phoenicians - great seamen, great manufacturers of goods, and above all great traders (as Harden (1962) and Herm (1975) describe). The breadth of their trade is graphically described in sources from antiquity like Ezekiel 27:12-24, which, as Lipinski (1985) discusses, was not written by Ezekiel at all, and is in fact "likely to be based on Tyrian commercial records". Commercial activity on this scale implies considerable legal, technical, and financial sophistication, and various writers describe such developments. Costouros and Stull (1989) even advance the thesis that the modern system of writing itself was the result of Phoenician commerce.

Certainly, by the first few centuries BC banking was very well established, as Rostovtzeff (1941) describes:

> in Greek cities various types of banks were taking part in the custody and investment of money: temple banks, city banks, and private banks. The natural money operations of any bank would be: transactions in foreign money, especially the exchange of foreign into local currency and vice versa; care of deposits of various kinds, such as those for simple safe keeping, current deposits without interest, and the so called 'depositum irregulare' subject to interest; what is known as 'giro' or 'incasso', i.e. various types of transfer operation; credit operations of various sorts - loans on collateral security, pledges and mortgages, and a special very popular type - bottomry loans. (Rostovtzeff 1941:1279)

The dictionary definition of bottomry being the "system of lending money to shipowner for purposes of voyage on security

of ship, lender losing the money if ship is lost"[1]. Interest rates of the Hellenistic states were eight to ten percent in the early third century BC (Rostovtzeff 1941:1286). Baslez (1987) describes rich and influential Phoenicians resident in Greece in the second part of the first millennium BC, involved in a differentiated range of financial and trading activities - often family based. Their rôle looks rather similar to that of the 'business angels' in modern venture capital. Rostovtzeff sums up the structure of this activity:

> trade companies formed .... not for lasting association but for one commercial operation, one voyage abroad. .... such voyages were carried out with the help of sea loans given to the 'emporoi' and 'naucleroi' by private men or professional bankers. (Rostovtzeff 1932:765)

AN EXAMPLE FROM THE MEDIEVAL WORLD

Burman (1986) describes a remarkably similar situation in the same part of the world over a millennium later:

> In the eleventh century the foenus nauticum, or sealoan, was in widespread use in trade with Latin Syria. .... In this type of loan the lender assumed the entire risk 'since payment was contingent upon the safe arrival of the ship and goods or greater part thereof at the destination'. It is also interesting to note that interest was usually 33½ per cent for a round trip of about nine months in the mid-twelfth century, sometimes rising to as much as 50-60 per cent.
> (Burman 1986:77)

Burman's subject is the Knights Templar. Normal trade, mediaeval pilgrimage, finance for the Crusades, and all the movements of gold and capital that this implied, necessitated financial innovation to allow transactions across Europe. The

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[1] The Concise Oxford Dictionary - Seventh Edition.

Templars original remit was aid and military protection for pilgrims:

> It seems likely that the Templars were involved in the great improvements in credit and payment techniques that took place in the twelfth and thirteenth centuries, when the volume of money was increasing rapidly. (Burman 1986:75)

As Burman observes: "like all good entrepreneurs, they were in the right place at the right time" (p.78) - the Holy Land was a new urban, cash-based, environment:

> The Templars soon adjusted to these new ways adapting their dress, learning the local languages, and even training specialists in Moslem affairs within the order. (p.76)

Burman (1986:82,83) also notes their opportunism: "The surprising feature is their open flouting of church rules against usury" at a time when: "avarice and money lending were seen to be the symptoms of the moral decadence of urban life". Yet: "without such loans, commerce would often have been impossible", and interest payments could always be disguised under such as 'deductions for expenses'. The result of all these cross-currents was that the Templars ultimately became bankers to the crowned heads of Europe.

The Templars were not, of course, alone in the provision of financial services. During, and after, their centuries of prominence, others took advantage of the changing times - but, as Tuchman in her review of the fourteenth century world, sums up:

> Templars had moved from ideals of asceticism and poverty to immense resources and an international web of power outside the regular channels of allegiance. Tax exempt from the start, they had amassed riches as bankers to the Holy See and as moneylenders at lower interest rates than the Lombards and Jews. (Tuchman 1989:42)

JOINT STOCK COMPANIES

In the centuries following the Crusades the horizons of trade moved beyond Europe to become worldwide. There were new ship technologies, new routes and lands, new commodities, new philosophies, and the growth of new institutions in the City of London to service new financial requirements.

In their own structures and rules, these institutions re-worked the perennial debates about the borders of acceptable finance, as the Templars had in their day over issues such as usury. Lloyd's, the insurers of this new trade, started in a City coffee house, and reflect even today such debates in their literature[2] (not to mention their unlimited liability):

> Other risks are so speculative that they are uninsurable and rightfully belong to the preserve of the bookmaker. But when a genuine interest exists .... then the risk can usually be covered at Lloyd's.

A very significant new structure was joint stock funding, of which The East India Company was a prime transitional example. The 'Honourable Company' had its roots in a rather buccaneering tradition of seaborn trade - Keay (1991) documents its history:

> The organization of the Company is usually characterized as a half way stage in the evolution of the medieval guild into today's public limited company. It is also regarded as the most sophisticated example of an Elizabethan chartered company; and certainly it was significantly different from most of its Tudor predecessors. (Keay 1991:27)

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[2] 'Lloyd's of London - a Sketch History' - undated pamphlet published by Lloyd's.

The company grew to a power and size reminiscent of the Templars, resembling them, as well, in its strange fusion of trade and military power, particularly apparent in its effective creation of the British Indian empire in the 1750s:

> How was such a role change legitimized; and how come that an association of London businessmen, so naturally cautious, so very distrustful of their overseas agents, and so desperate to avoid political commitments and military overheads, came to countenance such extraordinary developments? (Keay 1991:221)

The joint stock company was a great step in the financing of ever more costly trading ventures, notwithstanding the setback of the spectacular demise of the South Sea Company between 1719 and 1720.

MERCHANT BANKS

In the nineteenth century, the limited liability company provided a vehicle for the new industrial ventures. Limited liability also created a moral unease reminiscent of medieval concern over usury:

> The industrial shares on offer were issued by limited liability companies, a form of business organization that was pushed through Parliament against the tide of business opinion, which continued to believe that unlimited liability offered the only real security for creditors. (Chapman 1984:99)

In the nineteenth century the merchant banks arose to fill the intermediary rôle that the Templars had once occupied, and the Phoenicians before them. Chapman's (1984) book, 'The Rise of Merchant Banking', seems a unique source of reference in this interesting area:

> A large number of histories of particular merchant banks have been published in recent years but, surprisingly,

> there has never been a history of this important sector of our national economy (Chapman 1984:ix)

Merchant bankers developed from among the many smaller merchants involved in the vast trade that companies like the East India Company developed. They started as accepting houses whose letters of credit financed trade across the world, as Chapman (1984:127) says: "a traditional mercantile activity". Some made a clear break with their other merchanting activities to become bankers, others drifted into banking. Many became issue houses as well, mediating finance for the new industries (albeit patchily, as Chapman discusses).

The banks had little money of their own - their qualification was ready access to large sums of money, and the 'indubitable credit' that allowed them to issue acceptances for up to three or four times their own capital. As Chapman says, "The merchant bank as a whole stood or fell on its A1 credit rating" (Chapman 1984:81), nonetheless, he adds:

> The ultimate irony of the British merchant banking scene was that a number of houses .... were so meticulously careful in mutual assessment of credit-worthiness, while in state and public utility issues they became gamblers. (Chapman 1984:82)

Chapman concludes:

> the Victorian merchant banking scene presents a fairly familiar picture to economic historians: a predominance of family businesses with a high turnover of firms (Chapman 1984:178)

The nineteenth century merchant banks seem to follow in the somewhat accidental, arbitrary, and informal mould of Phoenician and Templar finance.

The nineteenth century ascendancy of the merchant banks was ended as joint stock banks grew up. The French 'Crédit Mobilier' model, short-lived and disliked by the merchant banks, was nonetheless influential here. It was based on the Saint-Simonian ideal of progress, as Kumar records:

> it was two prominent Saint-Simonian brothers, the Pereires, who founded the Crédit Mobilier .... in 1852, and so established the basic type or model of continental capitalism, 'finance capitalism', with the banks as an organizing and controlling centre, directing under a coherent programme industrial concerns, railway systems, town-planning activities, and public utilities. (Kumar 1978:37)

Gerschenkron (1965) describes the conflict with the older institutions:

> The Crédit Mobilier was from the beginning engaged in a most violent conflict with the representatives of 'old wealth' in French banking, most notably with the Rothschilds. It was this conflict that had sapped the force of the institution and was primarily responsible for its eventual collapse in 1867. But what is so seldom realized is that in the course of this conflict the 'new wealth' succeeded in forcing the old wealth to adopt the policies of its opponents. (Gerschenkron 1965:12-13)

However, the English case was different in Gerschenkron's opinion:

> Between the English bank essentially designed to serve as a source of short-term capital and a bank designed to finance the long-run investment needs of the economy there was a complete gulf. (Gerschenkron 1965:13)

The interviews suggested that this nineteenth century divergence of British and Continental view still lingers today.

VICTORIAN AND EDWARDIAN INDUSTRY

It remains to consider in historical terms the area that this thesis aims to investigate in current terms - how good were past financial institutions at financing past innovations? Chapman's view on British merchant banks is clear:

> In a word, they had little more interest in British industry than they had in the industry of any other country, and that interest was inevitably minimal (Chapman 1984:98)

Kumar (1978:159) says much the same, as does Wilson (1985), from his American perspective, observing that:

> The entrepreneurs who created the Industrial Revolution, for the most part, had to provide their own capital, or seek help from their suppliers or customers. (Wilson 1985:14)

Crafts reinforces this view of haphazard finance for technology in the nineteenth century:

> To a very large extent technological progress was based on trial and error and chance discovery rather than large research and development expenditures. (Crafts 1988)

Michie (1988) carries the analysis into the early years of the present century. His rather different conclusions are interesting - he sees problems not in a dearth of funds but in over-regulation by government in Britain. He does distinguish innovations involving "social overhead capital", from those where capital needs were small (Michie 1988:494-5), and says that the latter:

> were more dependent upon the non-institutional capital market, such as individual or corporate savings and the funds of partners, relatives, friends, neighbours or business associates. (Michie 1988:496)

But Michie concludes:

> What evidence there is suggests an abundance of both funds and channels through which finance could be provided for all types of ventures before 1914. (Michie 1988:501)

The real problem was restrictive legislation. Michie cites areas like electricity generation where:

> the legislation gave control of electricity supply to the very people who had a vested interest in slowing down and limiting development (Michie 1988:513)

The red flag provisions of the Locomotive Acts of 1861 and 1865 had a similar pernicious effect on development of the motor-car he says.

FINANCE OF IDEAS SINCE WORLD WAR II

Funding innovation is clearly not simple, it implicates both finance and government. In the twentieth century an 'equity gap' has been perceived by many commentators - an investment below which even very high profits could not repay the cost of its administration, but still above that needed by many nascent businesses:

> This gap has been mentioned by every report on the UK economy and was mentioned by MacMillan in 1932, by Radcliffe in the 1950s, by the Bolton Report in the 1960s, and by the Wilson Report in the 1970s. (Cary 1989:8)

After World War Two, the UK Government attempted to provide new ways of financing technology by establishing the predecessors of both the British Technology Group (BTG) and 3i. BTG was formed in 1981 out of the National Research Development Corporation (NRDC), and the National Enterprise

Board (nowadays a minor part). Cary describes NRDC:

> NRDC was formed in 1948 with the objective of making profits for the nation from research carried out in universities and government-funded laboratories. It was granted the statutory right of first refusal to patent and exploit inventions from these sources, but this statutory right was removed in 1985 (Cary 1989:457)

Cary also describes 3i (see also Bank of England 1990:80):

> For much of the period from 1945 - 1980, 3i was almost the only organised source of venture capital in the UK .... 3i, then known as the Industrial and Commercial Finance Corporation (ICFC), was established in 1945 by the Bank of England and the major clearing banks in order to provide venture and development capital to new and expanding UK companies. Since then, 3i has grown into a formidable institution with a large office block in London and 28 regional offices throughout the country, and has 800 employees. (Cary 1989:61)

BTG and 3i represent two different models of how to finance innovation. BTG cossets ideas from researchers, assembles patents around them, funds development, and then licences its use by industry - 'technology transfer', the trading of 'intellectual property'. 3i, on the other hand, looks more like an investment bank - assessing opportunities, investing, and then monitoring the investment. 3i looms large in discussions of venture capital. Over the 1980s, UK venture capital burgeoned, as chronicled by the Bank of England (1982, 1984 & 1990), and surveyed by Cary (1989). Pratt's Guide in the US provides a similar survey (Morris & Isenstein (1989))[3].

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[3] Wilson (1985) provides a good popular history of venture capital in the US.

5.3 A Finance Primer

5.3.1 INTRODUCTION

Obtaining money for a new idea is complicated, not least because of the wide variety of sources available. This section introduces something of this complexity, and outlines a little of the scale and the structures of financing. It also starts to use the finance interviews - these are listed in the appendix to this chapter. The appendix to chapter seven includes an additional list which includes the geographical locations of interviewees' organizations, and splits them into venture capitalists, bankers, and intermediaries - a basic but convenient partition. The analysis of the interviews was described in chapter three - this chapter re-assembles the categories that emerged into a sort of ethnography of finance. The appendix describes the process in more detail.

The view that the resulting ethnography gives is broad, it encompasses large companies at one extreme and single individuals at the other. But it is still these individual people and their problems who are the main focus:

> "It's very confusing for the small person who is not used to complex financing" ideas. "The biggest problem .... is knowing what door to knock on".

5.3.2 A FEW STATISTICS

This thesis attempts something far removed from a quantitative survey of finance. Such surveys are already provided by organizations such as Venture Economics, and their Venture Capital Journals in the US and UK (see also the appendix to chapter two); economic analyses have been published - the Report of the Wilson Committee (Wilson (1980)) is a major example. All that this sub-section attempts is a quick comparison of the relative sizes, in monetary terms, of some of the activities that will be described below[4]. In the following table figures for both the UK and US, where available, are presented - arranged broadly to reflect the subjects considered in this, and the following, sections.

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[4] This will illustrate the discrepancy between what economic measures indicate is important, and what the interviews saw as salient.

	UK	US
Total Assets - 1988		
Commercial Banks:		$1,000,000M[b]
Pension Funds:	£214,518M[a]	$920,000M[b]
Insurance Cos:	£198,341M[a]	$500,000M[b]
Venture capital:		$31,140M[d]
Venture Capital Amounts Invested		
5 yrs 1984-1988	£2,704M[c]	
2 yrs 1987-1988	£1,800M[c]	$7,000M[b]
1 yr 1988	£1,006M[c]	
Venture Capital Amounts Invested in Start-up		
5 yrs 1984-1988	£252M[c]	
2 yrs 1987-1988	£114M[c]	
1 yr 1988	£50M[c]	

Sources & Notes

M Millions.

a 'United Kingdom in Figures - 1990' - Government Statistical Service leaflet.

b Morris & Isenstein (1989:v) - published by Venture Economics Inc..

c Bank of England (1990) - tables A & B - figures compiled by Venture Economics Ltd.

d Venture Capital Journal (1990) - total US venture capital industry pool of capital.
Note: Wetzel (in Morris & Isenstein 1989:99) suggests an additional $10,000M per annum is provided by informal investors in the US, Thompson (1991:157) cites an OECD estimate that in 1983 they accounted for 10-20% of the US funds raised.

These figures are instructive, even venture capitalists only invest around ten percent of their funds in the start-up of new companies - the area where new ideas might be expected to grow. Moreover, organized venture capital is probably only one percent of organized investment capital overall. Such figures must, of course, be treated with circumspection - as even those interviewed observed:

> You could get the statistics from Venture Economics, but treat them with caution.

5.3.3 DEBT AND EQUITY

A reminder of the structures that finance uses is needed. Debt and equity are the most basic of these, but royalties from patents and licensing are also relevant to the development of innovative ideas.

Debt and equity constitute 'traditional' ways of providing money for ideas. The debenture once bridged the division between them, but today a complex array of newer financial instruments exists as well. The interviews reflected this complexity, although most agreed that equity was usually the only option for new companies:

> With a new idea, bankers say 'go away and get equity', but "equity is the most expensive form of finance" because the equity investor expects high profits with high risk; the banker gives lower interest for low risk.

For the small innovator the only 'new' financial development that is likely to provide equity is venture capital, nonetheless some new debt instruments were mentioned at times, albeit for later stage investments:

> "You've got a grey area growing - .... mezzanine debt, subordinated debt - .... bankers are looking at how far they can push debt finance". Venture capitalists are pushing too - coming together maybe - but never that close - "the potential for conflict is too great".

5.3.4 GOVERNMENT AND PUBLICITY

This sub-section considers two final general topics that were talked about a lot in the interviews. Government might have been expected to exercise debate, but the prominence of publicity, so forcefully demonstrated in the case-studies, was

less expected.

Government taxation, regulation, or funding can all have direct effects on the exploitation of ideas by business, but it was the more general effects of policy, and of the climate that government creates, that seemed important to people. The technology and finance interviews included a considerable body of expertise here - a former Minister at the DTI, various Civil Servants (some of them very senior), an ex-Chief Scientific advisor to the MoD, the Bank of England, people on government advisory bodies, professional regulatory bodies and associations (such as ACOST, the Engineering Council and the British Venture Capital Association). Added to these, and equally relevant, are those who have dealt with Government at all levels - the chairmen of large firms, working bankers and venture capitalists (including those in 3i and BTG), as well as the individual entrepreneurs and inventors.

The Department of Trade and Industry was mentioned in interviews as active in providing help, although some unease was shown at its periodic, and often drastic, political and philosophical changes. The rôle of the Treasury was also mentioned, with fiscal policy seen as a possible way to promote finance for new ideas. Many individual government policy options could be discussed here, but one scheme did excite much comment, and ought to be singled out as an example

- the Business Expansion Scheme[5]. Comment about BES was generally critical - it ranged from: "it has been a disaster" through to "partially effective", with judgements like: "a bit mixed" being typical.

At a more general level, observations on the rôle of Government were varied, but most seemed to accept that more government involvement, not less, was needed:

> "I detect .... - this is arm-waving - in Japan, the US, and Germany, there is more national pride about technology; .... they're more prepared to pump money in. .... No sign of that in UK in the small company sector". .... "I think government has to encourage industry in this".

Neither was comment particularly negative upon the concomitant regulation that government involvement implies:

> Red tape is not a problem in start-ups; they have to exist in the world as it is; Government intervention would just change the world.

Perhaps the main observation here is not so much what interviews said about government, as the very ubiquity of such comment. People saw government as clearly implicated in supporting innovation:

> "The criticism is we [the venture capital companies as a group] don't invest in innovation, and the arrow strikes home" maybe .... We have to make money, and our experience is that young things do not - it is a shame, but it is society's problem.

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[5] In the budget of 10/3/1992 the intention to end the BES scheme on the last day of 1993 was announced. The chancellor acknowledged the growth in venture capital since the introduction of BES in 1983, said it "has fulfilled a useful purpose", but observed "nowadays only a small part of the total invested goes to small businesses".

A crucial factor in innovation, the research repeatedly illustrated, was peoples' abilities to influence government and each other. Publicity and its manipulation emerged, unbidden, as a central theme in this thesis. Comments emphasized the various sides of this issue, one cited persuasion: "The most important thing is the persuasive power of the individual"; an American banker emphasized the need for a network of personal contacts:

> "The fund managers' friends at the golf course .... told them of fabulous returns". .... "It's like clubs, it's who you know".

People underlined "the power of public opinion, of the media", here, but they also saw its negative side too: "Hype is a problem, people get on the news, they influence people".

A few interviews discussed the use of public relations consultants here:

> So, PR people can provide a useful service, but it has grown a lot of late, and they are very naïve and inexperienced often. They are just intermediaries.

Others discussed more extreme methods:

> "in an attempt to get the City to take me more seriously I gave a lunch, it cost me a vast amount of money, at the Savoy - for two hundred and fifty people".

The case studies of the last chapter have already provided some fine examples of the effects of publicity - little needs adding here. Insofar as a common opinion emerged on publicity, it was a qualified acknowledgement of the aid publicity can bring to innovation. A final measured comment from the HOTOL interviews exemplifies this:

> "It's not something you can have a fixed view on", you do not want publicity all the time; it's give and take on each side between the company and the press.

5.4 The Traditional Sources of Capital

5.4.1 INTRODUCTION

In many ways this section is a description of the status quo, the ways in which most technological ideas have been, and for the most part still are, financed. There seem to be two elements to this - divided simply by size. On the one hand are traditional banks and finance institutions, together with big industry; on the other hand are individuals, entrepreneurs, inventors, together with local sources of funds and advice. These two poles might be characterized as 'formal finance' and 'informal finance'; both can be seen as 'traditional', in contradistinction to venture capital. This section looks at them.

5.4.2 TRADITIONAL FORMAL FINANCE

Formal finance is based in the relationship between large companies and their financial advisors, between banks and industry, and activity in the capital markets links the two. The interviews described these capital markets, together with the large investment funds, merchant banks and other banks.

MERCHANT BANKS:

As section 5.2 showed, the merchant bank is the oldest of the modern banks, and fulfills functions that go back into antiquity. One interview described its functions today:

> "Merchant banks are fee driven organizations". There are three areas, or profit centres:
> 1. Corporate finance, advice, the stock exchange.
> 2. Asset management - .... ninety percent is pension money .... it can be entrepreneurial, but this is limited.
> 3. the third area is lending.

Asset management is often through funds linked to the bank, as distinct from commercial and investment banks who invest their own money.

In the main, such banks and funds are not involved in supporting new ideas or unquoted companies, but, rather, in advising large industrial concerns:

> "Banking is a very conservative business - it's never had a very high repute as regards innovativeness". There are two sorts of banking - merchant, and commercial that was spun out of it. Merchant banks were tied to trading, commercial banks looked after the money. "The distinction is still very much there" - merchant banks advise, commercial banks still look after the money.

The merchant bank is not a place to go with a radical new idea, as was colourfully explained in one interview:

> "Most inventors think people with money are a spiv", financiers think inventors mad; "both are probably right". They "come from different backgrounds". Most researchers despise merchant bankers, and merchant bankers say "if they're so clever why are they so poor".
>
> Inventors want small amounts - "never for serious money" - merchant bankers say "'I haven't got time to look at it'. .... They haven't got clear ideas .... - they're different kinds of people". "The number of people that can act .... between those two is very small". It is different in the States.

FUND MANAGERS AND OTHER BANKS:

The large institutional funds clearly lay in the background when many of the interviews discussed investment:

> It is the institutions' attitude that is the key - a few own a large proportion of the funds. There were more individuals in the past, but we have seen the growth of pension schemes since the war, with the tax breaks.

Institutional funds may invest in venture funds (so may large corporations), and various interviews discussed this:

> A fund manager looks at the size of his typical investment - he could not justify lots of effort for one hundred thousand pounds - so he uses us as intermediaries. They have, I guess, a few percent in such investments, I am not sure.

They may also try venturing for themselves, although dangers were seen here:

> "The British institutions respond to an investment fashion by setting up their own teams .... This tends to flood the market with money to share too few good investments". They feel they have to do everything themselves rather than subcontract to specialists.

The other banks complete the picture of traditional formal finance; their own capital and deposit bases provide debt finance:

> "Our business is providing people with money" - we have obligations to our shareholders to lend wisely and with a return - we are not providers of equity.

However, the traditional attitude to entrepreneurs may be changing:

> Clearing banks are moving back in. These companies "structurally are not set up to deal with it". They are buying in the specialized skills to deal with venture capital.

Nonetheless, the traditional picture does seem to remain largely intact; merchant banks advise and facilitate,

commercial banks provide loans and financial services, the institutional funds invest equity. Behind this, a very strong network of formal and informal links becomes evident in research. Talking to a merchant banker, or to the manager of a fund associated with a bank, the researcher is often left with a sense of vague organizational boundaries - people seem to talk for, and about, both organizations at once.

STOCK MARKETS:

The traditional formal meeting place between finance and industry is the capital market. A banker described how a medium sized company might fund an innovation:

> A company will probably get a loan, or a rights issue on the stock market - not the BPs and ICIs - more likely companies on the Unlisted Stock Market, whose "directors are basically betting the company on the new idea".

However, with small companies the interviews suggested that stockmarkets were more important for their exits than their entrances. Investors may try flotation to realize the value of a start-up company, even if - as will be discussed below - a 'trade sale' to a larger company is far more common.

Nevertheless, the capital markets remain the cockpit of activity surrounding takeovers and management buyouts, and a focus for the proliferation of new tradable instruments. Both of these are of key significance to innovation and industry.

5.4.3 TRADITIONAL INDUSTRY

The interviews suggested that traditional finance leaves judgements about technological innovation to industry, so what does industry do about it? This is an important question - at the most global level, the things industry produces are indispensable to society:

> In the 1980s a third of manufacturing industry went to the wall. .... they are the only creators of wealth in this country - everyone else just passes it around.

Answers to this question of companies' abilities and willingness to develop new ideas, were mixed. Those 'for', saw industry as having the best resources for the job: "I think .... those projects are .... most likely to succeed if they are taken up by major companies". Those 'against', often from the seedcorn area, tended to see problems within industry of policy, inability, or simple unwillingness:

> "There are major British companies .... who are rarely prepared to take speculative ventures" - they just keep cash mountains, they won't underwrite projects.

US interviews showed a much greater sense of optimism, but a tension between big and small companies was still evident.

An institutionalized solution some have tried here, is 'corporate venturing' - a company spinning-off its own venture capital operation:

> "Corporate venturing has probably seen its heyday". It is largely through venture funds, but also direct too. It is really just an arm of a corporation - if it helps a large R&D collaboration they will put money in.

Comment about success was mixed, a key factor seeming to be how closely the parent restrained its offspring. Wilson (1985:153) discusses US corporate venturing, and observes that

'hands-off' relationships between parent company and fund seem to have been most successful. A banker, describing the internal investment activities within his own bank, raised some relevant issues in this clash of large and small:

> "one likes to think" the written proposal is what happens, but "sometimes what happens is so remote from what was written that it is amazing. People scratch their heads and say 'how the hell did we get here?'" It usually takes at least two management changes, too.

The tentative conclusion from all this would seem to be that large companies need small companies to innovate, as a final comment illustrates:

> "The large companies cannot be as innovative as the small ones". "They will always play an important role in funding the small companies". They have the experience of regulatory authorities, in scale-ups, and - very importantly - in marketing and distribution. "But what the big company lacks is new products, new ideas".

5.4.4 'SHORT-TERMISM'

The impression emerges that both traditional banks and established industry show resistance to innovation, so who is to blame? This question exercised the people interviewed, just as it seems to exercise the press and the public.

The issue is encapsulated in the debate over 'short-termism' - an assertion of evident symbolic significance, regardless of its veracity. 'The Financial Times', in an article tending towards scholarly rather than populist debate, defines short-termism thus:

> Businessmen, such as Ivan Yates, deputy chairman of British Aerospace, maintain that efforts to develop companies through greater emphasis on research and development have been frustrated by the need to watch their companies' share price continuously for signs of

> vulnerability to hostile takeover. They say that funds which could be put into R&D are being diverted into dividend payments and that management has to concentrate on keeping short-term profits high. (Financial Times 21/5/1990 p.11)

An interviews with an engineer from the world of big industry echoed this view:

> "One of the big problems we have in this country, not in Japan, is the short-termism of the City"; a three year view. That makes innovation difficult.

The strange thing was that interviews in the City also echoed such criticisms:

> "Another aspect of it is we're all under such pressure, investors keep judging you on your last six months", "the City is always blamed for short-termism" - for not funding - "perhaps we do, but we is, rather, you" - it is your money.

A close observer of industry supported the criticism:

> It is awfully difficult to identify for certain why there should be short-termism. There are lots of arguments - the evidence you've got is all anecdotal. It strongly suggests there's a problem [Comments augmented after interview]

The Bank of England seem to see short-termism as a problem[6], it damages: "long-term, high-risk investments which need patient money" (Bank of England 1990:82).

The only thing that emerges with clarity from all this is that the debate is not a simple one. The 'Financial Times' article catches something of its rhetorical character:

> There is a lot that is attractive in the short-termism critique. In the UK, at least, it offers a scapegoat (the City) for British companies .... But attractive as it is, there is little evidence to suggest that those who advance this argument can prove it.

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[6] See also Mayer (1987) for an additional scholarly perspective to the debate.

(Financial Times 21/5/1990 p.11)

Many of the interviews sensed this symbolic use of short-termism too, one person wryly observed:

> Inventors say it is "City short-termism .... - '[they] don't understand science!'" I see some invention on the morning news; "my wife believes it, my kids do, my neighbours believe it, ... [but] would I mortgage my house for that guy?"

5.4.5 TRADITIONAL INFORMAL FINANCE

If established industry is backward in funding innovation, then much of its finance must come from other 'informal' sources; the rest of this section considers these. Informal investors, and other highly localized sources, are easily missed in grand statistical surveys[7], yet the interviews underlined their importance.

A small firm, or individual with an idea, has many fewer options than a larger organization:

> "The position of the individual .... who has an idea is a very difficult one, ... there may be very few companies who are prepared to back" them, ... he is likely to go to a High Street Banker.

Even in the US a banker commented:

> The small man "has a hell of a time getting finance" on a senior basis. .... "In the US", once they have exhausted their own and their wife's money, "they are forced to go to high risk venture capital sources".

Their options, then, are their own funds, or those of relatives and friends, loans from high-street banks secured against their house, and possibly grant aid, or other

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[7] As note 'd' in the table presented in 5.3.2 above implied.

assistance from government and local authorities. If these fail their only salvation is an 'informal' source of funds - it is perhaps no accident that such sources are commonly given the soubriquet 'business angels'. Their rôle in financing new ideas was a topic that emerged clearly and consistently in a number of interviews:

> In the US "there are a significant number of people with enough personal wealth .... to gamble in the hi-tech. area", to put up small amounts of money - "one million bucks on a straight up gamble". It reflects more people with large disposable wealth who are not over-concerned with security. That is what you seem to see; there are a few examples in this country

This section has revealed the clear sense of a pathology whereby a deserving idea is ill served by large industry and traditional financial institutions alike. Over the last decade or so, venture capital has been seen as a possible cure - first in the US:

> "The idea was you get very good returns if you bankroll new ideas, new companies". Institutions would not touch them - "there was a gap". The traditional methods - family and friends - could not provide big enough sums for some industries. In 1969 most people would not have known what a venture capitalist was. "It's been around since the Phoenicians funded a few boats in the Mediterranean".

The rest of this chapter investigates venture capital.

5.5 Venture Capital - The Diachronic Structure

5.5.1 INTRODUCTION - STATISTICS AND STRUCTURES

At the outset it has to be emphasized, again, that most new ideas are not funded by venture capitalists at all - individuals, family, local banks and the like, still finance most business start-ups:

> Venture capitalists are really involved in very few businesses. There are a lot of new businesses - "ninety nine point nine nine nine percent would never go near a venture capitalist".

Nor is the majority of venture capital funding channeled into high technology[8]:

> "Most of the time .... the venture capital industry is not very venturesome", they seem to agree often, too.

Nonetheless, the venture capital portfolios are more biased towards new technology, high growth potential, companies, than are other funds. Their investigation is therefore a reasonable way to proceed in looking at technological innovation

STATISTICS:

It is worth starting with a few more statistics. The Bank of England (1990:80), and Venture Capital Journal (1990) in the US, split funds into three very broad types, and present the following statistics for 1988:

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[8] For example, the Bank of England (1990:79) point out that in 1988 62% of venture capital financing went into buyouts (Table B), and their sectoral breakdown of the non-buyout investments suggests that a quarter of the companies invested in were consumer related, and another fifth service related. Charles Batchelor states that in 1988, "Just 9% of venture capital funds went to technology-related businesses" ('Financial Times' 4/4/1989).

UK (% of amount invested)		US (% of total resources)		
Independent (includes 3i):	66%	Independent private:	83%	
Captive (financial):	31%	Corporate financial:	9%	
		Corporate industrial:	7%	(note a)
BES (note b):	2%	SBIC (note b):	1%	
Govt / local authorities:	1%			

NOTES:

a: The Bank of England comments on the lack of UK corporate venture activity: 'with a few notable exceptions .... UK companies appear to be lagging behind US and even European counterparts'.

b: BES (Business Expansion Scheme) and SBIC (Small Business Investment Company) schemes are very different, and should not be equated, but both represent Government attempts to promote the growth of new businesses (BES, since 1983, through tax - see Cary 1989:303-305; SBIC - since 1958 - through tax and Government loan funds - see Wilson 1985:21-24)

The sources of the independent funds' capital, and the geographical distribution of the practitioners are also of interest. The Bank of England (1990:table F) gives the following breakdown of sources for independent UK funds in 1988 (£612 million in total):

UK pension funds	37%
Foreign institutions	26%
UK insurance & fund mgt.	19%
Private / family trusts	11%
Industrial corporations	6%
UK banks	0%[a]
Others	1%

(Note a: Banks committed 6% in both 1986 & 1987)

The Bank also comments on the geographical preferences for investment: "The South East has consistently received more than half the total amount invested" (Bank of England 1990:79, see also table C). Strong regional biases are also to be seen in the US (Venture Capital Journal 1990:17-18, see

also the appendix to chapter two).

THE STRUCTURES OF VENTURE CAPITAL:

The intention of the rest of this chapter is to put flesh on these bare statistics. Its organization is suggested by two tentative structures that emerged from the interviews, or rather a structure and a process. The present section will discuss the way the venture capital industry, and the funds themselves, are structured. In the next section discussion will be loosely hung upon the process of making a venture capital investment. The final section considers the start of the process - seedcorn.

5.5.2 VENTURE CAPITAL FUNDS AND STAGES

FUNDS:

The venture capital fund, the first obvious structure to consider, is a common way of organizing such finance. Funds have fixed lives, at the end of which proceeds are shared out between the venture capitalists and the investors. A venture capital firm will raise a number of funds over the years. A US interview described the structure:

> Typically ten years. The general managers typically get twenty percent (they may have to pay off the limited partners first), plus a fee of one-and-a-half to two-and-a-half percent - for salaries and overheads.

UK interviews revealed similar structures, although they are not universal:

> It is just the most convenient, but not the only, way. There are other sorts of funds, ... that are stock market listed - but that can give more problems. It is really just a legal way of aggregating a number of investors.

Most of the comment on fund structures in the UK came from seedcorn venture capitalists.

STAGES

The discussion so far suggests various levels of venture capital funding, from isolated new ideas through to substantial industrial concerns. In the interviews, a subtler texture of funding stages emerged from this, especially when they discussed the actual amounts invested.

Banks and project finance suggested a minimum investment of tens of millions of pounds. The 'captive' funds, and the independent venture capitalists, clearly invest on a scale an order of magnitude lower - down to around half a million pounds. Below this level are the seedcorn venture capitalists[9]. These stages suggest a sort of structural framework for venture capital investment.

Cary (1989:8) suggests seed capital (£5000 to £100,000), start-up capital (£100,000 to £5M), early-stage development, later-stage development, and management buyouts (£5M to £25M); the cash amounts are merely indicative. The Bank of England (1990:79) use start-up, other early-stage, expansion, buy-out / buy-in / acquisition, and secondary purchase. The British

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[9] The levels of investment of the UK venture capitalists interviewed are presented in more detail in the final section of this chapter.

Venture Capital Association directory for 1990 talks of "start-ups and other early stage investments", "expansion investments" and "management buy-outs and buy-ins". In the US, Pratt's guide (Morris & Isenstein 1989 pp.2-3) makes even finer division into early-stage financing (seed, research and development, startup, first-stage), expansion financing (second-stage, third stage or mezzanine, bridge), and acquisition/buy-out financing (acquisition, management/leveraged buyout).

> The interviews described these stages too:
>
> they go through various stages - .... pre-seed - ten thousand pounds or so; then comes seed and start-up - up to one hundred thousand; then there is the early-stage development funding gap - up to two hundred and fifty thousand; beyond that late-stage development.

Moreover, people seemed to see these stages as based in real differences in financial need. There also seemed to be a UK perception that US venture capital was in some ways purer - earlier stage:

> "I think in the UK funds get blended - .... you get the hybrid funds that do anything. .... In the US you don't see this at all - in the US a venture fund is early-stage".

Venture capital emerged as very broadly specialized by stage - US interviews suggested this was a newer development:

> From the seventies to eighty-one there was no specialization, "then the industry grew", specialization by stage, technology, geography.

The interviews were clear about which stages the industry preferred - the later stages:

> there are very few real venture capitalists. The tendency is towards the other, safe, end - management buy-outs

The earlier stages, particularly the seedcorn stage, are seen as underpopulated, and different in character:

> "Probably only five percent in very early stages", like start-ups .... - they are different, older, a lot of experience in industry

There is a tantalizing hint in this of a very real boundary between early and later stages - a hint that will be further considered later.

These stages are useful categories for analysis - management buyout, development, start-up and seedcorn are used in the rest of this chapter. Management buyouts, and then development, are considered in the rest of this section. Start-ups are considered in the next, where the process dimension will be superimposed. Start-up provides a good vehicle for describing how venture capital is 'done', raising, as it does, many of the issues that also concern other stages. The final section will then turn to seedcorn.

5.5.3 MANAGEMENT BUYOUTS & DEVELOPMENT FUNDING

Management buyouts (MBOs) were frequently mentioned. The UK interviews gave some idea of their mechanics - this was greatly augmented by the Vosper-Thornycroft case described at the end of the last chapter, so little need be added here. The US interviews also spoke of MBOs, setting them a little apart from 'normal' venture capital:

> this notion that you can sit by and watch your money grow will change as the MBO market becomes more efficient - they will have to move back to venture to add value through their experience. "The buyout folk have

> financial engineering skills, the venture folk have operating skills" - but each needs the other.

There is a foretaste here of what the venture capitalist adds to an investment beyond mere money. This surfaces again as the sense that there is something unwholesome in venture capital's involvement with MBOs:

> "Venture capital in the UK is criticized .... for doing buy-outs. .... The counter-argument is that it unlocks entrepreneurial ability in the management".

This unease perhaps reflects a dichotomy within venture capital. One part of its activities creates new companies around innovative ideas that are beyond the view or valuation capabilities of any stock market, but another part applies additional funds in a much more formalized way to allow the company to grow so that it can be served up to a stock market or, more probably, sold to a large company. Development funding lies somewhere between the two:

> "It moves from the research to the development stage" - that's where the money gets spent. .... "At that stage, if you've got sufficient confidence .... you can raise money easily", if you have just got a new idea it's more difficult. .... "A sort of rubicon you get to when the thing becomes more than a gleam in the eye".

This whole process is encapsulated in the term 'start-up'. Start-up is more than just a stage, it is a diachronic process. Yet this process is also cyclical - the venture capitalist starts-up one company even as another is exited from and a third developed. The venture capitalist is concerned with all of these activities at the same time - the

researcher can thus lift them out of time, and study them synchronically - the next section does just that.

5.6 Venture Capital - The Synchronic Process

5.6.1 INTRODUCTION

The term 'venture capital' has been used throughout this chapter, but it has not yet been defined. The last section showed it to be new, small, and diverse, and this diversity makes a simple definition difficult. This section describes what venture capitalists said they do when they invest in the start-up of a new company. The result reflects their multiple perspectives, and provides - for the purposes of this thesis - their own definition of venture capital.

5.6.2 ENTRY TO A START-UP

PROCEDURES:

Formal procedures mark the entrance and exit to the start-up stage. Business plans, prospectuses, and their evaluation, allow the people with ideas to talk, symbolically, to the people with funds:

> "This business plan .... is a series of financial projections". "What we then ask, ... is ask them to do sensitivities on it". .... "The problem of playing with numbers .... is that you" can fool yourself you are looking at reality - in fact "they will be all over the place". "It's a real guessing game, ... balancing a number of imponderables against a bit of paper with numbers on".

These mechanisms are symbolic, perhaps, because they are trying to do the impossible - to value an idea: ""How do you price technology?"", and to predict the unpredictable:

> what emerges is "a third or fourth cousin" of what you first envisaged .... If you looked at it rationally you probably should not put your money in, but you back a hunch. In "every case .... we've ended up with a different business from the one you thought you were going to be in".

PUBLICITY AND REFERRAL:

Before a venture capitalist even receives a person's business plan, that person must know he exists. Venture capitalists need to publicize themselves:

> I was struck at first that "venture capitalists were clearly the most sophisticated users of the media". .... "It's a fairly small industry, and a fairly friendly industry too", a friendly and chatty one.

But publicity can bring an unmanageable deluge of hopeful approaches, thus referral from someone within a venture capitalist's network of contacts is more usual, not to say desirable:

> We generally get ideas from third parties - accountants, lawyers, other venture capitalists. We also get entrepreneurs sending ideas into us, but that takes time to assess. We get press coverage that lets them know we exist - but we really only deal through third parties - we want <u>them</u> to know we exist.

There was fairly general agreement that coming with an introduction was best:

> The chances for a cold deal are remote, well less than a thousand to one. Similarly, "a complete green field startup is very difficult and risky" .... only with someone here with a personal contact would we do it.

Another way of getting knowledge about ideas was to share it:

> "There's a good spirit of cooperation, not competition". .... "We will be introduced to deals quite often by other venture capitalists."

Perhaps not surprisingly, venture capitalists did not respect outsiders' views so much:

> "Recently there are much more financial intermediaries around the country" - the financial services act has opened that up - honest brokers. "We get a lot of approaches, ... not proper business plans, ... they will charge a fee, ... they are in the local area, ... they appear to be friendly where most venture capital companies are obscure edifices in London". .... "We get a lot of unsolicited mail from them; .... very rarely do they come and meet us, ... often they have no more of a clue as to what venture capital is than the man who came through the door"

In short, a sense of community seems to characterize venture capital. An observer of the industry summed it up:

> They are all intensely competitive if a really good deal comes along, but they cannot steal people afterwards, so they may then join in if they are invited later. So there is both competition and happily working together - but it is a very small industry - you have to get on with people. So, they will recommend things on to others if they cannot do them.

5.6.3 PRODUCTS, MARKETS, PEOPLE AND MANAGEMENT

The next question is what sort of ideas venture capitalists pick up? What pre-disposes an idea to success? An American gave a very basic answer:

> an entrepreneur needs a good idea, a team, an area of interest, a geographical area where venture capitalists congregate.

He further emphasized the chance nature of this:

> "Now, you would assume things like management, projections, would have something to do with it, and they do - but it is amazing to me what effect fashion and location have.

Nonetheless, most people were very willing to enunciate a clear set of priorities for investment:

> There are three 'm's - management, market, and .... money - you can get away with two out of the three - you are loathe to go with just management.
>
> "It's the cliché". "It goes people/technology/markets in my view" - do not "get carried away by the gee whiz syndrome" into just backing technology.

People were obviously rather used to talking in this way - it all smacked of an oft-told tale, something of a symbolic artefact. Many interviews returned to what are, basically, the same two themes - the technology and its market, and the people and their management ability.

The first of these is perhaps the more obvious. The interviews themselves best sum up people's concerns over market and technology:

> What do you look at? "You're still looking at technology", the market; you are still looking to make sure the engineers are not seeing too big a market - being duped by the technology. There are "far more products looking for markets than vice versa".

US views were similar, save for a difference in the potential size of the market. However, when it came to management, perceptions of the two countries diverged markedly:

> US business plans, entrepreneurial teams, are much more rounded; there is more "functional diversity within a person" - marketing, technical, etc.. "Brits are only one function".

This introduces the second element in the primaeval soup that venture capitalists saw as necessary to bring ideas to life as new businesses - people and management:

> "Within my experience - .... the last fifteen to twenty years .... - the biggest problem has been a lack of

commercial awareness/management among those who have good technical ideas."

Many interviews talked about these problems, for example:

> The problem is not the technology, or even the market, it is management. The "lack of good managers". "We try very hard to do the hands-on bit. I mean, I sit on the boards of five companies". .... "Usually they will take your advice, but there are inevitable problems as the company grows - "it exceeds the skill sets of the founders". The venture capitalist's skill is doing that in a non-threatening way, so the founders come out feeling good. .... So, we get reports every month, attend board meetings.

This willingness of venture capitalists to get involved in the running of investee businesses is the one thing that might make them distinctive within finance. Although even within venture capital a dichotomy was suggested here:

> "The glib thing is, seed has to be hands-on, venture capital hands-off". In the risk view, we minimize risk by getting more involved.

The general solution to management problems was to apply strict controls, and sometimes to change management:

> "Normally, at an early stage investors will put some tight constraints on a company". With "many things subject to board approval, and investors will have a tight control on the board". So it is equity control actively applied. "There's very seldom any technical problem in changing management".

Added to this was a clear need for judgement - one interview summarizes much of what this section has said thusfar:

> "what we would require is a .... business plan .... - fundamentally, how long .... is it going to take from the ideas stage .... to a production model .... to sale". .... "If we are sufficiently impressed by the management .... - we are backing the management team" - its your judgement, "at the end of the day you are putting money in". You want to see a basis of sound principles, and a knowledge of the market.

Venture capital, then, like interpretive analysis, has its narrowly 'rational' elements, but people need confidence

to break with too rigidly procedural a view too, as a final US extract says!:

> Feel is very important, it is the animal thing. If a man is wearing red suspenders - you would say braces - "I can't say I won't invest in you because you're wearing red braces - you're a prig" -but I won't

5.6.4 TECHNOLOGY TRANSFER

In a 'start-up' a venture capitalist takes an idea, and funds it by embodying it in the legal form of a private company - jointly owned with the person who brings the idea. The 'exit', whereby the investment pays off, is through selling the company, probably much later. This sub-section very briefly considers an alternative process - technology transfer.

Licensing, through the use of patents, is the usual form of technology transfer. A patent is a much more austere legal vehicle than a company - it contains only the technical elements, divorced from the people that originated the idea and the management skills of the venture capitalist. Considerable management skills are nonetheless present in the choice of which ideas to support, and how and when to patent them. The investor is then paid by the continuing revenues this may bring. Some of those interviewed had considerable experience in this area, and saw it as a difficult one:

> Ideas have to be turned into property. "A bridge between invention .... and potential products is a most important transition" - most financiers and inventors do not understand it - most patent agents do not either. Defensible patents that are acceptable and form the basis of a licensing strategy are very difficult.

The large sums of money needed aggressively to defend patents was noted, as were many of the same problems that venture capitalists raised. Nonetheless, the collection of attributes demanded for technology transfer were seen as unique:

> a mix of legal, technical and financial expertise, and money. Others have two, but not all four of those.

5.6.5 RETURNS, FAILURE AND SUCCESS, RISK

Returning to the venture capital process, the next questions to ask are the returns sought, and the success achieved. Expected returns were clearly high - although comments varied, typical ones suggested:

> "We are very happy if you get ten million back" - a fifty percent return - in reality we will accept twenty or thirty percent, "though no venture capitalist would admit it".

The real question is what did people achieve? The answer is difficult, people are often reticent over such matters, but their comments do show that many investments fail - the necessity of a portfolio of them is clear. An American gave a typical reply here:

> "The rule-of-thumb - .... if you start ten companies, one or two become real big, ... one or two fail, ... the rest are [in between, some become] living dead". "The real home-run hits - the twenty percent - .... are [usually] the public offerings". Knowing when you have failed is the problem often; even the living dead start-ups may get picked up and become successful later - maybe after years - that "happens more often than people believe".

There was an interesting consistency, here, in people's vocabulary and imagery - they talk of 'hit rates', and the 'stars' and 'dogs' bequeathed to the management community by

Boston Consulting Group. Homely images of 'rules-of-thumb', jostle Grand Guignol visions of the 'living dead'. Moreover, this seems the same on both sides of the Atlantic. People seem to shelter from the uncertainty and guilt of their failing investments under an umbrella fashioned from a shared vocabulary - a vocabulary that gives them a reassuring sense of a shared experience perhaps.

Another consistent theme is the idea of risk. This idea fits in neatly with the idea of stages - a quote gives the flavour:

> It is risk assessment - a financier would love a deal with almost no risk, but in practice you can draw a graph of the risk of failure against time .... risk falls as a prototype works, pre-production succeeds, the market survey is good. So, as the risk falls, the investor will accept a lower return. But it is not scientific - "I hate the horse racing analogy", but if I am only backing the favourite, I can back it more times with success.

Not only did people see risk changing with the various stages, what 'risk' meant also seemed to change. The bankers had no trouble with the concept:

> A bank will only take exchange rate or interest rate risk - not a market risk, certainly not technology risk.

But the seedcorn people certainly did:

> It is "often thought of as venture capital on a small scale" .... but often seen as more risky - I would take issue with that. "What is risk?" .... In the risk view we minimize risk by getting more involved.

A reified concept such as 'risk' seems to come increasingly into question as the problems of the seedcorn area are approached. Seedcorn seems to be on a cusp of changing ideas.

5.6.6 EXIT FROM A START-UP

The final part of the venture capitalists' involvement in a company is their exit - the realization of their profits. One of those interviewed discussed the options for getting out in some detail:

> "out means an exit to make some return". We may sell out our shares and call in the loan. The founders may come along and offer to buy out - even at a premium to persuade us, but "that very rarely happens. The other one is a trade sale .... to someone else", selling just your holding in the business - usually you have got to be careful of those. "Then, there's a complete buyout - .... lock, stock, and barrel" - maybe with management or without them, but they make a lot of money. Then there is a flotation, but that does not happen in hi-tech at the moment - the USM does not work - there may be a private placing very occasionally. .... "We try never to sell from underneath a management team, ... we don't want our reputation sullied". "My experience is, getting out is much more difficult than getting in" - there are many more parameters, "one of which is greed"

There was a degree of consensus that stock market flotation was the most desirable, but rare, form of exit. Even the unlisted securities market, set up with easier flotation in mind, was not seen as uniformly helpful. Sale to another company - a 'trade sale' - was regarded as most common, and even a simple distribution of the private shares to fund investors was not unknown. Comment from the US was similar, although, there, more optimism about market flotation (initial public offering) was evident.

Once again, some of the comment suggested seedcorn was different:

> "Expansion absorbs cash - .... at that point we'd go out and help raise that cash - but not exit; .... we generally think we can make more money by not exiting". So, if we start off with forty percent, and if then a venture capitalist comes in and wants thirty percent, we will all get diluted - we will then have twenty-eight

percent of an enlarged business.

Exit is difficult, then, and usually implies a loss of the start-up's autonomous existence, as an observer of the industry remarked:

> The paradox is that exit is selling it to someone else, so there is no real independence involved.

5.6.7 START-UP IN RETROSPECT, IDEAS IN PROSPECT

Bankers and venture capitalists, from all stages, sometimes seemed to sense that all is not quite well in the financing of new ideas:

> "This is an area that is not loved", people do not like startups - risky - you do not know where they will go, especially in technology; .... There are very few people who will invest at the level we invest - £150,000 costs as much in due diligence as £1½ million - they start at £350,000 or more.

There was also a lot of discussion of ideas and innovation in the interviews, and people very evidently cared. Two different quotes stand for many:

> "There is no substitute for vision, ... you can do all the research you like, ... but there have got to be men of vision around" - you need a man of power who is a man of vision.
>
> "Our experience of backing bright ideas has not been good over the years". .... "There has to be a way". "There's always going to be a flow of brilliant .... ideas that will work". "The problem is spotting it"; "the only approach is the shotgun approach". "Nobody's really got the right formula". .... "What we're bad at is the next stage", after our brilliant ideas. "I should think it'd be rather depressing as an inventor".

It is these problems that the seed capitalists have tried to solve. The rest of this chapter looks at them.

5.7 Seedcorn Venture Capital

5.7.1 INTRODUCTION

An observer of the venture capital industry caught the sense that something is not quite working as it should:

> "It clearly has filled a gap". Small firms are getting backing they would not have, but "it is a case of missed opportunity" - they are now going to management buyouts - there was a sharp rise, but the small start-up sector has not increased in proportion, just stayed level as it were. .... So, "there is only a very small core of the hundred and twenty or so venture capital companies in this country who are backing early stage ventures", and an even smaller number in embryonic seed capital.

This section will try to pose the questions that surround seed capital, and look at the ways in which it is distinctive. The seed capitalist seems to live in a world much closer to that of the inventors and entrepreneurs described in the last chapter.

5.7.2 SIZE AND SCALE - THE ARGUMENT SURROUNDING SEED

A central question in this section is why there should be such an apparent shortage of seed capital, and does it matter? There are many complex issues surrounding this question - for example, the small size of seed capital may belie its significance, and what of the 'traditional' sources considered earlier? There are few seed funds, but people from the majority of them were interviewed to illuminate its problems - this section is theirs. One of them introduced the issues:

> "There is an argument to say there is tons of seedcorn about". If you add up all the funds they may only have ten million, "but if you add up individual savings, ... the family, ... my local clearing bank .... before

> people come to us, ... they may have raised anything from ten to fifty thousand". Then they may go to seedcorn, use BES, private investors. .... There are loan guarantee schemes etc..

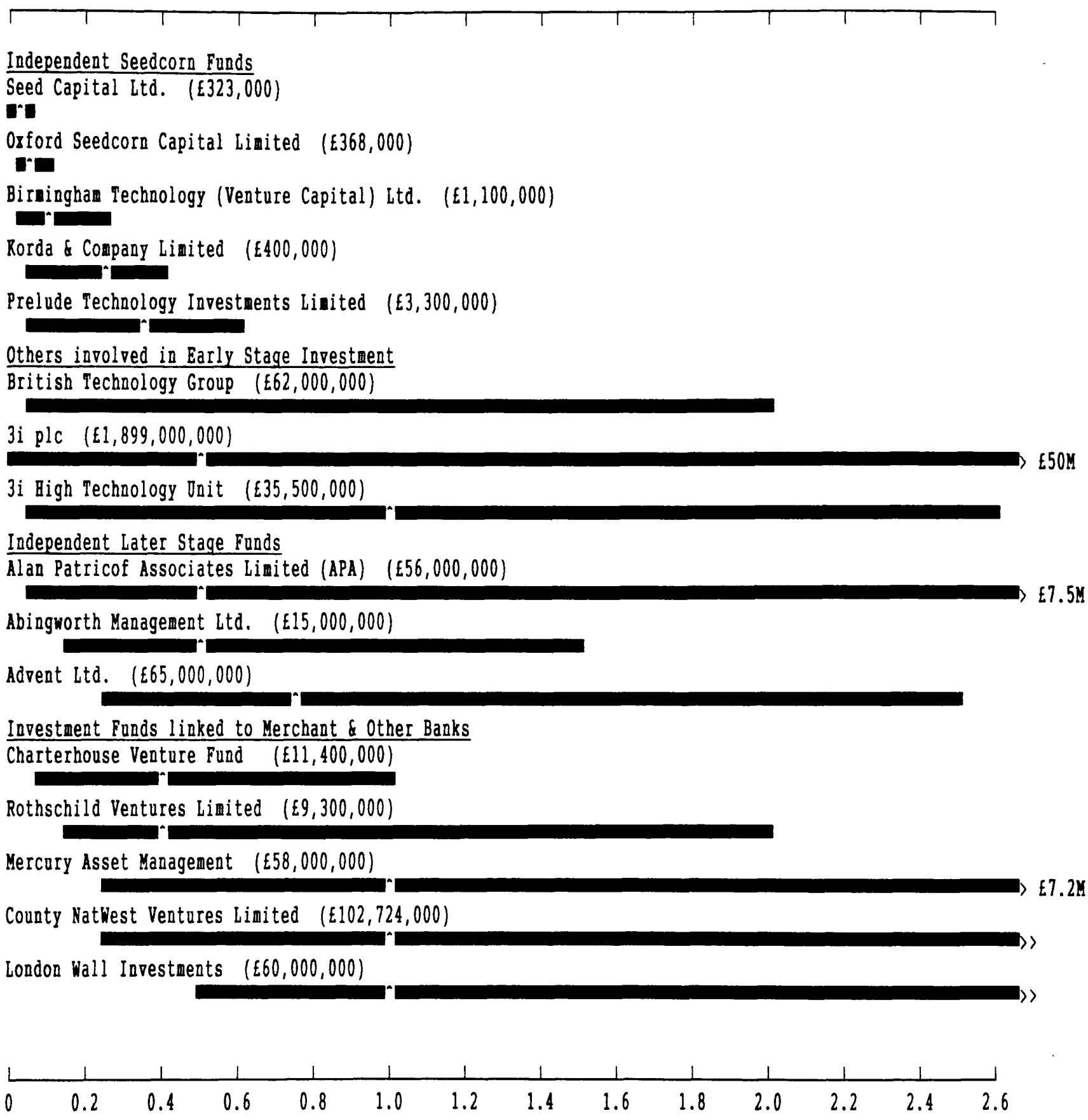
But he also observed, with evident feeling:

> "There should be fifty to one hundred .... like us; there are five to ten. ....It doesn't even scratch the surface."

This sub-section briefly develops these issues of size and scale. It is well to preface it by emphasizing the small size of seed capital. The following chart shows both the range and average size of investments, together with total funds invested, for all the UK venture capitalists interviewed. The distinctiveness of the seed funds is immediately apparent.

SCALE OF INVESTMENT OF UK VENTURE CAPITALISTS INTERVIEWED

Figures based upon data in Cary (1989), supplemented by British Venture Capital Association Directory (1990) and interviews. Figures in brackets indicate total portfolio of investments (at cost).



Seedcorn is clearly a small element in a bigger picture - just as venture capital itself is. But a lack of seed may have a disproportionate effect on that bigger picture. The rationale of venture capital is that it looks for ideas with a potential for growth far exceeding that of the average new enterprise. Technological ideas fit this prescription, thus it can be argued that seed capital is far more significant for future national economic growth than are more 'traditional' sources of early-stage funds. An expert observer of seed:

> "Seed and start-up are our major areas of concern at the moment". "There are significant deficiencies in the supply of seed" - but is it lack of demand, or just "a vociferous minority"?

The salience of these issues is emphasized by the degree of comment they attract, such as the Bank of England's (1990:82-83) excellent discussion under the heading 'Gaps in the Availability of Finance?'.

Seed capitalists themselves were in no doubt as to the value of their activities - moral as well as economic:

> The "wild passion of his" was new business start-ups - in his spare time at first - he took no money or fees, but a minority shareholding, in return for his "sweat and toil". In cashflow terms this is disastrous - he was starting something realizable in the long-term.
>
> "It just was frustrating to me as an engineer we had all these good ideas .... that no one was doing anything with". It is UK Ltd. that loses. ....
> "That's the end I wish to be in"; it is fun, there is altruism - "that was the original motivation" - no one else would have invested in them. .... "I wish these funds to succeed. .... The most useful thing I can do for the seed capital movement is to have a demonstrable success".

A very strong sense of purpose suffuses their words - it burns even more brightly when they describe what they would like to do:

> "I'd love to do it"; "actually to create something where something doesn't exist". "Proactive seedcorn, instead of waiting for .... people to come through the door".

The seed capitalists' zeal clearly went beyond most venture capitalists' enthusiasm for their work. The reasons people suggested for the lack of seed also stand in heightened relief - like the unhappy comparisons with the situation in the US that seemed a way of crystallizing a sense of moral unease:

> "The whole process seems to be more developed in the 'States". Peoples' attitude is just so much more optimistic". .... "There's a lot of responsibility on us to develop more of an entrepreneurial culture

Even though people in the US suggested the truth of such comparisons:

> "The view in the 'States is 'Goddamn it, I've got a good idea, ... I'm going to bring it to market and make a million pounds'".

The American attitude to failure and bankruptcy was often used as a ubiquitous symbol of superior US support for seed investment. There were many UK comments on this theme:

> In this country bankruptcy is "the mark of Cain. .... If you look at Californian entrepreneurs you see two or three bankruptcies before success".

Parallel to this thread of argument about seed capital and the support of innovation, was a similar thread, already introduced above, about the effects of size and scale on industrial innovation. People suggested that large industrial

institutions in the UK are ineffective at innovating:

> "The large companies cannot be as innovative as the small ones". .... "The people who join small companies often tend to be the better scientists, ... who want the freedom, ... lack of bureaucracy"; "with the potential for capital growth thrown in as a big advantage". Small and big need each other.

The burden of such comments is that large industry cannot handle small things. The evident corollary, already illustrated in earlier sections, is that neither can large finance, even venture capital finance, and it is precisely this point that the seed capitalists' arguments pick up:

> I saw that people needed twenty to thirty thousand pounds for the small engineering projects - individuals could not do it, no fees could be raised on such small amounts, the City were not interested for the same reason. For the professional venture capital funds the amount is much too small .... "So, nobody does them - that is the equity gap"

The equity gap was often mentioned as evidence of this pathology of size and scale within the world of traditional finance - a pathology that venture capital sought to remedy.

The equity gap is thus a symbol of the difference between venture capital and seed capital - the difference this section investigates. It is tempting to see the development of ancient banking, or Templar finance, or the nineteenth century merchant bank, as medicines for similar pathologies of the day. This might suggest that just as the merchant bank outgrew its origins in day-to-day trade, so venture capital has already outgrown the needs of individual people with ideas. Seen in this way, the equity gap is a dynamic thing - a sort of tectonic mechanism like the mid-Atlantic ridge. The

white heat of ideas throws up new financing mechanisms, but in so doing they grow apart and cool, leaving the gap still there. A happy simile, perhaps, given the symbolism noted in the discourse separating the 'new world' and the 'old':

> "I think it has to do with the mentality of the new world versus the old". "Americans are much more entrepreneurial, willing to take a chance on someone unproven".

5.7.3 PEOPLE OUTSIDE OF FINANCE

The human scale, and perceived moral basis of, 'the seed capital movement' is clear; so is a sense of seedcorn as a boundary area - the 'marches' of venture capital. At this boundary, people with funds meet people with ideas, and they are occasionally constrained to hurt one another. The interviews are rich in artefacts that seem to symbolize this boundary.

Peoples' views of inventors provide a good example. Inventors are seen as clever people, no doubt, but untutored in the ways of the world of finance and business:

> "The individual inventor, ninety-nine times out of a hundred, is going to find life very difficult" .... inventors are not the right sort of people to manage companies.

One stereotypical inventor was interviewed in the last chapter - no fewer than seven people mentioned Sir Clive Sinclair.

The 'entrepreneur' seemed another stereotype in UK interviews - there seemed little agreement as to what an entrepreneur actually was. One UK comment neatly summed up

the sense of a boundary here: "the "entrepreneurs speaking in French, the venture capitalists talking in German"." Another interview even noted the symbolic potential of the entrepreneur:

> There are not so many rôle models here, or maybe it is not publicized enough. It is "almost as though the establishment tolerates entrepreneurs as a necessary evil, whereas in the US they are seen as part of the economy".

US views indeed seemed different. A UK banker, turned US venture capitalist, caught something of both:

> You have to be flexible. "Unfortunately entrepreneurs aren't very good at that - .... they have this vision in their bathtub, ... and they get wedded to it". They do not change it to fit the market. "They need to be able to learn, ... and keep trying, ... and not be too demoralized".

The regular US view of the entrepreneur was more consistent, and sympathetic:

> The seedcorn venture capitalists add capital and management sophistication. Entrepreneurs add their engineering design experience, it is "very engineering intensive". .... The entrepreneur .... active as an engineer, marketeer or strategist.

There is more than a hint of blame in some of the UK comments - the 'necessary evil' view perhaps. This is nowhere more clearly stated than in the widespread comment on entrepreneurs as 'founders' of businesses:

> "my perception is that entrepreneurs in the UK are not of as high a standard. .... Entrepreneurs here prefer to have one hundred percent of nothing, rather than ten percent of something. .... That does not apply in other countries - at least not to the same extent".

The consistency of this complaint was impressive, and again, it did not arise in the US interviews. The frequency of such

comment may in part be explicable as an attempt to mitigate venture capital's perceived shortcomings. Nonetheless, even the seedcorn people, experts on founders that they were, raised it:

> "There's an attitude in this country: .... 'why should I give you any equity?'" .... this is unlike seedcorn in America. There, they will take two thirds, but provide everything - premises, marketing, etc., etc.. They are more likely to succeed - "a very much more healthy attitude".

An entrepreneur seems to need more than a burning desire to promote an idea - he must also fuse the languages of technology and venture capital. Even then, this new combined discourse is more acceptable in the US than the UK; as an American comment put it: ""A language which describes the process" exists here."

The problem with hunting stereotypes, is that they twist and change like will-o'-the-wisps. They are symbolic, not substantive, they allow people to visualize the boundaries between finance, technology, and business, in ways that do not threaten their sense of moral purpose. One final stereotype remains to be considered - that of the seed capitalists themselves. Idiosyncrasy abounds, but the one common factor seems to be an embodiment of multiple experience, a plural language, a mixed background:

> "We're all fairly technologically literate around here"; my first degree is electronic engineering, ... [two of us] also have MBAs - we are all primarily business people.
>
> "I don't really think it is that difficult - .... you do have to have a fairly thick skin", and the confidence to write the cheques. You have to be suited to it temperamentally .... "You've got to take decisions

> easily" .... You also need to have been an entrepreneur too.

5.7.4 THE SEEDCORN PROCESS

The question whether seedcorn is different can no longer be avoided. Seed capitalists do differ amongst themselves, but a community of views and aims also shines through, after all, they know each other:

> "About two years ago I thought we should have a meeting". Oxford Seedcorn, JMI, Birmingham Ventures, Korda, Prelude; we have had meetings here every six months since.

A return to the process of venture capital discussed in the last section allows the areas where the dissonant voice of the seed capital community was heard, to be highlighted.

The first part of the seedcorn process is finding people with ideas. Like the rest of venture capital, seed needs to publicize its existence, but not too much:

> It is networks. It is "absolutely no use putting an advert in the paper", you would get a room full of perpetual motion machines. We get referrals from our contacts, we target chartered accountants, ... we aim at bankers and accountants - we market ourselves to them.

However, differences of investment style - even a healthy degree of idiosyncrasy - start to appear when people describe how they assess these ideas:

> "It's a lively world", it moves fast. .... There is no point in investing five hundred thousand when all you need is five thousand. .... Seed ideas are not syndicated - there is no point with a few thousand pounds. I do not use lawyers for example - that is most unusual - I am the only one in England that does that. .... "You've got to be confident to do this sort of thing", you have got to

> make mistakes. .... The ability to move at speed in lawyers' terms can win, even if other offers are better

They ask the same questions as their later stage colleagues about products, markets, and of course people, but their willingness to invest in individuals was plainly different: "they are assessing teams, ... we look at individuals". The initial objectives of their investments were different too:

> "A seed objective need not be financial" - however ridiculous that may sound .... For example the objective may be to create a prototype. .... That is the seed process - it is risky, but the potential rewards are huge - trials lead to an order, lead to a market - so the objective is a technical one.

If the individual with the idea lacks management experience, then the investor has to supply it - as recurrence of the phrase 'hands-on' shows. Styles differed, but a willingness to get intimately involved still emerged as a key distinguishing feature:

> "We're as hands-on as you ever could be". .... "Other funds would just have to have given up", we could work as fully fledged executives. "We see that as controlling the risks".

US seed fund activities did not emerge as radically different, but there were a few hints that the boundary between seed and the rest of venture capital was more clearly drawn there. There also appear to be very few seed funds in the US. Another area emphasized in an American interview was the question of geographical preferences:

> We just did a San Antonio deal at incredibly low cost .... "they can't get the capital; so those of us who will go there get good deals at low prices". .... Californians sometimes refuse to drive more than sixty miles - DRIVE! .... In Texas, in San Antonio, "you have to change planes! Well, that cuts a lot of venture

capitalists out".

Exactly the same sort of comment arose in UK seed:

> We do not worry about distance like some people do. They limit investments to one hour's travelling time away - many of our's are four hours. .... Some people even think distance helps.

Seed capitalists, like the rest of venture capital, look for returns. If they are crusaders, they are, as the Templars were, entrepreneurial crusaders:

> "We want to stay in the seed business, not altruism, but because we think we can make money there".
>
> it is not purely altruistic - I get twenty percent of the funds when they mature in the mid 1990s - so it is long-term.

This leads to the question of exit. The seedcorn problem is that an idea, in growing large as they want it to, will exceed their financial capacity before it is a proven basis for a viable business. They have to stay with it, and this rather corrupts the fixed-life fund model so common in venture capital:

> We put in so little that "all we can hope to do is provide .... bridging equity finance". "We take the enterprise, project, ... or whatever", to a larger stage - the result "has to be attractive to the next round of finance". .... "Expansion absorbs cash - .... at that point we'd go out and help raise that cash - but not exit, ... we generally think we can make more money by not exiting".

All-in-all, the seed investor sometimes seems closer to those in whom he invests, than to the rest of venture capital.

5.7.5 SEED - INSIDERS OR OUTSIDERS?

The perspective seed offers on the rest of venture capital is the 'equity gap' from the other side. From a pure venture capital perspective, seed invests far too much time, and corrupts the model of the fixed-life fund because there is no clear exit route, other than more investment. This may explain the seeming dearth of US seed funds - the fund model started in the US, so a failure to apply it to seed may indicate a different mechanism at work - 'traditional informal finance'. Wilson's (1985:6) "vast network of informal venture capital" in the US supports such an inference. The true significance of the US 'entrepreneurial culture' may not be that institutionalized seed funds are easier to start there, but that they are unnecessary. The frequency of comment about fund structures amongst UK seed people is thus hardly surprising - they reap the problems inherent in the vehicle. But they are also the few people who have got around these problems - all had their own solutions, but they all basically amount to the same stratagem: ""it has to be subsidized by something"."

Solving the problems of seed investment exercises venture capitalists, commentators, and government agencies alike. The Bank of England (1990:82-83) are concerned - they review the debate. Venture capitalists are concerned. The seed capitalists are clearly concerned:

> What about the present climate? The BVCA has set up a seed committee, "I went to one of their meetings - that was enough" - I am not a committee person. .... The European Commission came to the same conclusion too; Korda wrote the proposals for that - a designated European Community seed fund can reclaim about half a

> millionish of overhead cost. .... The DTI loan guarantee scheme is good - it guarantees up to a hundred thousand, to fill the equity gap.

This thesis is concerned with many of these issues as well.

Seedcorn, then, is different, because it exposes not merely the pathologies of 'traditional finance', but the problems with the structures of venture capital's 'new finance' as well. But the interviews suggest that seedcorn also has a different character from the rest of organized finance. This was nowhere more evident than when seed capitalists talked about raising money for their own seed funds - trying to sell their own ideas. Suddenly they looked far more like the people with ideas in whom they, themselves, invest:

> "We went to all the wrong people; .... the City could not understand it", that we were investing our hands-on experience, as well as money, to reduce risk. They invest in amounts of more than a million - our whole fund size. "We then went to the venture capitalists with a slightly different story" - a way for them to get involved in seed - "we found quite a willing audience"
>
> I went to venture capital companies, but they turned me down. Then the Money Programme did a feature .... in February 1986. .... They wanted an interview afterwards with a venture capitalist and an entrepreneur - I recommended [someone] - in fact he had already volunteered. I also gave them twelve entrepreneurs - they talked to all of them, but said they were not articulate enough. So, on the Saturday they asked me to go up the next day - we were filmed late Sunday afternoon .... in the interview, there was [the man] whom I had asked for money - I steered the interview around - "I said the large funds could set up little funds - .... he says 'what a good idea'. .... Afterwards he said 'I'd better do it'". He later saw it was a good idea - he raised more money - so I never had to fund-raise money again.

In short, their stories make the seed community look like both insiders and outsiders to the world of venture capital finance.

CHAPTER 6 : FOUR DISCOURSES .... A TALE OF SIX BOUNDARIES

when god decided to invent
everything he took one
breath bigger than a circustent
and everything began

when man determined to destroy
himself he picked the was
of shall and finding only why
smashed it into because

(e.e. cummings 1969:60)

6.1 An Introduction to the Discussion

The last two chapters have presented diverse pictures of actual inventions and innovations. This chapter, and the two that follow, discuss the data and try to understand how all of its elements, and the people that told the stories, are linked. To do this, a tentative model will be developed. The theoretical discussion of chapter two is central to such an understanding - its various sections provide the theoretical grounding for this and the next two chapters. This one draws heavily upon the anthropological and symbolic boundary considerations of section 2.3, the next draws upon Giddens's sociology of section 2.4, and the last upon Derrida in 2.5. It is hoped that this arrangement will allow the model developed here to be seen in a clearer relief, unobscured by too much theoretical debate - it also underlines the way that any model must involve many other texts if it is fully to be understood.

The model emerges both from theory and from what was said in the interviews. This chapter, then, will selectively reintroduce theory from chapter two and data from chapters

four and five (along with a little fresh interview material and new theoretical discussion). It will also use the analysis outlined in chapter three - the appendices to this and other chapters make the specific elements of this analysis more visible, they show the crucial way in which the categories that emerged have informed the model presented here.

Before going further, a few comments on the status of this model must be made. What is proposed is not a predictive model, it is an aid to understanding - more in the nature of a map. A map cannot show where a journey will go, but it can show both where a journey has been and where it could never go. The people interviewed were constrained by this map of possibilities, but <u>they</u> chose where their journeys went - the next chapter will re-introduce the journeys of innovation that resulted, and try to understand more about the way such journeys combine agency and constraint.

As a model, however, a map is a little simplistic - the journey of innovation changes the map itself - the results of choice and chance are often irrevocable, they define a journey of no return. Chapter eight will therefore add time to the space of the map, in a final attempt to understand the journey from invention to innovation.

6.2 Four Discourse Communities

6.2.1 INTRODUCTION

The idea of discourse was discussed throughout chapter two and in section 3.10, where a working definition was quoted:

> Discourse is the social process of making and reproducing sense(s). (O' Sullivan et.al. 1983:73)

This thesis uses the term at a practical level where the various theorists seem to be in broad agreement. Durkheim introduced the idea of empirically observable moral codes, and of socially shared categories. Lévi-Strauss took up this idea of different codes constraining relations between cultures. Giddens discusses knowledge, and how "belief claims are ordered in terms of overall 'discourses'" (Giddens 1984:92). White (1979:82) says of Foucault: "Discourse is the term under which he gathers all of the forms and categories of cultural life". Derrida refers to:

> the moment when language invaded the universal problematic, the moment when, in the absence of a center or origin, everything became discourse (Derrida 1978:280)

Discourse, in all of these ideas, shares an uncontentious central element of shared social experience. Discourse's basis in language and cultural symbolism is also plain, as is its implication in, and reproduction through, social interaction. As Cooper and Burrell, already quoted in chapter two, say:

> In the organizational context we are faced with a set of primary data constituted by programmes and technologies. These are essentially groups of discourses belonging to

> different systems of rationality[1]
> (Cooper & Burrell 1988:107)

This thesis uses discourse as a practical analytical tool. The rest of this chapter teases out various discourses from the interview data - discourse is thus used, not as some arid structural concept, but as a link between the 'real' people of the interviews and their social systems. Cohen's (1985) discussion of community (section 2.3 above) provides the practical basis for this project, 'community' emphasizes the human element in 'discourse'. In a community:

> its members make, or believe they make, a similar sense of things .... and, further, ... they think that that sense may differ from one made elsewhere.
> (Cohen 1985:16)

The interviews evoked a strong sense of community; not only that, people used the very word itself, often:

HOTOL INTERVIEWS

There is a "vast gulf between the technological community and the Lords, and those motivated by finance like the present government and the city."

"The very fact of his appointment was seen by the space community .... as an indication that Britain was taking the thing seriously"

'this technology is or should be of interest way outside the aerospace community'

"tail end of retiring '60s community"

You have to use the back-door safety net via the research community.

---

[1] The discussion of Latour in the next chapter will consider the problems of relativism inherent in any debate combining truth, rationality and discourse.

"The whole technical community didn't deserve any more."

"that would come out of the intelligence community"

FINANCE INTERVIEWS

the firms and the inventor's community

"As you go round the venture capital community"

The money comes from the finance community

"the ideas that the venture community are looking for"

it started in the "venture capital community".

"the venture community got sucked into it"

We started the BVCA eight years ago - the community has grown a lot since then.

we work within the financial community

I hope I speak for others in the community.

Many different communities are evident here, but few of them coincide with the formal organizations that are the usual focus of management research. This is why the idea of discourse is so useful - it consists in different 'ways of talking', not in any organizational grouping. Analysis could proceed by ascribing a different discourse to each of the multifarious communities in the data; however, many of these discourses would look very similar. A financier talking about finance theory sounds remarkably like an engineer talking about thermodynamics; both use a technical discourse - only the details differ.

A better course is to try and arrive at a minimum set of discourses. Four discourses emerged here, and the model they allow provides a framework that has proved very useful in understanding innovation. It is important to emphasize that such a framework can in no sense claim to be absolute or fundamental, nonetheless it is more than purely relative and arbitrary. Other frameworks could no doubt have been chosen, but the very fact that such a framework draws in all the interviews, goes back to the time of Parsons, and extends across the Atlantic does suggest that it might have more than a purely local utility.

The four discourses have been named 'Moral', 'Industrial', 'Financial', and 'Public'. Each one is very broad, but the words, attitudes, and senses each encompasses still seemed more similar than they did different - they made semiological sense as analytical tools. This section starts by reviewing them, the rest of chapter then illustrates their empirical justification by looking at the evidence for their boundaries one with another.

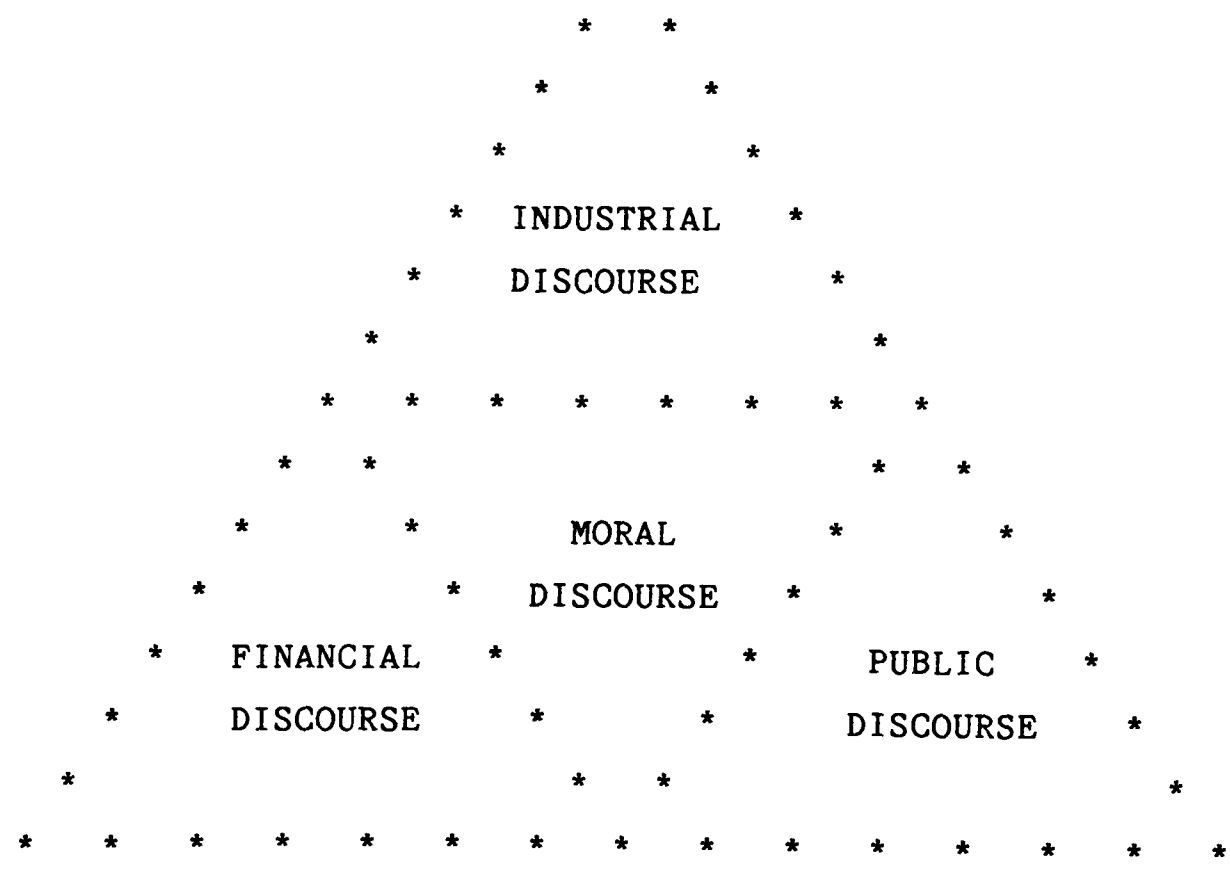
Their boundaries appeared through the symbols and words that seemed to 'stick out' from the texts of interviews. Sometimes several boundaries came together in one fragment of an interview - the venture capitalists' programmatic descriptions of how they evaluated new ideas provide a rather obvious example with which to proceed:

There are three 'm's - management, market, and .... money

> "It's the cliché". "It goes people/technology/markets in my view" - do not "get carried away by the gee whiz syndrome" into just backing technology.

This shared way of making sense of a chancy world seems to define the very discourses themselves - if management resides in industrial discourse, money in finance, and markets in the domain of public opinion, then the 'gee whiz syndrome' surely evokes something of a moral order.

A graphical picture of the framework is a useful way to end this introduction - it is best conceived as four triangles. Imagining them cut-out and folded into a three dimensional solid (a tetrahedron), emphasizes the vital property of this model - that each discourse has a boundary with every other:



6.2.2 MORAL DISCOURSE

Durkheim's ideas of categories of knowledge and moral codes (section 2.2) come together in the idea of moral discourse. A moral code - for Durkheim the 'non-contractual element in contract' - catches the way people explained actions in terms of their moral justification, not their organizational effectiveness. People's sense of right and wrong shone through their words, and it is this that 'moral discourse' in intended to encompass. The way moral discourse is used here implies no ethical judgement of peoples' actions - it is purely an empirical tool that, following Durkheim's lead, treats moral facts as observable phenomena.

Moral phenomena are often to be observed in the interviews:

> I have had a burning drive towards spaceflight since childhood.
>
> "An awful lot of HOTOL work has been done at home". There is competition overseas, "Crikey, you've got to keep working, or they'll all catch up".
>
> "an inner circle of space enthusiasts who, as they disappeared under the table in the pub, jotted down some notes"

When people associate their 'work' with the home or the pub - the usual social sites of 'leisure' - they symbolically set it apart from the normal organizational conception of work. This again emphasizes that discourses are much more subtle analytical tools than organization - a discourse evokes a shared sense of the world, not a shared location.

Moral discourse is plainly evident in words like 'burning drive', 'passion', 'altruism'. The last quotes were from

HOTOL - the following come from finance:

> The "wild passion of his" was new business start-ups - in his spare time at first
>
> "It just was frustrating to me as an engineer we had all these good ideas .... that no one was doing anything with". ....
> "That's the end I wish to be in"; it is fun, there is altruism
>
> "I'd love to do it"; "actually to create something where something doesn't exist".

6.2.3 INDUSTRIAL DISCOURSE

If Durkheim provides a theoretical introduction to the idea of moral discourse, then it is perhaps his sociological predecessor, Saint-Simon, who is the father of industrial discourse[2]. In many ways, organizational discourse might be a better term, but this name has been resisted because the tradition of management scholarship, within which this thesis sits, has many different and more precise uses for the word 'organization'. Industrial discourse is not meant to describe people espousing some management theory, but people talking in a way that evokes the procedural, logical, and above all organized character of the application of science to modern industry.

---

[2] The sociological baton was arguably taken up by Weber, whose ideal type 'bureaucracy' stands in a similar relation to the industrial discourse, posited here, as Durkheim's moral codes do to moral discourse. However, Weber's term is widely misunderstood, thus definition of a shared 'bureaucratic discourse' could have led to confusion. MacRae's (1987) explication of Weber's ideas is pertinent here.

Many of those interviewed were associated with the sort of large organization that industrialization brought about - it is hardly surprising that industrial discourse emerged from the interviews. Industrial discourse needs little introduction here, save to offer a few examples, and refine what it is taken to include in its present usage.

Charles Parsons's description of the epic story of the introduction of marine steam-turbine propulsion, in an 1897 lecture, seems a wonder of under-statement - it is a good example of industrial discourse:

> In January, 1894, a syndicate was formed to test thoroughly the application of the compound steam turbine to marine propulsion .... The fulfillment of these anticipations was, however, much delayed. (Parsons 1897:8)

There are many other examples of industrial discourse in the interviews, it is often most noticeable in subsequent written comments. In many ways, people seemed to see it as the 'proper' way of talking about industry, as a reassurance that it really is a scientific enterprise perhaps. But if such language could reassure them, it could also frustrate them, as Alan Bond's description of his departure from the UKAEA shows:

> Culham was more frustrating than SKYLON work at the end - you had to account for every last piece of work with the accounting systems.

Industrial discourse - this procedural view of organization - covers a wide swathe of human activity. It pervades industrial organizations, financial ones, the civil service and the armed services. The definition proposed here embraces all these areas - Fisher's descriptions of his Navy,

and financiers discussions of their organizations, can illustrate industrial discourse just as much as descriptions of Rolls-Royce or British Aerospace can.

6.2.4 FINANCIAL DISCOURSE

A strong sense of 'community' emerged from interviews with the people in finance:

> "Ultimately venture capital is a cottage industry"
>
> "It's a fairly small industry, and a fairly friendly industry too", a friendly and chatty one.

The boundaries between finance and the other discourses were distinctly drawn, financial discourse illustrated the character of discourse as a shared language even more clearly than the other three did. A venture capitalist in New York caught this with the remark: ""A language which describes the process" exists here".

Financial risk is a good token of this shared language - it seemed to be a symbol that stood for the unquantifiable in other discourses - a way of categorizing, and so sharing, the unknown, and linking it to other financial symbols like profit and money. Risk was mentioned quite often. The important point is that such symbols allow people who share financial discourse - even though they may have divergent goals, aims, and beliefs - to talk to, and understand, one another because those goals, aims, and beliefs, are all expressed in the same terms. A few quotes make this point well, they show perceived divisions between different areas of finance, but divisions

set within a broader shared view of the world:

> So there is both competition and happily working together - but it is a very small industry - you have to get on with people.
>
> In the main, venture capitalists are "bankers in another guise"
>
> "The buyout folk have financial engineering skills, the venture folk have operating skills" - but each needs the other.

6.2.5 PUBLIC DISCOURSE

Public discourse is the broadest of the four. In a way it is antithetical to the other three - they all express a sense of 'them and us', but public discourse reflects people's perceptions of 'us as a larger community' versus 'us as many smaller communities'. It is present in popular ideas like 'the public domain', 'public opinion' or 'the man on the Clapham omnibus'.

Public discourse is used here to suggest a language that is shared by the wider collectivity that the smaller communities make up - a discourse where collectively espoused values and norms can emerge that impinge upon those smaller communities. It is a medium through which individuals and communities can further their own values and norms in society.

The interviews revealed public discourse in the many different ways of expressing collective opinion that emerged. 'Public opinion' is the most obvious, and least definable of

these, but public discourse also comprises 'government' as an expression of a collective will, as well as 'the market'. All of these ideas are treated together as public discourse because they all share the one key element of people stepping outside of their local discourses in order actively to influence 'outsiders'. Public discourse might thus be caricatured as the rhetorical expression of the other discourses within a wider collectivity.

An example is a good way of illustrating this, and one aspect of the News-at-Ten's revelation of HOTOL, described in chapter four, perfectly encapsulates the capriciousness of public discourse. Frank Miles, the journalist most closely involved, told how the notion of HOTOL: "cutting the time to go to, say, Australia to a matter of some forty-five minutes" arose. This had a very clear symbolic significance - it greatly influenced the public perception of HOTOL, and those involved plainly used it rhetorically:

> "the excuse that DTI use is that it has been represented as ninety minutes to Sydney - so they say you should be talking to British Airways .... - absolute balls"

Frank Miles explained the prosaic origin of HOTOL's Australian connection:

> A word about the claim "London to Australia in 45 minutes" which has got stuck with the HOTOL story. .... I had written the script and asked for David Chater to present it on the air. When he read over the script before going into the studio he came to a point where I had written that HOTOL would "orbit the earth at 5 miles a second". ....
>
> David wasn't convinced and asked "Well, how long would it take to go round the world?" "About 90-minutes," I said. .... "So," said David, "we could get to Australia in forty-five minutes?" We were approaching News-at-Ten and the argument was holding up production -

> so I agreed. The claim has since appeared almost every time HOTOL has been mentioned and it's even appeared on the official literature produced by British Aerospace! (Miles)

Public discourse acts as a rather wayward and uncontrollable magnifying lens that can enlarge an invention into an innovation, but it can also make a mountain out of a mole-hill. Its importance to innovation is one of the key observations of this research - something that Fisher also understood:

> public opinion has far more weight than the strongest representations of even the fighting Admirals, and 3 lines in a newspaper produce more effect than an ultimatum from 'the Admiral in command of the Mediterranean' (MI:186)

6.3 Six Boundaries

6.3.1 INTRODUCTION

The four discourses were most clearly seen through their boundaries with one another - this is implicit in Lévi-Strauss's ideas of different linguistic codes which, Boon says:

> establish terms of exchange among different social divisions and cultural categories; and they constrain the possibilities of translation across languages and cultures (Boon 1985:169)

This section looks briefly at each of the six mutual boundaries that four discourses imply, and shows a few of the cultural artefacts that marked them. These discourses are clearly implicated in the process of innovation, but it is

well to remember that innovation only happens through the human action that takes place within them. This chapter can provide a framework for understanding, but before innovation can be understood the framework must be animated - the next two chapters try to do this.

It is also worth noting that new ideas can arise in any of the discourses. The lone inventor tends to inhabit moral discourse, but other innovations have their seeds in financial, industrial, or public discourse. Innovation is a messy business - ideas may look for support anywhere, as the interviews repeatedly show. Words that seem to signify the existence of one discourse often contain more than a flavour of another, and it is important that this subtlety is not lost in what follows, through too structural a view of discourse.

What follows in this section, then, is a brief selection of illustrative examples of the boundaries between the discourses. If a fuller demonstration is required, then a return should be made - armed with the four discourses - to the material of the last two chapters

6.3.2 THE MORAL-INDUSTRIAL BOUNDARY

Conciseness is a problem in seeking examples to illustrate this boundary, there are so many from which to choose - a very few must suffice. One of the most striking symbols associated with this boundary is that of madness:

> "I explained it to him, he said '.... you're off your mind'" - 'you're mad' - "'leave the mad, hairbrained, schemes to us'" - "'it's not going to work!'"

A collage of phrases from HOTOL interviews show the strength of this symbol:

> "drifting to megalomania"; "a bit of a Dan Dare"; "some of the queer things we've never had to do with aircraft before"; "a weird cycle"; "one of seven or eight funnies in the organization"; "these crazy ideas"; "HOTOL got in through the back door"; "if he's someone who's a bit nutty like me"; "this hair-brained scheme".

It is easy to see that anything not conforming to the logic of industrial discourse might be symbolically termed 'mad', but what is interesting was the use of 'madness' in moral discourse too. The inventor of HOTOL himself refers to its "boy wonder aspects", and Fisher comments in letters:

> everyone thought me a lunatic for developing the submarine (MII:494)

> Everyone thinks I'm mad! <u>but really I'm not!</u> (MIII:161-162)

This 'madness' in moral discourse can be interpreted as a playful way of claiming licence to transgress the regulated norms of industrial procedure - a symbolic acknowledgement of their force. John Scott-Scott's comments on the entry of HOTOL to BAe Filton perfectly sum this up:

> "they were all a bit daft. ..... Frank Crowfoot - a nutter - he made the first model of HOTOL", "he made a little man, said 'don't forget this is a big machine'", it is to his "eternal credit"

Symbols like madness may be salient on both sides of the boundary, but there were also stark differences of view between the industrialists and the 'enthusiasts'. Words from John Scott-Scott, when compared with another HOTOL supporter in Rolls-Royce, show this:

> "It wasn't respectable in the early days" .... "how could this have grown up with two blokes at home?". It "generated a lot of resentment".

> "It came into the company by a completely unorthodox route .... it was born in the vestry"

These very different views of same thing make the boundary very clear. But such differences are also more substantive, as Peter Conchie's words eloquently indicate:

> "The traditional engineers come to meetings with books", try to write it all down; I remember one from Warton in one of the early meetings, the "poor guy got lost" - Bob Parkinson would say one thing, Alan Bond would say: yes, but: John Scott-Scott would throw his ideas in - "they talked a different language, ... ideas could be bounced off with those four".

Further illustration is scarcely needed, but it is hard to resist adding one example each from Parsons and Fisher. Fun is not a word that is commonly acceptable within industrial discourse, and Lord Rayleigh shows something of the moral-industrial boundary when he records Parsons's comment: "We have now made a bit of money, and deserve to have some fun". Rayleigh also records an earlier transgression of the boundary.

> The directors were not perhaps predisposed in Parsons's favour by his habit of experimenting with rockets, which sometimes resulted in shattering explosions under the windows of their luncheon room.
> (Rayleigh, in Parsons 1934:xx-xxi)

Fisher, in a letter to no less than the leader writer of 'The Times', combines (as only Fisher could) his naval discourse's need for secrecy with a blatant, though morally zealous, transgression of it:

> THIS IS ABSOLUTELY BETWEEN OURSELVES, I have received a secret letter cancelling all previous letters and substituting a fresh set of instructions in preparation for war (MI:198)

6.3.3 THE INDUSTRIAL-FINANCIAL BOUNDARY

Once again, there is far too much illustrative material here to make selection easy. Some comments were simply rhetorical descriptions of a boundary:

> "none of it cuts any ice with the money-bags people"
>
> "there's something wrong there. .... It stems from a cultural difference - .... the city doesn't understand its customers".

Other comments contained symbolic artefacts - one of the commonest being the claim of 'short-termism':

> "One of the big problems we have in this country, not in Japan, is the short-termism of the City" .... That makes innovation difficult.

On the face of it, this is a claim that the City takes little note of industry's need for stable long-term development. The problem is that those in finance seemed to mention short-termism as often as those in industry, and, in any case, the economic evidence of a 'real' effect is sparse. Short-termism appears as a symbol of difference between communities, far more than as a reasonable explanation.

The interviews gave access to a rich vein of personal experience, and the boundary was sometimes clearly apparent when people described this:

> in a meeting with City people "I would announce I was an engineer - and the temperature would drop" - instantly.
>
> "the biggest problem has been a lack of commercial awareness/management among those who have good technical ideas"

Even after intimate experience of finance, in the course of a management buyout, the boundary remained:

> Most engineers do not know much about the City - it is a black art - "the people we met were charming, very fine people, they remain our friends"

The same man went on to describe, and symbolically to rationalize, the practical procedures through which his financiers monitored him:

> "They were like iron - .... they would go through those management accounts with a fine tooth comb". .... - "their main interest was their reputation". It is a City characteristic, "they can afford to cover losses, they can't recover a tarnished reputation".

This delicious association of 'management accounts' and 'reputation' bespeaks a rich symbolism behind the instrumental façade.

A technologist encapsulated the feeling of a shared boundary when he observed: "One of the challenges is breaking away from the 'them and us' idea". Each side needing the other is perhaps the most distinctive thing about this boundary, the symbols arise because people also need to maintain their distinct identities. When the boundary has to be straddled, ideas from the language of 'financial discourse' like risk take on a new symbolic significance and allow translation. Two quotations well illustrate this, one from Charles Parsons in 1893, the other from a modern venture capitalist:

> I have in all cases clearly pointed out that there is considerable risk, but that I consider the matter as sound as can be seen without trial on full-sized scale, so that no one can blame should anything happen. (A:101)

> risk falls as a prototype works, pre-production succeeds, the market survey is good. So, as the risk falls, the

> investor will accept a lower return. But it is not scientific

It is worth noting in conclusion, that however clearly this boundary emerges, the other two discourses are not far away - for example, discussions about risk were never far from the wildwood of the unscientific:

> "The problem of playing with numbers .... is that you" can fool yourself you are looking at reality - in fact "they will be all over the place". "It's a real guessing game, ... balancing a number of imponderables against a bit of paper with numbers on".

6.3.4 THE MORAL-FINANCIAL BOUNDARY

> I ask you to further consider the position of the inventor - the man anxious to achieve success where others have hitherto failed. To be successful he must be something of an enthusiast; and usually he is a poor man, or a man of moderate means, and dependant on others for financial assistance. Generally the problem to be attacked involves a considerable expenditure of money; some problems require great expenditure before any return can thereby accrue, even under the most favourable circumstances. In the very few cases where the inventor has some means of his own they are generally insufficient to carry him through, and there have unfortunately been many who have lost everything in the attempt. In nearly all cases the inventor has to cooperate with capital
>
> (Charles Parsons - Presidential Address to the Engineering Section - British Association - Cambridge 1904 (Parsons 1934:46))

Parsons's comments convey a strong sense of the problems in financing innovation - a sense of a boundary. It was precisely these problems that led to the investigation of finance in this thesis. Madness was again a symbol here, as it was with the moral-industrial boundary. However, this time

it was confined to those on the finance side, and seemed to be used a little less playfully. A page of examples could be presented, but a handful will give the flavour:

> "Somewhere out there there are probably thousands .... of lunatic deals - .... get financed by uncles and aunts".
>
> you "get all sorts of nutters writing in". It is a delicate balance, you cannot be seen to put up hurdles.
>
> "We certainly don't advertize - .... you get completely swamped by wallies, ... the bathtub invention syndrome"
>
> "a bit of a mad scientist, ... a bit of a joke character"

The Archimedean stereotype of the inventor in his bathtub was also an image that arose a number of times.

There is an historical dimension here, many of the dichotomies of finance unearthed in chapter five have moral overtones, and interviews sensed these at times. For example, the very word 'equity' evokes a moral context, the more so when the nineteenth century debate of the ethics of limited liability is remembered - or indeed the mediaeval distaste for usury. Other dichotomies have similar force - like the subtle distinction between investment, insurance and bookmaking that emerged in some of the interviews - when does an investor become a punter?:

> "There are a lot of philanthropic individuals who will <u>punt</u> bits of money at people"
>
> "There are more individuals in the US willing to take a <u>punt</u> - .... the 'angels'".

The moral character of informal finance is beautifully caught by words like 'philanthropy' and the soubriquet 'business angel'.

Financiers' stereotypical views of the 'inventor' emerged in profusion from the interviews - one elegant example will suffice:

> "This is what the inventor is always worried about - it's like a composer - .... it has to be played for someone". Money, for "the true inventor, comes low in the scale".

This, perhaps, encapsulates the boundary at its most basic - the 'inventor' is driven by a moral imperative, <u>not by money</u>. This observation is forcefully illustrated in the historical material - both a ruling over Parsons's patents, and a Fisher letter of 1910, make the same point:

> Mr. Parsons executed all the work for the company at net cost, and without making any charge for his services. (A:86-88)

> 21/06/1910 I was offered practically <u>£10,000</u> a year .... and the certainty almost of being a millionaire. I declined, because I wanted to be Commander-in-Chief in the Mediterranean, Commander-in-Chief at Portsmouth, and First Sea Lord, and work a revolution! (MII:328-329)

An example from seed capital raises the interesting question of whose side of the boundary it is on: "he took no money or fees, but a minority shareholding, in return for his "sweat and toil"."

There were many direct references in finance interviews to the problems of this boundary, usually with a rhetorical flourish:

> "It's very confusing for the small person who is not used to complex financing" ideas. "The biggest problem .... is knowing what door to knock on".

> the "entrepreneurs speaking in French, the venture capitalists talking in German".

> "Most inventors think people with money are a spiv", financiers think inventors mad; "both are probably right". They "come from different backgrounds".

The boundary was also clear when personal experiences were recounted - as in Alan Bond's despairing words:

> Despite eloquent arguments - they could not fund it - "those were the rules of the game". "Some days we'd go off quite optimistic, ... but we'd still end up round the pub getting blitzed".

However, discourses intermingle - moral discourse is not the preserve of the 'inventor' alone, financiers eloquently bared their moral sensibilities as well, as two long extracts show:

> "There has to be a way". "There's always going to be a flow of brilliant .... ideas that will work". "The problem is spotting it" .... "What we're bad at is the next stage", after our brilliant ideas. "I should think it'd be rather depressing as an inventor".

> "There are lots of reasons ideas don't get to the marketplace - .... it is not because of lack of money - I honestly, honestly, believe that. .... There are oceans of money about in the City". .... "The original ideas are very fragile, very fragile indeed. .... A very unscientific area. .... The propensity for people to have an idea, ... to go out and do something about it". .... "People look at their ideas through rose-tinted spectacles". .... People see their ideas like children. It is an interesting psychological problem. "Those ideas are often the child of people who don't have a view of the market " - very fragile - the papers get hold of them and do not understand that. .... "The fragility of ideas"

People very evidently care. The 'equity gap' emerged as something of a symbolic scapegoat here - an explanation for the paradoxical problems of internalizing moral discourse within financial discourse. The seed capitalists eloquently illustrated this paradox when they retailed their experiences of looking for their own funds:

> "We went to all the wrong people; .... the City could not understand it", that we were investing our hands-on experience, as well as money, to reduce risk.

One even described his activities in the a moral context of a 'movement':

> "The most useful thing I can do for the seed capital movement is to have a demonstrable success".

This debate shows the promise of the proposed four discourse model. It suggests why seed capitalists look more like inventors than financiers. It shows how financial discourse's assertion that seed makes no economic 'sense' may be re-interpreted as an accusation that they have, in fact, been co-opted by the moral discourse of their investees. It asks - is the 'equity gap' real or symbolic?

6.3.5 THE MORAL-PUBLIC BOUNDARY

The case studies abound with examples of people's attempts to sway government, or publicize ideas - all testify to the boundary between moral and public discourse. Two cases will serve as examples here - Fisher and HOTOL.

Even a random selection from Fisher's letters would probably show his manipulation, through the press, of the public, the Admiralty, and the Government. A 1901 and a 1910 letter illustrate:

> I have no doubt a little 'stiffening' from outside in the shape of one of those unmistakable 'do-your-duty-or-you'll-catch-it' leading articles in 'The Times' will help him (MI:179)

> <u>Without the Press it couldn't all have been done!</u> It may not be politic to say this, but it's true. (MII:306)

Another 1910 letter shows the ubiquity of publicity as a political tool - it was used against Fisher too:

> A Navy scare is the Tory sheet-anchor! at judicious intervals. That pestilent cad, Sir William White, is, I fancy, Providing Winston with ammunition against the Navy Estimates. (MII:345)

The originators of HOTOL shared Fisher's sense of the need to present their ideas to Government in the right way:

> "You've got to work this up to a fairly high level before the officialdom of the world allow it to become respectable". You have got to really understand the show stoppers, you have got to be "able to say 'I firmly believe it could be solved by ----', that conviction carried a lot of weight".

The consequences of the presentation of HOTOL by Frank Miles and ITN have already been told in chapter four and earlier in this chapter. They will be raised again, but Miles story is worthy of mention here for the way in which it mixes moral and public discourse - particularly when Miles described his own feelings:

> We went on the air. I remember sitting in the control room - I felt dreadful - I worried if any of the people I had spoken to, and any of my informants, would get hurt - you run it because it is in the public interest, but you worry.

The peculiar character of the boundaries of public discourse is clear in all this. Crossing them is irrevocable, there is no going back - it involves a rhetorical presentation of moral belief that lays a person open to attack without the possibility of retreating once more behind the symbolic palisade of the boundary. Alan Bond's public statement of his willingness to risk prison by infringing the Official Secrets

Act to further HOTOL is a striking example - his ideas were under attack from the public discourse of government itself:

> 'I have been in prison for the past five years with this project', he told ITN news last night. .... 'I am not going to stand by and watch the Government sit on something which is going to introduce a modern industrial revolution comparable to that of 150 years ago' (The Times 13-10-1987)

This also very clearly shows the innovator's dilemma - sooner or later his idea must enter public discourse, in order to find support, funding, or customers. Geoffrey Pattie put the dilemma very clearly:

> "If you have conceit enough to have vision .... then you have to set everyone else on fire, ... otherwise the danger is you go on burning in the corner".

6.3.6 THE INDUSTRIAL-PUBLIC BOUNDARY

This boundary has many facets - first is the relationship between a company and its markets, influenced as they are by public opinion and fashion. Second is the relationship between industry and government, with all the questions of political manoeuvring, and policy that this raises. Third are the effects of publicity, and its manipulation, in all areas that industry enters.

Marketing and government policy are frequent topics of discussion both scholarly and popular, and are therefore not developed here. Publicity, however, is considered far less often in management debate, particularly its wayward character. This sub-section will therefore concentrate on publicity.

Chapter four provides a fine example of the clash of industrial and public discourses - the ITN revelation of HOTOL[3]. The reactions of the companies, especially Rolls-Royce, to the News-at-Ten report made its importance abundantly clear ("No-one at Rolls-Royce knew -"John Scott-Scott was kicked all over the universe". Tombs made some very silly statements"). It is instructive to delve beneath the surface and look at how those most closely involved saw this event. Frank Miles had unearthed the story independently of BAe, although his visits there had given him vital clues. Nonetheless, Peter Conchie, the relevant director, was quite happy to claim tacit responsibility for the embarrassing 'leak':

> "Frank Miles" .... had an embargoed press release, but he also had an informal source tell him about it, so he felt it was legitimate to use the story early.

This acceptance of responsibility seems rather perverse - had Conchie openly released details of HOTOL it seems likely that he would have drawn the wrath of Rolls-Royce that, in the event, fell on Miles. Why, then, not keep quiet (even to an academic researcher)? After all, Miles had already served BAe's ends, 'woken the world up', forced Rolls-Royce to 'step up to the line', whilst shielding them from the negative consequences. In addition, Frank Miles denies BAe people as his sources anyway, and refutes Peter Conchie's claim:

> Peter Conchie has claimed that the information I had on HOTOL was embargoed - I would like to make it very plain

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[3] Parsons at the Spithead review is another.

> that it was not - British Aerospace had not given us any information on HOTOL.

It is Conchie's very evident acceptance of the benefits of publicity that gives the clue to his reaction here:

> "My job all the time has been concerned with international and PR" .... "It's not something you can have a fixed view on", you do not want publicity all the time; it's give and take on each side between the company and the press. You "can't be sure you're right" anyway. "The whole thing is consistency" - if they see you are consistent the "press won't drop you in it". HOTOL is good for BAe's image

Conchie's words straddle the boundary between public discourse and the industrial discourse that would ostracize his self-acknowledged action. The paradox of publicity's double edge explains this action. Conchie, like Fisher before him, clearly <u>needs to see publicity as more under control than the events suggested that it was</u>. Industrial discourse deals in control, public discourse in rhetoric and influence. Publicity is the touchstone that separates control from influence, and that here separates management (to which it is inimical) from action (which it often catalyses). Others, influential within British Aerospace, seemed to grasp this subtlety too:

> Most people, particularly the young, the under 25s, think in terms of space, of Concorde. If they had to vote "the lad or the girl on the Clapham omnibus" would support it. You could generate pressure that way, but that is "not really the right way to go", you "need to feel your way forward, step by step"
>
> "I spent a lot of time talking to politicians, it was no good talking to the City". I would talk to the Government aviation committee, the Opposition aviation committee, to the ministers, Geoffrey Pattie, the guy from the Financial Times, journalists - encouraging them to write articles, appearing on television - they try to trip you up there - to try and get a point across.

Frank Miles - the intermediary - saw the importance of his publicity as a <u>bridge</u>:

> I later got to know Alan Bond, and he said in a letter at Christmas that, without ITN, HOTOL would have got no further. Cliff Nicholas of BNSC later said that the publicity was important too. .... Publicity is very important in these things - we are the bridge with the public - we get an audience of twelve million - the quality dailies only get three million.

The interview with the Minister very clearly showed the effect publicity can have on a new project:

> Tombs and Lygo "would tend to" do it themselves, it would end up getting reported, "I read the newspapers, then I ring them up, ... they having sponsored the article themselves - .... clever, clever".

6.3.7 THE FINANCIAL-PUBLIC BOUNDARY

The intention throughout these sub-sections has been to illustrate, rather than comprehensively present, the evidence for each particular boundary. It is hoped that this has empirically demonstrated the utility of the proposed four-discourse model. It seems appropriate to use the financial-public boundary to summarize the different ways in which the boundaries have emerged as salient in people's discourse.

There appear to be three main ways. The first is a quite straightforward acknowledgement that things are in some way different on the other side. The second is a calculating and instrumental attempt to use the resources of the other discourse. The third is symbolic use of the difference as a comfort, or a shield, or a scapegoat, in order to protect against some perceived ill.

The first of these - acceptance of the simple objective reality of a boundary - is perhaps the most obvious, and requires the least discussion. The Director-General of the Government agency set up to co-ordinate the financing of space activity in the UK provides a good example. He outlined the boundary between his public rôle and the finance he provided in the most instrumental of terms:

> "My wish was, ... within BNSC, to keep about fifty percent of the action" - you cannot keep involved unless you put money in - "you have to pay for it".

Stock markets are another good example of the ordinary ways in which finance crosses into public discourse - as the very phrase 'going public' betokens.

The second way that boundaries emerged was as hurdles to be crossed, or across which to exert influence, in order to gain access to some benefit. People in finance sometimes sought, and used, publicity to further their particular aims:

> I was struck at first that "venture capitalists were clearly the most sophisticated users of the media".
>
> I had needed a way of getting my needs onto hundreds of people's desks. .... So, "in an attempt to get the City to take me more seriously I gave a lunch, it cost me a vast amount of money, at the Savoy - for two hundred and fifty people".

They also acknowledged the use of public discourse by others seeking finance, and their problems:

> I see some invention on the morning news; "my wife believes it, my kids do, my neighbours believe it, ... [but] would I mortgage my house for that guy?"

The final area where boundaries outcropped was, in many ways, the most fertile area for their analysis, and the most clearly related to the semiotic theory that has influenced this thesis. This chapter ends with a curiously persistent set of symbols that were common in the interviews - the comparison of UK financial practice with what happens overseas. Two examples must stand for many:

> "I detect .... - this is arm-waving - in Japan, the US, and Germany, there is more national pride about technology; .... they're more prepared to pump money in. .... No sign of that in UK in the small company sector"

> "We're probably quite bad as a culture fostering those things"; it is difficult, an idea has to be good to warrant it. It is a criticism of the finance industry.

At first sight comments like these scarcely seem symbolic at all, they appear more as frank admissions of failings in the finance community, but they are a little more complex than this. In simply admitting guilt for failure there would be no need to bring in other countries, or culture, or national pride - no point in introducing public discourse at all. Public discourse is introduced as <u>something outside of financial control</u> - as something on the other side of a boundary that can be blamed - as a symbol.

Some of the symptoms that suggested a 'national disease' seemed plausible, but their frequent repetition hinted at a basis in symbolism rather than in analysis or experience. Many interviews raised such symptoms:

> "Entrepreneurs here prefer to have one hundred percent of nothing, rather than ten percent of something. .... That does not apply in other countries - at least not to the same extent".

> "In the UK what tends to happen is the founder wants to keep fifty-one percent for ever. .... Well - the chances

> are he will not get funded". "Now, in America, you don't get founders wanting to keep fifty-one percent - a British disease".

Of course, material international differences do undoubtedly exist - this is a matter of interest, but not of great importance, to the present research. The important point that this chapter has argued, is that differences, whether material or imagined, exist as resources for the construction of symbolic artefacts. These symbols allow people who share discourses to come to terms with the paradoxes that confront their workaday lives and their attempts to innovate.

The next chapter will investigate the process of innovation by looking again at the case-studies, and at the people who innovated. It will seek a deeper understanding by setting them in motion within the framework of the four discourses proposed in this chapter.

CHAPTER 7 : IDEAS, INNOVATION AND ACTION .... CROSSING THE BOUNDARIES.

> .... I dont start new businesses: I let other fellows start them. They put all their money and their friends' money into starting them. They wear out their souls and bodies trying to make a success of them. Theyre what you call enthusiasts. But the first dead lift of the thing is too much for them; and they havnt enough financial experience. In a year or so they have to let the whole show go bust, or sell out to a new lot of fellows for a few deferred ordinary shares: that is, if theyre lucky enough to have anything at all. As likely as not the very same thing happens to the new lot. They put in more money and a couple of years more work; and then perhaps they have to sell out to a third lot. If it's really a big thing the third lot will have to sell out too, and leave their work and their money behind them. And thats where the real business man comes in: where I come in. But I'm cleverer than some: I dont mind dropping a little money to start the process.
> (G.B.Shaw 1964:94 'Heartbreak House' [first pub. 1919])

7.1 Introduction

The last chapter tried to show the robustness of a four-discourse model of the worlds in which innovation happens. This chapter will use the model as a framework within which to set people's innovative action. This will re-introduce the people to the structure of the discourses that those people's words suggested in the first place.

This introduction is a good point to pause for a theoretical overview of the task ahead. Chapter two discussed various theoretical perspectives, and introduced many of the issues that now confront analysis. This allows the present three chapters of discussion to proceed with the minimum of theoretical overburden, save for an outline statement of analytical intent.

Adding people to structure recombines, in a way, Saussure's 'parole' and 'langue'. However, the innovation that will be investigated here is not some arbitrary diachronic flow of behaviour, it is the purposive, conscious action of individual people. Agency must be of central concern in what follows, and the ideas of Giddens are of great relevance - he is sensitive to structural notions of discourse, but resolutely adds human agency throughout his discussion - what he terms 'the duality of structure'.

Chapter two must stand as a theoretical reference for the elements of Giddens's ideas that underpin what follows in this chapter, but it is necessary first to note the way that the four-discourse model diverges from Gidden's tight view of Saussurean structuralism. He sees multiple discourses, but his analysis adds other structural dimensions to what he seems to see as a single code of signification - moreover he excludes symbols from this code. The model here sees the four discourses as multiple codes, but only a single structural dimension of signification - one that does, nonetheless, allow symbols.

Given this disagreement, Giddens's views of symbols, institutions and discourse are still extremely relevant. The use of discourse here incorporates many of his ideas: discourses are seen as recursively organized social structures; they are routine ways of talking and behaving for those who share them, manifest within the parole of everyday life through words, symbols and routine. Human agency can use the resources of discourse, and action may result in both

intended and unintended consequences. Both discourses and action are recursively implicated in social systems, and in the institutions and social collectivities that bring people together. In this sense, an innovative project can itself be regarded as a social institution. Discourses are not properties of, nor is there any reason why they should be coterminous with, any particular organization.

These ideas will be applied to the innovations in the case-studies. Discussion will use, as a loose working principle, the idea that the originators of ideas continually cross the boundaries between the different discourse communities in order to innovate. An innovation is thus recursively created, and the discourses themselves recursively re-created. It is tempting to see a parallel in the way this thesis itself continually crosses between scholarly concerns with data, theory, and method. The decision to consign theory, data, and method to separate chapters forces discussion into a continual recursive reconsideration of each, which has greatly aided interpretive analysis. This analysis aids understanding of innovation, but innovation aids understanding of analysis.

The resources that discourses afford innovators consist in aid from other people; their ability to benefit depends upon how well they can 'speak' the discourse. The empirical evidence of success in boundary-crossing is seen in the breadth and number of the contacts that a person has with others, and it is here that the investigation of this chapter

will start. Its central theme is then to re-draw, onto the four-discourse model, the progresses of the case-studies described in chapter four.

Of course, the model was based on analysis of these case-studies in the first place - it is, therefore, only fair to test its mettle upon something else as well. To do this, the four-discourse framework will be applied to different data and different theory - Latour's model of 'science in action'.

This chapter can be seen as a further investigation of the structure-process debate, so central to radical structural conceptualizations of the world. The last chapter was about structure - in the words of the quotation from cummings at its head, it was about how people 'smash why into because', and call the result discourse; this chapter puts the people back in, and tries to show how they can create as well as destroy.

7.2 Networks and Mythology

Innovations grow through human agency, and involve each of the four discourses, but they are also subject to chance. People have thus to talk across the discourses to accomplish innovation, but they must also have ways of shielding themselves when frustrated by chance calamities. This section focuses on the linguistic and the symbolic to illuminate both of these areas.

Innovation needs the help of other people, and the analysis of the interviews showed networks of personal

contacts - most noticeably around HOTOL and the City of London (both areas are developed further in the appendix to this chapter). A few examples will be considered here, starting with Parsons.

A look at turn-of-the-century engineering shows the network of friends and acquaintances upon whom Parsons could call. His father was an Earl, and a respected amateur scientist; his brother succeeded to the title, and brought others into Parson's companies. Parsons corresponded with eminent scientists and engineers, Rayleigh wrote a memoir on Parsons, he sought advice from prominent shipbuilders, and from William White; Lord Kelvin appeared in court as an expert witness against him. Moreover, just as various people in HOTOL shared the experience of working on the Blue Streak project, so people around Parsons shared his experience of working for Armstrongs (whose directors included, at various times, White, Watts and Fisher's friend Vavasseur). The various biographies of Fisher provide other examples of networks, like his acquaintanceships with royalty, political leaders, industrialists and bankers - in many cases socially as well as on Admiralty business.

The passage of time allows the full ramifications of, at the time clandestine, associations to become public in such historical material. Nonetheless, similar networks emerged from HOTOL like Blue Streak - "a bit of an old-boy's club" as one interview put it - or the interface between industry and government. Many interviews evidenced the breadth of the

network of personal contacts between people in the two companies and those in DTI, BNSC, the MoD research establishments at Farnborough and Pyestock, and technical procurement agencies like DGEng[1].

Finance throws up its own networks, and their importance was frequently mentioned - a merchant banker, for example, noted: "we have a lot of contacts"; an American banker commented: ""it's a lot of networking really"", and another added flesh to this with the observation:

> "The fund manager's friends at the golf course .... told them of fabulous returns". .... "It's like clubs, it's who you know".

He went on to suggest, like others, that people seeking funds for a new idea were much more likely to succeed if they came with an introduction from someone the financier knew and trusted ("The local professional mafia", a UK venture capitalist termed it). This is borne out by the evident distrust of financial intermediaries or deal brokers, although their existence does illustrate innovators' attempts to gain entry to the networks, however misguided.

An idea needs a network of people to become an innovation, but this means that those who originated it have increasing difficulty controlling it. As it crosses boundaries into new discourses, myriad other people enter, so does chance. Giddens discusses the limits of intentionality, but he also discusses symbols "as one main dimension of the

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[1] For glossary sees appendix IV, section II.

'clustering' of institutions" (Giddens 1984:32). An innovation can usefully be regarded as an institution within Giddens usage of the term - it is a socially persistent, and growing, band-wagon that the originators of an idea heave into motion, and onto which those in their growing network of contacts may jump. To maintain some sense of ownership, the originators have recourse to symbolism - to the mythology of the innovation.

The most fundamental myth is that of creation, and chapter four suggested creation myths for HOTOL the idea, HOTOL the BAe project, and for 'Turbinia'. The HOTOL originators saw the birth of their idea at a 1982 technical meeting - moreover, they saw it in the classic symbolic sense of a reversal of the previous technical wisdom. The origin of HOTOL funding in BAe had an equally mythical dimension - another meeting, where an Admiral challenged a Government Minister to match his funds. Parsons, likewise, has a story to tell people about the birth of the idea that led to 'Turbinia' - a suggestion from a friend whilst out shooting.

Innovation's symbolic side is seen in the ways that engineers talk of meetings, and venture capitalists of business plans. Their endeavours seem to need 'rites-de-passage' at times, as HOTOL's passage into the public domain through the sacred rites of publicity well exemplifies. That the modern shamans of innovation wear business suits, and would be appalled at such a mythological reading of the texts of their interviews, need not be cause for concern - as Giddens, mentor of this chapter, says:

> Modern organizations .... operate in a social world in which the retreat of the gods and the dissolving of tradition create the conditions in which reflexive self-regulation is manifested as history - and as sociology. (Giddens 1984:203)

He goes on to note "the prevalence of historicity", and "the 'feeling for world history'" in the modern era. The relevance of Kumar's sociological commentary, in chapter one, on the industrial revolution, and of narrow readings of enlightenment rationalism, become very clear here. The very evident desire of the originators of HOTOL to set it within just such an historical framework was clear in what they said (indeed this thesis makes some slight contribution to their efforts at inscribing an historical record). Alan Bond has described his project as:

> something which is going to introduce a modern industrial revolution comparable to that of 150 years ago. (Quoted in 'The Times' 13-10-1987)

Peter Conchie has also been quoted as saying that launcher developments "will be another industrial revolution" (Ferguson 1988:89).

Innovators very evidently need to build up symbolic resources around their projects. This reassures them that they still control and symbolically own the project, and that, despite the vagaries of chance and the hostility of others, its 'historical' inevitability cannot for ever be gainsaid. In the process, the project, and the discourses that constrain and enable it, are continually reflexively re-created. The rest of this chapter shows that crossing discursive boundaries is thus a complex business. People like Fisher seemed to know

this well - a 1910 letter might serve as a motto for innovators and boundary-crossers of every age:

> the real secret of successful administration is the intelligent anticipation of agitation. Control the whirlwind and direct the storm. (MII:336)

7.3 HOTOL Crossing Boundaries

The originators of HOTOL trace its conception to an exchange of remarks at a meeting of the British Interplanetary Society at Easter in 1982. Alan Bond, as he put it, "perceived what was being stated was fallacious". Its origin was firmly planted in the moral discourse of a small group of engineers who shared past experiences in the UK space industry. The three key people - Alan Bond, John Scott-Scott and Bob Parkinson - all eloquently described the processes of inventive thought, and hard work at home, that characterized this time.

This section retraces the story of HOTOL already told in chapter four, but this time sets it within the four-discourse framework. A strictly chronological course will be followed - discussion demands this since the present chapter adds a diachronic process of innovation to the synchronic structure of discourses. Section 4.3 must stand as a source of reference, for clarity its detail will not be repeated here, although a few choice quotations have been saved up to maintain something of the atmosphere of the data. Two of the originators, talking about invention, set the scene:

> You "cannot say 'we're going to innovate.' .... Spontaneous[ly ..... my mind says] - 'I've posted the

> letter' - .... subliminal level, then maybe a fortnight later it comes out and you have to write it down fast."
>
> "To innovate technologically you have to have the imagination to see things don't have to be the way they are .... but most of what you've got, is what you've got".

The originators described their early meetings; their sums suggested HOTOL might work, but, by the end of 1982, they needed a little more support - the boundary between moral and industrial discourses had to be crossed. Scott-Scott worked for Rolls-Royce, and Parkinson had very recently joined BAe, so both already straddled the boundary. HOTOL, too, sat astride it for a while - comments like Scott's recollection of a ""discussion in Alan's office (where we shouldn't have done it)"", well catch its in-between status.

After some difficulty Rolls-Royce found some unofficial "local money", whereas BAe found more official funds via Parkinson's newly returned Director (and old acquaintance) Peter Conchie. HOTOL remained betwixt discourses in Rolls-Royce, but crossed into the industrial discourse of BAe (albeit a part of it rather tainted with moral discourse) - a status well caught by Clive Leyman:

> "Actually, that's fairly typical of the way an aircraft comes about - .... doodles on the back of an envelope, ... takes it to the boss, who says 'don't be so bloody stupid'" .... "It's a glamourous thing, engineers like me tend to get interested and say 'let's have a go at that'."

Parkinson describes getting help from Leyman at BAe Filton "almost by accident", when <u>they</u> asked <u>him</u> a question, which showed they "were quite willing to accept these strange

ideas". Scott-Scott describes how he "found another old diehard at Bristol" in Rolls-Royce:

> So with director's support I made contacts - "'don't worry about the bookings'"

These two examples from BAe and Rolls-Royce sound remarkably similar, but there is a difference that proved absolutely crucial for HOTOL. By no stretch of the imagination could the phrase 'don't worry about the bookings' be ascribed to industrial discourse - it is an outcropping of moral discourse within a large company. HOTOL had not yet entered the industrial discourse that would give it legitimacy in Rolls-Royce, yet it had in BAe. Certainly, in BAe, responses like:

> "we said yes, having checked it with the gaffer .... - when the money came it only scratched the surface of what we'd spent"

are firmly located in moral discourse - save for the crucial: 'having checked it'.

The companies' internalized elements of financial discourse are also evident in this - as a word like 'budget' betokens - management accountants negotiate the boundary through procedures of industrial discourse like 'bookings'. People in BAe were very clearly aware of the boundaries of three discourses here, as two sophisticated comments show:

> In the very early stages - the gestation period - you have "got to have some capability .... for unofficial projects, because when a thing starts, when it's very small, no-one will give you money, and you need to do something to get money" - you have "got to start with someone doing what he's not supposed to be doing" - you "need a degree of slack". We "had trouble early on" - someone wanted to know why I was working on something. You "have to go underground"; "we turn a blind eye".
>
> "The early stages were freelance, ... there is a critical sum of money beyond which it is [,has to be,] formalized". "We're not mincing words" - it involves

> "'losing' money [expenditure] - .... because you haven't got a budget for it".

BAe next sought to tap the funding resources of public discourse in the guise of the DTI, and, as Parkinson says: ""after some considerable time, liberated forty thousand pounds"". This was for an outline study of launchers in general, and was clearly nothing very unusual for either side - many such relationships emerged in the interviews as institutionalized ways of crossing this boundary between people in the companies and those in Government agencies.

BAe's support grew, Rolls-Royce's did not - Alan Bond summarized:

> The idea was not propagated into Rolls-Royce further than the line management. BAe, however, talked at director level - they produced higher level money

In BAe, industrial discourse slowly came to predominate over moral discourse, but the latter still showed through:

> "We .... put a whole lot of hurdles in the way .... one way to make sure it is robust is to throw in everything. .... If it's that robust, it must go ahead, ... be worth sticking your neck out for"

This illuminates a different part of the moral-industrial boundary - no longer a small-scale, clandestine, 'losing of expenditure', but rather morally fired insiders negotiating within the accepted rules of industrial discourse. The important point is that the industrial discourse obtrudes in both cases - as procedures to be 'got around' or procedures to be used.

Thus the moral project, HOTOL, had entered the industrial discourse of BAe, but not Rolls-Royce, and the Governmental variety of public discourse. All of these came together in the curious case of the patent which Alan Bond, with advice from both companies, then took out. Patents are themselves a bridge between moral and public discourses - the idea becomes legally reified as public intellectual property. The twist in HOTOL's case was that MoD promptly classified the patent, effectively making it private property again - the government's private property.

HOTOL's public début was an event of critical importance in its growth. The story arose insistently and unbidden from the interviews, and has been well enough discussed already to require little further explication here, save to acknowledge it as a fine example of crossing boundaries, and to ask the key question - which ones? Frank Miles very clearly appreciated three of the four discourses at this point - he describes the moral discourse of those who helped him with the story, and his own moral fears that they "would get hurt". He also shows his own organization's industrial discourse - his need for confirmatory hints that HOTOL existed (for example: "I read a memo upside down on someone's desk which seemed to confirm it"). Finally, he mentions serving "the public interest" by publicizing HOTOL.

In industry, likewise, all of these discourses were implicated. In the spring before the August 1984 News-at-Ten scoop, the originators and both companies had been involved in soliciting public discourse through their usual routes -

presentations to technical experts and the like. This industrial-public discourse boundary was very clearly perceived in comments (with clear moral overtones) like: ""It wasn't respectable in the early days, ... had to have the blessing of Pyestock"". Even here HOTOL was a little unusual - a threat even - it ""floored them completely""; nonetheless, as interviews made clear, government departments were just following the usual procedures of their own industrial discourse in soliciting their experts' advice.

As a way of crossing the industrial-public boundary, the publicity was altogether different. In BAe, by this stage, the chief executive had become involved (both morally and industrially: "Ray Lygo fell in love with the project"). But in Rolls-Royce the Chairman had never heard of HOTOL:

"it was all done without any bookings, nothing had ever shown .... The Chairman said we're not working on this"

The interviews colourfully describe the results, in summary: now HOTOL was a part of Public discourse, Rolls-Royce were forced to accept that it existed, and therefore had to make it exist within their industrial discourse ("a proper programme was put together"). Rolls-Royce's general distaste for the project was evident for ever after. BAe, on the other hand, after a little righteous indignation, used the publicity to good effect.

HOTOL was now lodged in the industrial and financial discourses of both companies, and public discourse had been mobilized. The 'general public' had seen HOTOL on the

television, and then again at the Farnborough airshow, and had approved ("It was "the only thing that generated any attention"") - HOTOL became a symbol in the moral discourse of 'the public'. Both companies, and government, had to take account of this; if they did not, Miles was there to ask them why:

> I said 'what about HOTOL?', and went on to say that if he did not raise it I would be sure to interview him on the News-at-Ten

However, within the composite discourse that was HOTOL, the element least in evidence was the one it most needed - a wider financial discourse. It still needed money. This informs the meeting of 'the Admiral' and the minister - at Lygo and Pattie's meeting all four discourses met. They had a measure of control within financial discourse, however circumscribed:

> "the Admiral doesn't have any money, neither does Pattie", it is all in budgets, it took nine months.

A 'proof-of-concept study' (itself a name to conjure with in the four-discourse framework) followed in the autumn of 1985. Interviews described the way industrial and financial discourses met within BAe, and how the Warton site was chosen as HOTOL's new home. They also described the meeting of industrial and public discourses, with new proposals "To get DTI money", and so cross the government's own internal public-financial boundary, itself described in other interviews.

HOTOL, then, seemed happily settled within each discourse, with each helping the others across their mutual

boundaries - all in a sort of balance. If it had ended there, the story would be a neat example of a successful innovation, but of course it did not. Pattie supported HOTOL - it was a suitable symbol of government's new interest in space to use in the debate of European public discourse. But the government view of space changed, the minister changed, the consensus across all four discourses was broken, and it all, as Bond said, ""crashed in the space of a few days"".

Teasing apart the wreckage of HOTOL at this point shows the way the resources of the four discourses are used to further people's own ends - and how the people and the discourses started to diverge. BAe continued along the old route, soliciting European cooperation for HOTOL (now firmly a part of their industrial discourse). Rolls-Royce, however, now embraced the new view of HOTOL that government were presenting in public discourse, as an ordinary commercial project subject to the invisible hand of pure market competition - a view that allowed them to escape from the project:

> "it was difficult to see how our company would make any money out of it, ... it was difficult to see how BAe could lose"

A very subtle point emerges here - the BAe HOTOL looks an unimpeachable symbol of national pride, the Rolls-Royce HOTOL, meanwhile, looks a classic example of poor economic husbandry. At a substantive level both views are correct - interviews clearly showed that, on the one hand HOTOL and BNSC had improved the standing of the UK in European space circles, whilst on the other a few HOTOL engines would never pay Rolls-

Royce, whereas BAe's satellite business would do very nicely out of a radically cheaper launch system. Government supported first one view, then executed a speedy U-turn and supported the other. How could all this be?

The four-discourse model provides an answer; the reason for the care that was taken over the definition of public discourse in the last chapter is now apparent. The evident co-existence of mutually exclusive views of HOTOL shows why public discourse must include markets, the media, and public opinion, as well as government - in short it must include the public symbolic faces of each of the other three discourses. It is the rhetorical battleground between them. When the boundaries between discourses cannot be internalized within an organization, or proceduralized between organizations, or fudged amongst friends - then another alternative is to don rhetorical boxing gloves and slug it out in public discourse. Before the government's U-turn, the winning HOTOL rhetoric in public discourse was moral, after it, it was financial.

Thus, the early history of the project - <u>the discourses in which it grew</u> - set the scene for the vituperative and sad publicity that surrounded Clarke's very public withdrawal of funds in October 1987. The early failure to talk in the industrial discourse of Rolls-Royce blighted the project there when it explosively entered public discourse. Publicity, however, gave rhetorical assistance to BAe, but the stance they adopted led to later rhetorical failure. Entry to public discourse, across whichever boundary, is clearly dangerous -

but it is inevitable. Before the ITN publicity even, everyone had already placed HOTOL on the stage of public discourse by seeking government funding. Government in turn, spending little, but swaying with the changes in rhetorical debate, came to play a crucial catalytic rôle for HOTOL[2]. As Roy Gibson, the man who made the ultimate moral statement of resigning, summed it up:

> "The lesson .... is how fragile these programmes are when they are dependent on one-off Government decisions."

The way an innovation starts clearly has tremendous influence over its subsequent growth. This prompts the question - what if the innovators early choices had been different? The continuation of the HOTOL case study gives a rare opportunity to answer this question. While BAe kept on in the same direction without government support, Alan Bond, an independent actor throughout, the representative of moral discourse perhaps, started all over again.

A result of the commotion that Clarke's announcement caused in public discourse (of his financial assault on moral discourse) was the property developer's offer to help Bond. The problem was that only industrial discourse could muster the resources needed to develop the big project that HOTOL would have to become, and thus BAe and Rolls-Royce would have

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[2] The symbolic character with which people imbued the government's patent classification provides a striking post-script here. The Rolls-Royce interviews bemoaned its presence far more than BAe did, even though it helped them get out of HOTOL. Rolls-Royce perhaps needed the scapegoat of classification to deflect public moral criticism of themselves.

to be involved. Rolls-Royce were approached, aided by lobbying from such as Pattie, but they could ignore this with impunity - which they did. Nor was there any possibility this time of enlisting the aid of public discourse, since the potential benefactor wanted no publicity.

Bond continued by trying to talk in the language of financial discourse - if HOTOL's financial face was in the ascendant in public discourse, then why not call Clarke's bluff, and try to sell it across that boundary, but:

> they could not fund it - "those were the rules of the game". "Some days we'd go off quite optimistic, ... but we'd still end up round the pub getting blitzed".

Bond applied his own considerable technical abilities to elaborate financial schemes, but none was able to help HOTOL. HOTOL makes no sense, has no meaning, signifies nothing, in the linguistic codes of financial discourse. The irony seems to be that HOTOL is too small a presence in any industrial discourse for financial discourse to be able to understand it as a 'major project'. Yet within moral discourse it is too large to be the sort of idea venture funding can translate into financial discourse[3]. Bond is still trying - sensible of this paradox, he has started to create his own industrial discourse by setting up his own embryonic company to develop the HOTOL idea.

So, what does the four-discourse model suggest that 'the HOTOL idea' is? For Alan Bond it is now a radical new engine

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[3] and the potential international market in which it might sell, a curious and oligopsonist one, is unacceptable.

design, called SABRE, powering a putative elegant and sleek vehicle called SKYLON. For BAe it is still called HOTOL, but a very different and cut-down, short, fat HOTOL with conventional engines, that they are studying with Russian partnership. There are other ideas that look like HOTOL too - the German Sänger, the US NASP, among them. It seems likely that one such will succeed; perhaps that will be the real 'HOTOL idea' - Alan Bond said as much: "someone will do it, whether HOTOL succeeds is irrelevant".

Everything that has been called HOTOL in this section has consisted in drawings and calculations on pieces of paper, not some metal or advanced composite artefact that anyone could actually touch - such is the reality of engineers. However, even that would not be the real 'HOTOL idea'. Just as, to the people in the interviews, Blue Streak was a network of old friends and colleagues - not a sad corroding rocket in a silent science museum - so the real 'HOTOL idea' consists in people. This section shows that the reality of HOTOL exists within four discourses that those people share; the story shows that their composite HOTOL discourse will only succeed if it incorporates elements of all of those four discourses - three discourses are not sufficient for success.

7.4 Parsons and the Others Crossing Boundaries

7.4.1 PARSONS

Charles Parsons's innovation is similar to Alan Bond's in many respects; both are radical new turbo-machines, both involve lost patents, and both drew in a network of past associations. HOTOL's News-at-Ten début prompts memories of 'Turbinia' and the Spithead review. However, it is the way that Parsons fills in a possible future for HOTOL that is interesting here. Parsons started with a small company but within twenty years he was providing engines for the most advanced battleships and liners of the day. This sub-section looks again at Parsons and Fisher.

When Parsons set up his 'boat company', between 1893 and 1894, his existing company already provided a foot-hold in industrial discourse. He had considerable personal funds in this company, and experience of setting up others. He also had access to a network of what would today be called 'high-nett-worth individuals'. His contacts were considerable in the area where industrial, moral and financial discourses meet.

There are hints that fund raising was not always as easy for Parsons as this implies, but it is fair to say that neither of his marine turbine companies seemed to have great problems crossing the industrial-financial boundary, with a bit of help from the moral discourse Parsons shared with his contacts. In any case, his original company could have helped out - indeed a letter a month before the new company was

registered, showing that preparations for building 'Turbinia' were already in hand, suggests that this is precisely what happened.

Parsons had control over the financial and industrial resources he needed - he could thus go wherever his moral discourse suggested, even when others disagreed. In all of Parsons's private descriptions of 'Turbinia' it is his moral discourse that is most evident; her problems were technical, but even here Parsons used his many acquaintances in the loose marine engineering 'community'.

Nothing has yet been said of public discourse; the most insistent boundary here was with the potential market for marine turbines. As early as 1895 he was writing: "We propose to have a directors' meeting to consider the foreign patents for the boat". Parsons was also looking to another part of public discourse as a customer - to government. In December 1896 he wrote that he was: "In correspondence with W.H. White", although, despite the old acquaintances that dotted this boundary, it still evidently needed circumspection for Parsons later: "sent W.H. White a long very buttery letter". Success followed - in February 1897 Parsons wrote: "W.H. White is coming on monday to look her over. I think there is business coming in this quarter".

This was four months before the spectacular emergence of 'Turbinia' into public discourse at the Spithead review - a puzzling event, given the paucity of evidence that the naval business which did follow had anything to do with the

publicity. The evidence suggests that relevant people - particularly White and Vice Admiral Durston were already convinced of the turbine's promise. Brown (1983:75) suggests that Durston sanctioned Parson's intrusion at Spithead, which seems likely since Parsons would hardly have wanted to upset his Naval contacts by an unnecessary, and provocative, appearance.

The four-discourse model can suggest explanations, rooted in each of the other three discourses, for this event in public discourse. Publicity, as HOTOL showed, can be instrumental in bridging boundaries, and Spithead might have been an attempt to persuade the financial discourse of government to provide the money for more ships. Evidence of a new high-speed threat would have been a fine way of keeping the destroyer pot boiling - one of Parsons's potential markets - as well as publicizing the turbine's wider potential for fast commercial applications.

Within the industrial discourse of the Admiralty it is possible that Spithead represented Durston's attempts to promote the turbine (maybe even to Fisher). The seeming cavalier disregard for the procedures of the Navy's own industrial discourse may have reflected the need to avoid prior veto from Durston's opponents, or it might just have been quicker to side-step such regulations (something of a naval habit, in the long tradition of Nelson's blind eye). A moral discourse explanation, on the other hand, might be found in the ample evidence that Parsons, Fisher and their colleagues simply enjoyed doing extravagant things at high

speed, or in terrible weather, with their machines - Spithead must have been fun, patriotic fun.

Many more boundary crossing theories could be postulated - the point here is that the model does not claim that any of them is the true explanation - but that any of them could be 'true'. Each is true within its own discourse, the model allows each of the many people or groups involved to have their own reasons for acting as they did. This explains why the Spithead review seems important across a range of social institutions - it became incorporated into them and played its part in their reflexive re-creation.

'Turbinia' led on to small passenger steamers and the destroyers 'Viper' and 'Cobra', whose loss delayed the turbine's entry into the Navy. Parsons commented in the laconic industrial discourse of a 1903 lecture that this: "had a very detrimental effect". By 1905, however, the Navy had adopted the turbine as its standard propulsion machinery. Fisher claimed the credit for this, notwithstanding the fact that as late as 1900 he had blamed the lack of destroyers on the Admiralty's waiting for turbines! The great watershed was Fisher's pushing through the first turbine battleship, 'Dreadnought', and around the same time, the adoption of turbines for ocean liners.

Fisher's machinations in championing 'Dreadnought', provide a good case with which to test the four-discourse

model's explanatory powers[4]. Fisher's continual use of the word 'push' to describe his activities well shows how an idea growing in one discourse can entrain all the others. He pushed his schemes across the boundary between his moral discourse and the Admiralty's industrial discourse, and then kept on pushing them into public discourse as ideas demanding government funds.

A standard route for Fisher's ideas was via the letter-boxes of his contacts in the press, but in the case of 'Dreadnought', Fisher's route was even more subtle. It shows the behind-the-scenes interplay of discourses that often remains hidden in contemporary case-studies. In August 1903, Fisher became Commander-in-Chief at Portsmouth, and his correspondence makes very clear that over the next year he got together a team of confidants and that they designed 'Dreadnought'. It is also apparent that, until he became First Sea Lord in October 1904, he was very economical in what he told the Admiralty and the government about all this ("Think I'm such a born fool as to tell them").

Fisher was clearly in a powerful position, but even his most devoted biographer acknowledged the very considerable opposition of Admiralty, Treasury, and others in the Navy, to his plans (Bacon 1929,vol.I:256). However, Fisher had many friends too; possibly the most influential was Lord Esher, a confidant of the King and his ministers - an 'éminence gris'

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[4] Although, it should be said at the outset that the true advocates of turbines were probably Durston and Watts, not Fisher (Brown 1983:86), it was they who made the turbine and 'Dreadnought' inseparable.

who eschewed appearance in public discourse but clearly shared Fisher's moral discourse. Marder (1956:100) says: "Through Esher Fisher was 'au courant' with high politics", and a letter from Esher himself, in August 1904, reinforces this:

> I don't think you need to trouble about H.M., for he will always back you. When it comes to a change of Government .... All these people are really cyphers. Remember, not more than a dozen people in England count for anything (a large estimate), and you happen to be one of them. (MI:324)

Fisher was at the height of his powers; even so, he still required a little deftness in pushing 'Dreadnought' - that very August he told Esher that the First Lord had agreed to a design committee for the already designed ship:

> I explained to him that I had got the designs out of what had to be; but it was the politic thing to have a committee of good names (MI:325)

Even two months later, two days before becoming First Sea Lord, Fisher was still treating the First Lord, his new political master, a little disingenuously:

> Well! having thus more or less got a favourable opinion from you, I elaborated the manuscript which you had read, and printed it .... Then I gave it secretly to the five best brains in the Navy .... and associated two other brains ....
>
> This is the 'modus operandi' I suggest to you. If these proposals in their rough outline commend themselves to you .... then let me have these seven .... and secretly .... get out a detailed statement supported by facts and figures. (MI:330-331)

The committee was appointed in December 1904; it considered various evidence, including that of Charles Parsons, settled the designs, and in October 1905 'Dreadnought' was laid down. She was launched just eighteen weeks later. The idea of the marine steam turbine had become a major innovation.

This example of how innovation happens is rather different from the others, it was conducted largely in secret, yet a four-discourse model still elucidates it. Fisher was continually pushing his moral discourse across the boundary into public discourse where it was continually assaulted by the rhetoric of those who disagreed with him. Thus Fisher well knew how easily his plans might become stalled in public debate - just as HOTOL did - with the government doing nothing. He could not present 'Dreadnought' in the industrial discourse of day-to-day Admiralty life, or in the Treasury's financial discourse, because as Commander-in-Chief of a dockyard he had no official access to those discourses - and Fisher had too many enemies for him to trust anyone else to do so. This posed a dilemma for him and his confidants because the moral vision they all shared simply could not wait until he took over as First Sea Lord. The only solution was, therefore, that his plans remain secret.

The result was a clandestine design that was complete before the committee that arrived at it was constituted, and that was produced for the First Sea Lord months before he actually became First Sea Lord. In other words, 'Dreadnought' was a creature of moral discourse. To push it across the boundary into financial discourse Fisher had to hide behind the stalking horse of his committee of experts; to push it into legitimacy within the industrial discourse of the Admiralty he had to create a façade of the First Lord's approval by pretending the plans were less advanced, and more under the latter's political control, than they were. Only

when the designs were solid Admiralty proposals could the devastating lobbying powers of Fisher, and his friends like Esher and the press, ensure their official acceptance into the public discourse of government policy, much less their financing and launching on the high seas.

7.4.2 THE OTHERS

The single-interview case-studies in chapter four, and a few examples from chapter five, augment this chapter's use of the four-discourse model to investigate innovation. These examples concentrate, as it was intended they would, upon the boundaries around financial discourse.

The historical examples with which chapter five began have a common thread - people involved in various activities, when faced with the lack of effective financial services, had to invent them. Activities within an industrial discourse seeking to act upon a public discourse found that the resources of a financial discourse were wanting.

Phoenician traders serving distant world markets settled in Greece as bankers, brokers, and 'business angels'. The Templars, by definition a moral order, but cast into the rôle of soldiers protecting pilgrims, had to turn bankers to protect the pilgrims', and their own, money in distant lands. They thus internalized the boundaries between their moral discourse and both the ungodly financial discourse of usury, and the profane industrial discourse of soldiering. The East

India Company, likewise, came to have a strange mix of discourses - it started as a novel way of crossing the industrial-financial boundary at home, but its charter also gave it the right to internalize public discourse as a financial monopoly in the East; it then became, unwillingly, an imperial power in India, and had to internalize first the industrial discourse of a navy and an army, and eventually the public discourse of colonial government itself. Finally, the merchant banks of today started life as merchants who had to administer for themselves the financial instruments they needed; acceptances mediated the boundary between financial discourse and the industrial discourse of trade, stock issuing mediated finance's boundary with industry, and with the public discourse of states.

All of these organizations can be seen as institutions in Giddens's sense. Even though they have a deceptively structural character to the people involved in them, they all show how human action is implicated, through the routines of day-to-day life, in their own continual recursive re-creation and change.

Institutions change slowly; the individual inventor of the day has to accept this, and negotiate across the boundaries as they are. The first short contemporary case concerned a man within a large company - his idea was a change to a company product, it thus started in industrial discourse. Nonetheless, his words were very clearly within a moral discourse that others came to share: ""People began to get

enthused"". To 'enthuse' them he had crossed various boundaries - finding difficulty in entering BAe's industrial discourse directly, he used that of Dowty as a stepping stone. He also described his rhetorical use of the internal public discourse of BAe: "I said 'get everyone in a cinema - I'll do a presentation'", he also resorted to practical demonstrations - for which he even borrowed a fire engine.

Still failing to penetrate beyond the local industrial discourse, he next tried to use the wider public discourse as a way round, by involving government experts. Interestingly, though the project did gain publicity, this area of public discourse was little used by the protagonists (even though some of them had been active in HOTOL - in reversed rôles!). He was unsuccessful, and felt ill-treated, if unbowed; presentation of private ideas in public discourse is a painful business.

A biotechnologist, with intimate experience of seed capital in action, was interviewed for the second case. His description of his backers makes it clear that crossing the boundary between moral and financial discourses involves more than simple funding:

> they help you make that first business plan that goes beyond the technology alone. "That's a critical rôle that's difficult to fill"

Seedcorn is also a bridge between the founder's moral fervour and the nascent industrial discourse of a new business. It is thus implicated in three discourses, but the fourth, public discourse, also enters in a curious way.

Section 6.3.7 showed how 'problems', like the refusal of 'founders' to relinquish control, could become the symbols of the finance-public boundary. In the interview considered here it was the founder himself that raised this:

> Do you want one hundred percent of the action, where in the end it is only worth one unit, or do you want to end up with ten percent of one hundred units?

Around seedcorn funding, the four discourses press hard against one another - paradox and symbolism mediate the boundaries that a new idea needs to cross.

The third study looked at a similar but different case - venture funding for the buyout of an existing business by management. Here, an industrial discourse was not so much created, as transformed - its change mediated by the venture capitalists. The leader of the buyout, an engineer, emphasized the intermediary role of the financiers, and their boundary with industry:

> "They were in teaching mode too"; they were dealing with chaps who do not know much about the City and finance .... "There was a job to do in reconciling our view and the City view"

But he also showed considerable insight into the character of financial discourse itself, commenting that: ""their main interest was their reputation"", and linking this to their ability to mediate within financial discourse for funds:

> "What they did was to open the door to people who would give a term loan .... They gave the confidence to the people who would do that".

A financial discourse where words like 'reputation' and 'confidence' open doors, contains an interesting added dash of

moral discourse - of Durkheim's 'non-contractual element in contract'[5]. Moral discourse also shone through the man's words themselves. Discourses are often difficult to separate.

Chapter four ended with Sir Clive Sinclair's rather world-weary views on funding for innovation. He was dismissive of attitudes to innovation in all four discourses - large companies, Banks and the City, and stock markets all provided little help to the innovator. His view seemed to be that crossing boundaries was only possible with "drive to push it all the way through" - his own schemes had mostly been funded from the proceeds of past projects. But, even in moral discourse, his observations to the Daily Telegraph interviewer hardly evinced enthusiasm: "Accepting things are ultimately pointless, but doing them anyway".

The other innovators interviewed in this thesis certainly seemed to have the 'drive' and 'push' to confront the arguments of industrial and financial discourses, and to counter them in public discourse. But it was in moral discourse that they discussed the 'point' of doing it all.

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[5] Gowler and Legge's (1986) brilliant discussion linking careers, reputation, and rhetoric is peculiarly apposite here.

7.5 How Latour Perceives Boundaries

> historians of technology are often aligned along the technical specialities of the engineers, some studying aircraft industries while others prefer telecommunications or the development of steam engines; as to the anthropologists studying 'savage' reasoning, very few get to deal with modern knowledge. (Latour 1987:16)

In the last two sections the four-discourse model has been useful in understanding why things happened as they did. The objection might reasonably be raised, however, that the model emerged from analysis of the very cases considered in these sections, and could therefore be expected to work - this was its intention. Nevertheless, whilst no explicit claim to anything other than an empirical basis for the model has been made, it does implicitly invite at least tentative extrapolation. The model's fusion of social context and human agency (that Giddens sees as essential) allows it much more than a purely relativist status - even whilst denying the possibility of any absolute basis in theory. Extrapolation allows the model's robustness to be tested.

This section tries to provide a practical test by looking at the alternative data and theory provided by Latour (1987). Latour discusses the way science and technology 'work', and shares something of the present research's sceptical methodological stance. Comparison of his model with the model suggested in this thesis allows a critique of both.

Latour starts with several 'flashbacks' to look at the early days of innovations that are now accepted as 'black

boxes' - he sees controversies and people doing what they were not supposed to be doing - "two pictures .... utterly different" (Latour 1987:4). In a similar way, in the four-discourse model different views emerge from each side of the boundary being crossed - the boundary where, in Latour's phrase, "context and content fuse together".

Latour introduces the 'dissenter' who has to be convinced by scientific literature, which seeks to close the black boxes - to create facts accepted as 'true', or machines that are acknowledged to 'work'. Rhetoric is central for Latour; he describes the scientific article as "the most important and the least studied of all rhetorical vehicles" (Latour 1987:31):

> Fact construction is so much a collective process that an isolated person builds only dreams, claims and feelings, not facts. (Latour 1987:41)
>
> The power of rhetoric lies in making the dissenter feel lonely (Latour 1987:44)

His ideas here suggest, surely, both the industrial discourse of science and a flavour of a moral discourse.

If scientific papers fail to convince, then the next line of defence is the laboratory, where their devastating figures are inscribed, and spokesmen and women can point to the vast resources that underlie them (Latour 1987:ch.2). But, counter-laboratories may be set up to challenge them - a situation clearly echoing the battle over HOTOL in public discourse, where BAe's moral rhetoric was pitted against Rolls-Royce's financial rhetoric.

Latour presents a similar fine case study, the diesel engine. Diesel managed to push from his moral discourse into the industrial discourse of the MAN company, and presented an engine to the public[6]; it had problems, the idea failed to gain acceptance in public discourse, it failed to entrain financial discourse as Diesel's bankruptcy evidences; his breakdown followed. A decade or so later all the discourses did eventually come together, and the engine was eventually a success, but Diesel committed suicide.

Latour's treatment of the study is subtle, and he isolates some germane points, most notably the problems of seeing innovation in stages - as a 'trajectory'. He is worth quoting at length:

> A series of terms are traditionally used to tell these stories. First one may consider that all diesel engines lie along one 'trajectory' going through different phases from ideas to market. These admittedly fuzzy phases are then given different names. Diesel's idea of a perfect engine in his mind is called 'invention'. But since, as we saw, the idea needs to be developed into a workable prototype, this new phase is called 'development' .... 'Innovation' is often the word used for the next phase through which a few prototypes are prepared so as to be copied in thousands of exemplars sold throughout the world.
>
> However, these terms are of no great use. Right from the start, Diesel had an overall notion not only of his engine, but also of the economic world in which it should work .... So no clear-cut distinction can be made between invention and innovation. .... On the contrary, making separations between the phases and enforcing them is one of the inventor's problems: is the black box really black? (Latour 1987:107)

Latour might seem to raise potent objections here to the four-discourse model. After all, it deals in stages - each time an

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[6] In the very same month as the Spithead Review.

inventor pushes an idea across a boundary into a new discourse, a new stage in innovation is reached.

However, in no way do the discourses impose stages on an idea - they can be traversed in any order, and ideas can start in any of them. Discourses are simply shared ways of talking about a project, they are merely the linguistic conditions in which the notion of stages, however erroneous, can arise - not that notion itself. Latour supports such a reading when he goes on to imply that the roots of such terms are to be found in discourse itself - financial discourse:

> No matter how clumsy these traditional terms are in describing the building of facts, they are useful in accounting, that is for measuring how much money and how many people are invested (Latour 1987:108)

It would be tempting to proceed as if nothing had happened here, but a small element of Latour's doubt does remain - a doubt that the next chapter will take up again.

Even though the four-discourse model trembles a little at this, it still seems a simpler and more comprehensive way of proceeding than the taxonomic approach that Latour goes on to develop. He proposes a detailed taxonomy, admittedly impressionistic, of how inventors 'translate' (in his significant word) their own interests into interests that others can share. Latour ends this 'stage' of his development with a well drawn critique which shows the inadequacies of a deterministic idea of diffusion that sees behaviour being caused by the 'discoveries' of a few supermen - of geniuses who 'have ideas'.

The four-discourse framework, concordant as it is with Giddens's duality of agency and structure, is in accord with Latour at this point. Science is just one sort of shared industrial discourse amongst many others - its relation to behaviour, much less to action, is not causal but recursive - its 'truth' merely local. Latour shows how the distinction of scientific 'insiders' from 'outsiders' who lend support is a false dichotomy. The 'dirty reality' is that scientists are just as much mobilized by their, largely military, patrons as the other way around, <u>but</u> Latour adds a caution:

> If we are ready to doubt what scientists say about science, it is not so as to believe what generals, bankers, politicians, newsmen, sociologists, philosophers or managers say about its limit, shape, usefulness or cause of growth. (Latour 1987:175)

The four-discourse model shares this view of multiple discourses, none with access to an absolute 'truth', however, it most certainly does not accord with Latour's assertion that:

> since the settlement of a controversy is the <u>cause</u> of Society's stability, we cannot use Society to explain how and why a controversy has been settled (Latour 1987:144)

The symbolic resources that define the boundaries of stable communities often <u>consist in</u> controversy itself.

Latour goes on to look at the networks of ordinary people that support science and technology. Scientists invoke 'rationality' as a basis for criticism of other points-of-view; this he sees as a mere rhetorical trick: "no one on earth is durably rational" (p.191). In saying this, Latour gives himself an insuperable philosophical problem - it leads

to the admirably postmodern, but confused, conclusion that rationality is purely relative. This denies Latour the philosophical right to claim anything at all. The four-discourse model accepts that rationality may be traduced in rhetorical use, but circum-navigates the shallows that Latour enters by seeing relative local truths within a broader framework of discourses, where the framework itself remains rational. Latour's comment here is particularly apposite: "We are struggling to live in different worlds" (Latour 1987:198).

There is still much to stimulate debate in Latour's final chapters on the growth of networks. The scientists' solution to outside 'irrationality' is to bring back what is observed by making it mobile, stable and combinable. Science thus constructs space and time - museums, maps and suchlike are tokens of this. But "the true heart of the scientific networks" are equations which allow the information to be assimilated by the 'centres of calculation' at the core of the networks. All this demands the immensely expensive business of metrology - of maintaining standards - and this is controlled through the shuffling of paper:

> Going from 'science' to 'technology' is not going from a paper world to a messy, greasy, concrete world. It is going from paperwork to still more paperwork. .... Those who would try to replace the common history of these centres of calculation by clean distinct histories of science, of technology, and of management would have to butcher the subject. (Latour 1987:253)

All of modern bureaucracy, accountancy, government and the 'administrative sciences' are enabled by the same fragile

networks. It is this 'paper shuffling', Latour says, that makes society stable.

All this will not really do. A project like HOTOL is certainly paper-based and fragile - but it is so because it is at every moment exposed to the chances and vagaries of each of the discourses that compose it. HOTOL is not, as Latour would have it, hermetically sealed against the outside world within a network - it consists in all the discourses of that outside world. Latour's problem becomes plain when he has to issue dire warnings about what happens when his networks are punctured, yet at the same time has to assert that those very networks get support and resources from the outside world. His emphasis upon metrology is not a satisfying resolution of this problem. Moreover, its control by small, evidently élitist, centres of calculation sits ill with his distaste for the diffusionists' 'great man' view of science.

The four-discourse model is a considerable aid in understanding both Latour's data and the problems of his own model. Its attempt, like Giddens, to combine structure and process emphasizes that a model based upon networks must not only theorize the relationships that bridge network boundaries - it must also consider the structures that are bridged as well. Latour's insistent relativism means that his model eschews structure as scientific, and therefore out-of-bounds[7];

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[7] Analogous to linguistics' problem of discussing language in language.

he is thus forced to look only at process - at a flow along a network, or the manufacture of 'black boxes'.

Nonetheless, Latour's is still a superb process model, and points to considerable problems in the process elements of the four-discourse model. Cooper (1989:493-494), in his discussion of Derrida, is surely right to note the postmodern character of an earlier book of Latour's. The problems in Latour's analysis are the problems of many postmodern critiques - problems that Derrida avoids through his rigourous philosophical stance, as the next chapter will try to show.

7.6 Epilogue - Where are the Boundaries?

This chapter has tried to use the rough sort of model that can be assembled from four discourses. The four can be seen as the faces of a tetrahedron, a triangular-based pyramid, across the surfaces of which the course of an idea can be traced. Seen in this topological form, each discourse is joined to every other. The idea becomes associated with different discourses in turn, crossing between them each time it meets a limit to its growth. It uses the resources of the people who share a discourse in order to grow, and in the process supports that discourse - allows it to recreate itself. The whole pyramid thus grows.

This suggests why discourses and their boundaries are so well described in the data. In order to cross a boundary into a new discourse, people have to cajole, persuade, and lobby those in the neighbouring discourses. They have to bring them

around to, quite literally, 'a new way of thinking' - it is the effort involved in this that they talk about in the case-study interviews.

Plotting the examples of the case-studies onto the 'model' has been broadly successful in testing it; all of its suggested properties have been seen. The model has aided understanding, but it also allows thought experiments in innovation. An idea may emerge as the life's work of an impassioned inventor in a moral discourse, as a niche perceived in some market by an industrial organization, as a new type of financial instrument, or a government project. Its next step can be into any of the other discourses, facilitated by reasoned commercial argument, moral pleading, a publicity coup, or government fiat, and so on.

An innovation needs all four discourses to succeed - success is an advance in all of them at once. To fail, however, an idea only needs one discourse to block its further growth - an idea of any potential has, sooner or later, to escape the confines of a discourse that binds it. Sometimes this is easy - the boundary might be internalized within a large company for example. Sometimes it is not - industry may not be able to handle a large project without government or international support. Discourses help to show why things do not happen, as well as why they do.

However, the model also has a few problems - it is almost too capacious in its ability to incorporate events, comments, or problems. They seldom seem to sit neatly in only one discourse, or to straddle just on boundary, the other

discourses are never far away. This is why Latour's critique of the notion of 'stages' is so powerful - an idea does not move from discourse to discourse in neat progression, to succeed, it must inhabit all of them all of the time.

This paradox of the idea moving over the surface of a tetrahedron of discourses, but also being the tetrahedron itself, is the paradox of structure and process. Giddens shows how the routine of people's discourse and the process of their agency in innovation are bound together in recursively and reflexively re-creating each other. Innovation must itself be a compound of all the discourses in which its own compound language is understood.

Moreover, and more obviously, the people who innovate also share more than one discourse. The people who seem most active and successful in innovation often seem to be those able to straddle boundaries - to have a foot in two camps, or even three or four. 'Three-discourse' people emerge from the case studies - Alan Bond, Fisher, the providers of seedcorn, the entrepreneurs they fund. Perhaps the very confusion in the interviews over what an entrepreneur is, has its explanation here - an entrepreneur is a three-discourse person. Three-discourse people internalize paradox, just as a company internalizes a boundary between industrial and financial discourses. That is what gives them their facility in crossing boundaries.

The next chapter will ask how all this can happen. It will focus on innovation across time as well as space. The

dimension of time has thusfar been used rather uncritically, yet notions of crossing boundaries, or passing through stages, have already caught something of the cyclical periodic character of the short-term growth of an innovation. Historical examples have shown something of the long-term growth of institutions, as well as the way they can change out of all recognition in the process. The Templars changed from protectors of pilgrims into the central bankers of England and France. The East India Company changed from a loose association of adventurers into a pseudo-state. Merchant banks changed from trading concerns into aristocratic institutional intermediaries.

Chapter five strongly suggests parallels here in the recent history of funding innovation - venture capital arose on the one hand from an attempt by government to fill an 'equity gap' and on the other from 'business angels' getting together, but now it looks something more akin to traditional investment banking or fund management. It is tempting to see the seed capital 'movement' as a group of innovators, who, frustrated by the institutionalization of venture capital, have set up their own brand of financial discourse - as three-discourse people caught in the long-term act of becoming a part of financial discourse themselves. But even they are not the latest in this long line - Alan Bond, the lonely innovator in chapter four, described his considerable efforts to use, even to invent, new financial instruments, with which to support HOTOL.

CHAPTER 8 : THE TRACE OF A NEW IDEA .... DIFFÉRANCE IN TIME AND SPACE

> The endless cycle of idea and action,
> Endless invention, endless experiment,
> Brings knowledge of motion, but not of stillness;
> Knowledge of speech, but not of silence;
> (T.S. Eliot 1974:161)

8.1 Introduction

The research into innovation described here produced a mass of data. Chapter six showed that it was productive to see areas of shared human experience as discourses, and how the symbolic boundaries between them could be teased out of the data. Chapter seven introduced human agency, underwritten by the radical sociological ideas of Giddens, to the discourses, and showed how they are created and maintained through the routine of recursive social activity, and how they are implicated in the actions that further people's innovative projects.

A sort of model of the innovative process has thus been created, but this model is still unsatisfying. Chapter six presented a structural model that, nonetheless, hints at process; chapter seven presented much more of a process model, but one that invokes and uses the structures of chapter six. The problem is that, like many other social science, organizational, and management models, process and structure are not fully articulated (a good example here is the ubiquitous metaphorical approach well illustrated and argued by Morgan (1986)).

Such models either reduce structure, willingly or not, to a thing (Morgan's metaphors of organism, machine, brain or whatever), or, like Latour and the postmodern social critics, they are forced, knowingly or not, to throw out rational structural explanation altogether. In the first case, human agency - the motivation of process - is excluded in precisely the way that a neural model of the brain itself might exclude thought. In the second case, by studiously de-privileging every discourse (except perhaps their own speech), nothing is left to afford the analytical leverage needed to investigate human agency. Even Giddens, in his elegant and sophisticated attempt to resolve this problem has constantly to reassert the duality of structure and agency.

This chapter, then, tries to articulate structure and process. Such a project must try to bridge the schism between the social and the psychical elements of human action - a schism, perhaps, between the successors of Durkheim and Freud. It may be no accident that a Saussurean thread has been followed in trying to build this bridge; after all, Lévi-Strauss turned to Saussure to push beyond a Durkheimian cultural analysis, as Lacan did to reclaim the purity of Freud's insights.

Structural linguistics, at first sight, seems an improbable basis for such radical theoretical developments. Yet, like so much in the gaudy sparkle and carnival of French structuralism, it is firmly rooted in common perception and experience - strip away the display - and structuralism, quite simply, makes sense of day-to-day life. After all, language

does not neatly separate structure from process, its study forced Saussure rigorously to theorize the 'langue-parole' distinction, rigorously to conceptualize time itself. The fecundity of his semiological successors consists largely in their refusal to elide structure and process (or at least to feel very guilty when they do). They press far beyond Saussure's vertical hierarchy of signification (phoneme, word, sentence - phonetic, semantic, syntactic structures), to see a horizontal anarchy wherein the chain or trace of signification is supreme. If language is the map, then a trail of signification is the route that meaning itself takes - the route by which people make sense of their world. That the same signifiers mean different things to people at different times shows that different discourses are implicated.

The last two chapters suggested innovation proceeds through four discourses - <u>what</u> happens. This chapter tries to answer <u>how</u> it happens. This chapter suggests that, to be faithful to the data of chapters four and five, it makes the most sense to see innovation as a trail of signification - theorized in Derrida's rigorous terms as a disseminated trace - within the set of four discourses.

Lévi-Strauss saw contradictions being resolved through the 'slated' structure of myth, meanings changing little by little through a succession of different versions - yet all of those versions of the myth co-existing for the anthropologist to collect. Similarly, the passage of innovations through discourses seemed to show diachronically changing meanings,

but at the same time, 'synchronically' as it were, innovation existing in all of the discourses all of the time. The appendix to this chapter shows how the analysis of the interviews drives towards this conclusion that people spoke in all of the discourses. The innovative idea has multiple meanings all of the time, even in the minds of the individuals who support or oppose it - it is only the fact that different meanings become prominent at different times that creates an illusion of change.

The rest of this chapter looks at the data, and at the ideas of Giddens and Derrida, to investigate how people come to terms with multiple meanings - how they innovate. This investigation must end with the thoughts and actions of individual people, but it must begin with the institutions that result from such actions over large expanses of space and time.

8.2 Differences? in Space and Time .... The 'Longue-Dureé' of Institutions

8.2.1 INTRODUCTION - CONCERNING LEVELS-OF-ANALYSIS

The problem upon which the development of this thesis has come to rest - the problem of a synthesis of structure and process - is a problem with the whole idea of levels-of-analysis. A 'level' is something of a structural construct which rather erodes the processual aspect of human activity; choosing just one level tends to exclude others. Considering four discourses, none of which is tied to any particular

level, has emphasized this - at any chosen level, all of the others clamour for consideration.

Some modern management scholarship has faced, but not solved, this problem. Open systems and contingency models represent, perhaps, its most sophisticated formulation. Morgan's (1986) alternative solution is to propose multiple analytical paradigms, each rendering organization within a different metaphorical structure and thereby, in aggregate, drawing in the different levels that a unitary perspective excludes. Morgan's insuperable problem is that the required practical aggregation of paradigms and levels is never theorized. Allison's (1969) much earlier use of multiple paradigms in political science is interesting here - it suffers from the same lack of synthesis as Morgan's attempt, but Allison's 'conceptual models' have clear affinities with the discourses proposed in this thesis.

The model that most nearly overcomes the problem, as the last chapter suggested, is the sociological formulation of Giddens (1984). Giddens squarely faces the need for, inevitably structuralizing, categorization into levels-of-analysis, but reformulates it under his rubric of the 'duality of structure' which prevents one particular level ever being privileged, and accords primacy to neither structure nor process. Giddens's key distinctions for the present discussion are between the reversible 'dureé' of day-to-day life, the irreversible time of an individual's life-span, and the 'longue dureé' of institutions - in reversible time once again (Giddens 1984:35-36). Giddens introduces these not to

classify, but to analyse how people's worlds are created and sustained - how levels-of-analysis intermingle:

> All social systems, no matter how grand or far-flung, both express and are expressed in the routines of daily life, mediating the physical and sensory properties of the human body. (Giddens 1984:36)

Giddens's ideas, broadly outlined in section 2.4, will be used again here as a framework that allows consideration of what he calls institutions ("Those practices which have the greatest time-space extension" (Giddens 1984:17)) without falling into the chasms that the unwary dig to separate their levels-of-analysis.

From a classic level-of-analysis viewpoint, the data of this thesis scarcely looks institutional at all - interviews and letters often consist in intimate and personal comments. However, from Giddens's viewpoint these can be seen as clear institutional outcroppings. The few extracts that follow are tokens of the much fuller data of the interviews that even chapters four and five could only hint at. The interviews themselves are in turn only tiny windows into the institutionalized discourses that comprise people's thoughts and innovations.

The rest of this section tries to show something of the pervasiveness through space, and persistence across time, that underlie these tiny tokens of data. This is a caution against the academic temptation to set them immutably in one level-of-analysis - a temptation characteristic of some of the discourses themselves, as an ironic comment of John Scott-Scott's shows: ""how could this have grown up with two blokes

at home?"". Once again, the problems of the researcher and the researched come together, providing some encouragement that the research might prove useful.

8.2.2 HERE & THERE

This sub-section draws upon people's comments about some of the most persistent social institutions of all - perceptions of national differences. Views about such differences, particularly those between the UK and US, were often expressed, for example:

> "Americans are much more optimistic, entrepreneurial, ... willing to take a chance on someone unproven".

The interviews in New York were an attempt to question such opinions, and tease out the symbolic from the material in them. They were not, of course, intended to provide definitive answers, what they have succeeded in showing is that assertions of national difference are so pervasive as to suggest they have a symbolic character[1] - that they are, in Giddens's phrase: "a major institutional locus of ideology" (Giddens 1984:33). Indeed, one interview playfully sensed an ideological dimension here: "We may have, of course, bought an American PR exercise". The examples that follow, then,

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[1] This in turn, of course, warns of the extreme difficulty that any definitive assessment of the 'reality' of such international differences would involve.

offer a window upon the ways in which the longue dureé of institutions is recursively sustained[2].

A first example of trans-Atlantic differences is the view of bankruptcy in UK interviews, noted in chapter five. Concisely put, the assertion is that, unlike Britain, no stigma attaches to bankruptcy in the US, thus there is a greater willingness to accept the risk of financial failure. A strange symmetrical inversion emerges here, in the feeling perceptible amongst the UK 'technology community' that, even if financial failure stigmatizes, failure in technological projects is a fire that hardens resolve. The cancelled Blue Streak project exemplifies such a boundary symbol, as does Parsons's remark: "'Well, we have the satisfaction of having bust it'" (recorded by Rayleigh in Parsons 1934:xxvi). The way that this shared technological 'failure culture', as it might be called, seems to strengthen resolve, suggests why the symbol of bankruptcy was important. Both are implicated in the longue dureé of the shared institutions that constrain and enable innovation.

A second example is the seemingly explanatory and straightforward assertion of a 'British disease' affecting inventors, the symptoms of which are a perverse formula along the lines that one hundred percent of failure is better than ten percent of success:

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[2] Giddens own use of the City of London as an example of the development of institutions is worth noting here (Giddens 1984:319-327).

> Twenty five percent of ten million is more than one hundred percent of one million
>
> "Now, in America, you don't get founders wanting to keep fifty-one percent - a British disease".

The symbolic nature of this fine example was exposed at the end of chapter six. Financiers recursively repeat such seemingly unexceptionable formulae in order to sustain their discourse's prescription that control of the innovations they fund <u>should be</u> ceded to them by inventors.

A third example seems related to this - an olive branch to the poor UK inventors perhaps, that symbolically provides a cultural explanation for their plight, and so removes blame from them <u>and</u> from those who finance them:

> The US "is a society that has a far greater respect for engineers, scientists and technologists"
>
> "there is more national pride about technology"
>
> engineers and technicians are regarded as equal to doctors and lawyers

The importance of these examples is not their substantive accuracy <u>but their pervasiveness</u> - their ubiquity underlines them as symbolic, and implicated in the process of maintaining institutions and discourses in action. This is perhaps seen most clearly when they become full-blown attempts at cultural explanation - statements of the full aetiology of the 'British disease'. Just such explanations are to be found in interviews from both sides of the Atlantic:

> Peoples' "ancestors had to make an entrepreneurial decision to up sticks and go". ....
>
> "In Europe you just have a history of hierarchy", ... a view of foreigners. "The US <u>is</u> foreign" - a mix of races.

> In the old world there is a collective memory of not trusting foreigners, wars - it is psychological.
>
> people both love and hate the British inheritance - remember that people came here to get away from it.

The last commentator also observed, with admirable rhetorical economy, that it ""all goes back to George III""! The entry of an historical strain into this last set of quotations serves to emphasize what Giddens sees as the "prevalence of historicity" (p.203) in modern institutions. Such quotations, then, illustrate the 'longue dureé' of institutions.

8.2.3 NOW & THEN

The last sub-section, like chapter six, showed the pervasiveness of national cultural differences as boundary symbols, but it went further than chapter six in connecting the maintenance and use of these symbols with the sort of mechanisms suggested by Giddens. This sub-section will take the temporal aspects of Giddens's 'longue dureé' one step further by looking at the persistence of boundary symbols through time[3]. This will allow later sections to consider the links between 'longue dureé' and 'dureé'. It was with such investigation in mind that the 'historical' Parsons's case-study was included in this research.

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[3] The present research continues to emphasize symbolism more than does Giddens (as discussed in previous chapters). Where he sees symbols as "one main dimension of the 'clustering' of institutions" (Giddens 1987:32), they are regarded here as the main dimension.

Examples from the data emphasize the long timescales of the discourses implicated in innovation. The recurrence of the problems, explanations and excuses surrounding innovative activity is startling - as two formulations of the problem of measuring the value of ideas, nearly a century apart, show:

> And the benefits which the world reaps from this originality are apt to be underrated. For they do not come all at once like those gains which a large business reaps by utilizing existing knowledge and well proven economies; but they are cumulative, and not easily reckoned up. (Alfred Marshall - 1890, Quoted by Prais 1976:21)

> It is something to do with us here, that we start on certain programmes, then we get into a great panic about them, start saying 'We can't see where they are taking us' or 'What's going to happen? The costs are going to be terrible', and we come out of them. (Geoffrey Pattie - Lords 1988:para.764)

Attempts at explanation likewise persist over a century:

> Unfortunately foreign accidents and failures were kept quiet while British mishaps were 'advertised with as much persistence as Pears' soap'. (Mackay 1973:200 - quoting Fisher)

> "The Brits have a habit of shouting their failures from the rooftops"; the US and Germans have them too, but they just get out of them and do not make a fuss. (finance interview)

Other explanations of the failure of innovation noted in the last sub-section, like the inadequacies of inventors, also had historical parallels:

> unfortunately the best inventors are often bad men of business. (Parsons in Parsons 1934:46)

> the biggest problem has been a lack of commercial awareness/management among those who have good technical ideas (finance interview)

The ways that people use rhetoric in the boundaries between moral and public discourses have very clearly not changed either:

> We cannot afford to lag behind merely for the sake of a few thousand pounds for present experiment. The opportunity will quickly pass, and no expenditure may help us to recover our lost ground.
> (Statement advocating formation of the 'Association for the Promotion of Flight', established in 1907, of which Parsons was a founder member - Appleyard 1933:67-70)

> the way forward for Europe must be a 'go-for-broke' one. The chances of failure are small, for only the unimaginative can believe that mankind's destiny lies on a small overstrained planet in a Galaxy of plenty. (Bond 1988:15)

Some of the most striking similarities across time are seen when people talked candidly about their actions in manipulating opinion, and about how they created rhetorical façades around those actions. Two journalists talk of their 'contacts':

> 'I used to meet him, as he said, like Nicodemus, at night, in all sorts of out-of-the-way places'
> (Stead, a sensational journalist, talking of Fisher - quoted by Mackay 1973:179).

> I had a group of informants - they came to my office, we sat and talked and drank cans of beer from my fridge; I remember it got quite dark. (HOTOL interview)

Two organizational politicians talk of their committees:

> FISHER - 21-08-1904: I had got the designs out of what had to be; but it was the politic thing to have a committee of good names (MI:325)

> "I thought there were precautions to be taken. .... We set up the academic groups". "To get the best brains, ... a bit of backside padding" to show we had got the best advice at the time. "In part, a bit of a political point .... if the chips come down". (HOTOL interview)[4]

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[4] Even the phrase 'best brains', is also used by Fisher in other letters.

These last two extracts are particularly relevant in this discussion - they show the cycle of longue dureé's reversible time in action. Committees are the stuff of industrial discourse, and for a new idea to become institutionalized, it has to be seen as the product of a committee, even when it predates the committee - thus 'The endless cycle of idea and action' turns, and time is reversed.

Giddens's discussion of the longue dureé of institutions explains how an innovative discourse recursively grows to become an institution itself. Giddens shows why it must incorporate elements of the institutionalized ways of talking about the world that the four discourses proposed here represent. The innovation's originators can then marshal and combine the resources to which those discourses give access.

This section has shown how the longue dureé of institutions and discourses is seen in the ways that people talk about innovation. An example from 1906 has an uncanny resemblance to the world of today - all four discourses seem to have changed little:

> The worried manufacturers blame the capitalists for not finding money, the men of science declaim against the manufacturers for not doing pioneer work, ... and the educationists scold everybody for not building more technical schools. On the other hand, capitalists are blamed for over capitalising because dividends are small, and the men of science are told they do not know anything except theory, and are of no real use in the workshops. ('The Times' 20-10-1906, quoted in Michie 1988:509)

8.3 Day-to-Day Discourse and 'Real-Life'

> FISHER - 22-2-1905: Mr. Barnes, the greatest Editor 'The Times' ever had, used to tell his staff that '<u>Repetition was the Soul of Journalism</u>'! What he meant was that the British Public required the same thing (if it was a good thing) continuously and persistently put before them, though in different language, till they got impregnated with it and felt it as their own, with the consequent result called 'Public Opinion'! I think my phrase is better than the Barnesian: '<u>Reiteration is the Secret of Conviction!</u>' You have only to keep on saying the same thing long enough and everyone will believe it! 'Pears' soap is the best' - at last you buy a cake of the beastly stuff! (MII:51)

This section will set the scene for the next. It continues this chapter's theme of investigating how innovation comprises elements of all of the discourses all of the time. The last section provided support for Giddens's sophisticated way of meeting these same problems - that recursive use of discursive resources continually re-creates the discourses themselves; as Fisher says, 'reiteration <u>is</u> the secret of conviction'. The picture to emerge provides a rather more messy view of innovation than the neat topological model with which discussion started. That model has not been discarded, but its structural character has been leavened with the processes of 'real-life'.

This section looks at 'real-life'. The last five chapters have successively teased apart the texts of the interviews and letters, seen the underlying skeletal structure of the body politic of innovation, revealed the flows of its life-blood and the processes of its growth. A knowledge of stillness has resulted, not an understanding of motion. The time has come to put these fragments of understanding together

- the individual experiences and the individual discourses that comprise the collage must somehow be set in motion.

Giddens provides resources for this struggle with the seemingly un-social-scientific notion of 'real-life'. His conception of "belief claims" - ordered as discourses, provides a good starting point:

> Characteristic of most common-sense, everyday claims to knowledge is that they are formulated in a fragmentary, dislocated way. (Giddens 1984:92)

Far from being un-social-scientific, there is a social science tradition of common-sense theorizing. Ethnomethodology is particularly relevant here, as Turner's quotation from Caton shows:

> 'whatever technical language a person may acquire is, and as things are, has to be acquired against the background of ordinary language' (Turner 1974:8)

In French structuralist thought, as well, Barthes's idea of jouissance catches perhaps the other extreme of real-life, and Lacan's introduction of the 'real' (as one of the three orders with which he thinks about the mind) is particularly pertinent, as Bowie (1991:ch.4) discusses:

> The network of signifiers in which we have our being is not all that there is, and the rest of what is may chance to break in upon us at any moment. (Bowie 1991:103)

The real did break in upon the discourse of interviews and letters at times. Parsons and Fisher provided good examples in the way that their personal experience overbore the boundaries of the shared discourses that surrounded them:

> PARSONS - 20-2-1897: It has been blowing a whole gale for the last 10 days. We went out on Friday in the midst of it. .... We got drenched and had to turn home on account of a blowing joint and losing our fresh water,

and also to get back in time for the meeting at 2. (A:132)

PARSONS - 7-3-1897: I hope you will excuse the scrawl. Fingers are cold and raw with runs. (A:135)

FISHER - 27-06-1902: It's everything for the Admiral and much for everyone else this getting together and keeping together of a mass of ships. The difficulties increase in more than geometrical progression, and this is not understandable except by actual experience. When a fog comes on you suddenly, as we once had last year, all hands have to brace themselves up! and the unexpected will happen even at sea! (MI:250)

Each of these loudly makes the 'real-life', the 'actual experience', of developing innovations stand out from the routines of day-to-day discourse. Something of the same sense came across in the interviews with the originators of other potential innovations like HOTOL.

It may seem perverse to reintroduce Derrida at this point, especially given his battle with the 'common-sense' view of the 'British school' of philosophy (Norris 1987:ch.7). However, there is a subtle distinction here that begins to show Derrida's relevance - the distinction between the ordered, and quite literally common sense (in that it is shared) of a discourse, and the 'real' extra-discursive elements that occasionally irrupt within it, as they did in the letters of Parsons and Fisher just cited.

Such irruptions cannot be theorized away as just another discourse - the evidence suggests that their character is different, as does Lacan's need for a distinct 'real' conceptual aid to thought. Derrida provides the way out - he refuses to read a 'common-sense' philosopher like Austin in

terms of Austin's own unitary, and closed, philosophical discourse:

> Derrida assumes, on the contrary, that the most revealing passages of Austin's argument are those where his choice of metaphors, parables or casual locutions is such as to create real problems for any close reading of his text. (Norris 1987:177)

The irruption of the real within a discourse shakes it in precisely the same way that Derrida's use of an undecidable word like 'pharmakon' makes Plato's text tremble and deconstruct itself. Derrida theorizes <u>how</u> 'real-life' disrupts any conception of a regular and ordered discourse - <u>how</u> 'parole' implicates not just the orderly 'langue' of spoken language, but also the chaos and excess in his arche-writing.

Derrida might, therefore, allow the synthesis of the four discourses that ultimately escapes even Giddens, as chapter two hinted. Chapter two stands as a reference for Derrida's ideas throughout the rest of this chapter, which will now re-consider, in the searing light of these ideas, the model of the last two chapters.

Derrida opens the spacing or interval of différance by levering discourse open at points where its meaning is undecidable. Such points allow fleeting access to the excess of the discourse, to the excess of all of the four discourses suggested here. In this excess other meanings are possible and, as one of the interviews put it: ""things don't have to be the way they are"". Such points mark an old meaning and a new meaning, perhaps an old discourse and then a new

discourse, but the spacing where meanings change is outside of space and time. Spacing is only perceptible through Derrida's double mark, and only a double mark is possible - the sign cannot be separated, signifier from signified. The way meaning changes, and changes all the time, is through the disseminated trace of all the double-marked intervals.

Deconstruction, when described thus, powerfully illuminates the model of the last two chapters. Spacing is a way of thinking about crossing the boundaries between discourses. The problem with the four-discourse model, and other models, is that they theorize discourses, but do not theorize anything outside of those discourses. Derrida's spacing - not outside, not beyond, but within, the writing of discourse - is where discourse comes together with its excess to forge the trace of meaning.

The irruptions of 'reality' noted above can be seen as excesses - as glimpses of Derrida's arche-writing where the actions of agents are written, and where these in turn write the trace of innovation. The signs and symbols in innovation's trace need all of the discourses if they are to be understood - such is the intertextuality of innovation. The boundaries where the symbols outcrop should therefore be just those points where the Derridan lever might be inserted to deconstruct. They should all have elements of undecidability about them. A return to the boundaries of chapter six is clearly overdue.

The first boundary considered was that between moral and industrial discourses (sub-section 6.3.2), where the symbols noted were none other than ideas of madness and of fun - ideas Derrida himself discusses in his readings of Foucault and Lévi-Strauss as section 2.5 described. The second boundary (6.3.3) was between industrial and financial discourses - the symbols chosen as examples included the procedures of finance, ideas like risk and reputation, and the allegations of short-termism. The discussion of these showed that in every case there were strong elements of undecidability. People seem to use short-termism to mean what they want in debate, and risk and reputation serve as tokens, in financial procedure, for things undecidable in monetary terms. As one of the comments said: ""It's a real guessing game""!

The boundary between moral and financial discourse (6.3.4) was marked by madness once again, and the undecidable difference between investment and 'punting', together with ideas about inventors, pay and the 'equity gap'. Inventors are fine Derridan levers - someone suddenly having an idea from nowhere smacks of the metaphysics and 'writing-in-the-soul' that he so incisively challenges, whilst their distaste for money returns to the undecidable paradox of money versus value. The 'equity gap' is a phrase that might delight Derrida himself, whilst the word 'equity' is surely ripe for deconstruction. Does the inventor see a lack of equity in the financiers' refusal of funds for seedcorn, or a lack of equity? Given the absence of equity from this stage, should the people act?

The boundaries around public discourse also invite Derridan readings - that with moral discourse (6.3.5) took Fisher as its spokesman, and Fisher's letters could never quite decide what publicity meant - assistance or hindrance. Frank Miles's musings, on whether publicity might hurt his contacts, hint again at the signifier 'publicity's' power as "a positive lever" as Derrida puts it. HOTOL, too, was undecidable in early discussions with government - was it a 'proper' project, or the jottings of ""two blokes at home""? Sub-section 6.3.6, looking at the public-industrial discourse boundary, provides yet more illustration in its discussion of what publicity means. Peter Conchie and others in BAe all seemed quite undecided whether the 'pharmakon' of publicity was cure or poison.

The final sub-section of chapter six (6.3.7) used the boundary between financial and public discourse to draw all the elements of boundaries together - people sometimes talked about them in a matter of fact way, saw them as simple obstacles at other times, and at yet others used the full symbolic grandeur of national cultural stereotypes. These viewpoints are all amenable to Derrida's 'overturning', where, as he says: "one <u>opposes</u>, insistently, the pole of the signifier to the dominant authority of the signified" (Derrida 1987a:82). The pole of a national stereotype quite evidently combines nonsense and meaning at one in the same time, just as every way of seeing a boundary can. This section has shown that Derrida's deconstructive readings can use them all as a way of overturning discourse.

To demonstrate the first step of deconstruction in this way - by using the boundary symbols of chapter six - is almost too easy. Just as anything can be symbolic, so anything can present an entry to a deconstructive reading. In fact Derrida shows how the discourses of this thesis deconstruct themselves at the hands of those who share them. This encapsulates the importance of his ideas here - he theorizes how 'real-life' continually breaks in upon the routine of day-to-day discourses, how the traces of innovations switch between discourses. He might also help to explain how the people involved in the longue dureé of the institution of innovation can think in all of its discourses at once, as the preceding sections, chapters, and the appendix to this chapter, all clearly show they do.

8.4 From Giddens's Dureé to Derrida's Time of Writing

8.4.1 INTRODUCTION

The model of people creating and sustaining innovation is, as yet, incomplete - many questions regarding how they do it remain unanswered. The last section suggested that the theoretical discussions of Giddens and Derrida might provide answers to these questions. This section, the last major section of debate in this thesis, picks up the model where it was left at the end of the last chapter, and tries to resolve its problems. A very brief reminder of the development of the four-discourse model will be provided first.

Discrete world-views emerged from the data on innovation - each was characterized by communities where people shared experience, knowledge, lexicons of words, symbols and practices, together with a competence in using them. These were represented as four broad discourses, where individuals come together and exchange the resources their innovative activities need. Innovation itself becomes a compound of elements from each different discourse, its growth a successive crossing of the various boundaries between them. This, in turn, recursively reproduces and strengthens the discourses themselves.

The problem is that if the innovative institution is a compound of all four discourses, then its supporters must 'talk' their different 'languages', and to succeed they must be competent 'linguists'. The caricature of innovators as 'three-discourse people' was therefore introduced - well underlined by an interview comment like:

> US business plans, entrepreneurial teams, are much more rounded; there is more "functional diversity within a person" - marketing, technical, etc.. "Brits are only one function".

The hint of a national stereotype in such a comment also underlines people's need for symbolic explanations of the problems they perceive - their need for reassurance (for 'ontological security' in Giddens's apt phrase). It is this that motivates the production of symbolic artefacts around the boundaries of a discourse. The problem thus resolves itself into the question of how the innovators can maintain their

'ontological security', when it is so clearly threatened by their having to exist in all of the discourses all of the time.

The purpose of this section, then, is to investigate the innovator's problem, and to suggest theoretical ways in which it might be understood, so that the four-discourse model can remain a useful way of looking at innovation. Giddens, Lévi-Strauss, Sperber, and Derrida, all consider models of the mind that are relevant to this, and it is to these writers, and particularly to Giddens and Derrida, that discussion now returns.

8.4.2 DUREÉ

The discussion of Giddens in chapter two must be expanded here to look at his views of cognition and perception. Giddens's (1984:3) says that his conception of structuration "<u>begins</u> from temporality":

> Human action occurs as a 'dureé', a continuous flow of conduct, as does cognition. Purposive action is not composed of an aggregate or series of separate intentions, reasons and motives. (Giddens 1984:3)[5]

Thus he sees this most basic level of people's involvement in the world as continuous, <u>not</u> continual. However, perception, through all five senses, <u>is</u> continual - perception, memory and the unconscious are all closely linked says Giddens; the

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[5] Giddens (1984:202) discusses Bergson's concept of 'dureé' more fully, and also notes Lévi-Strauss's use of the idea.

unconscious, even innate, organization of perception through anticipatory schemata is the same as the mechanism of memory:

> Perception, then, depends upon spatial and temporal continuity, actively organized as such by the perceiver. .... Perceptual schemata are neurologically based formats whereby the temporality of experience is continually processed. Such processing may in turn be understood as inherently involved with the reflexive monitoring of action in general. (Giddens 1984:46-47)

Giddens conceptualizations here are closely argued, if a little slippery, and brief discussion cannot do them justice. Nonetheless, to see his relevance to the present discussion some attempt to outline the basic way in which he groups concepts together is necessary.

Reflexive monitoring is the basic level of Giddens's stratification model of the agent (Giddens 1984:5), upon which the processes of rationalization and motivation are overlaid. Giddens implies that reflexive monitoring is unconscious, and linked to routine and to the agent's ontological security, whereas the rationalization of actions depends upon the agent's 'knowledgeability', which is embedded in 'practical consciousness'. 'Discursive consciousness' allows the agent to put his reasons for action into words. Giddens proposes these three (a 'basic security system', practical and discursive consciousness) as an alternative model of the individual's psychic organization to Freud's 'id', 'ego' and super-ego'.

Giddens's clear articulation of his ideas of consciousness, the unconscious, and memory, are very relevant to the way in which this thesis uses the idea of discourse.

The first important point is that, like Derrida, Giddens rejects any idea of a 'present' - dureé is a flow:

> If the 'present' is not cut off from the flow of action, 'memory' can be nothing other than a way of describing the knowledgeability of human agents. If memory does not designate 'past experience', neither does consciousness .... express the 'present'. (Giddens 1984:49)

Giddens continues, presenting a set of definitions of central importance to the present discussion:

> If memory refers to this temporal mastery so inherent in human experience, then discursive and practical consciousness refer to psychological mechanisms of recall, as utilized in contexts of action. Discursive consciousness connotes those forms of recall which the actor is able to express verbally. Practical consciousness involves recall to which the agent has access in the 'dureé' of action without being able to express what he or she thereby 'knows'. The unconscious refers to modes of recall to which the agent does not have direct access (Giddens 1984:49)

The distinction between discursive and practical consciousness is not intended to be "a rigid and impermeable one" (p.7), but subject to such as socialization and learning - the basic distinction here is "between what can be said and what is characteristically simply done" (p.7).

So how do Giddens's ideas help to solve the problem of the innovative agent, beset by contradictory and paradoxical worlds? Giddens sees discourse as a 'mode of articulation of knowledge' (pp.91-92), although in the immediately preceding pages he considers Goffman's ideas of frames and frameworks[6]. The sense in which discourse is used in this thesis is

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[6] "Frames are clusters of rules which help to constitute and regulate activities, defining them as activities of a certain sort and as subject to a given range of sanctions." (Giddens 1984:87)

entirely compatible with both of these, although it is also clearly linked to Giddens conceptualizations of individual memory and of structures of signification. This, in turn, is consonant with Giddens's view of discursive consciousness as a 'mechanism of recall' - recall of discursive knowledge. Giddens thus offers the promise of integrating the social and the individual - discourses and the mind - and so explaining how people resolve the paradoxes of innovation.

However, at this point Giddens seems to limit both discourse and signification structures - he prohibits discourse from recall and use by practical consciousness, or in the agent's unconscious reflexive monitoring of action. This is the point of departure for the extended use of the notion of discourse in this thesis, from Giddens's limited use of the term (as already discussed in chapter two). What is needed to resolve the present difficulties, is to make the conception of discourse used here accessible to the cognitive level of Giddens's practical consciousness - then a way forward might be found.

Giddens's conception of structure is threefold, signification being only one dimension (along with legitimation and domination) (pp.28-34). He justifies this limited view of structures of signification by noting the problems of a structuralist tendency to elide or confuse its paradigmatic and syntagmatic dimensions (p.17). The extended sense in which discourse is used here does not do this - the paradigm of signifiers associated with a discourse is the basic resource for the syntagmatic associations allowed within

it. Moreover, although the present research will not develop the syntagmatic dimension, it is entirely conceivable that such a dimension might subsume Giddens's structures of legitimation and domination (or even power itself, which he sees as basic). Giddens himself hints at such a possibility when he says: "Symbolic orders and associated modes of discourse are a major institutional locus of ideology" (Giddens 1984:33) - indeed he has to hold it at bay by excluding symbols from his structure of signification.

The idea of discourse used in the present thesis does not deny itself the use of symbols, indeed it incorporates Giddens sense of a Symbolic order. Giddens's definition of discourse is finer than that used here, yet his trinity of structures is a more complex way of articulating human action within social systems. This necessity of some form of division to catch the observed variety of action and systems is accomplished, in this thesis, by way of the <u>four</u> discourses proposed, as has been illustrated in the last two chapters. The question that still presses, however, is: can these four discourses be melded with a controlled abuse of Giddens's idea of practical consciousness? Giddens almost invites such an enterprise when he says that practical consciousness: "is that characteristic of the human agent or subject to which structuralism has been particularly blind" (Giddens 1984:6) - he accuses psychoanalysis of much the same. He also sets up such a possibility in remarks like:

> The fixity of institutional forms does not exist in spite of, or outside, the encounters of day-to-day life but <u>is implicated in those very encounters</u>. (Giddens 1984:69)

Previous sections and chapters of this thesis have already abused Giddens in just this way by uncritically applying his ideas of institutional formation to discourse - and this has served to show the promise of pushing discourse beyond the confines of a purely structural idea, and allying it with his conception of practical consciousness. Again Giddens seems to endorse such an enterprise, and the use of multiple discourses in everyday language:

> linguists have very often sought to analyse semantic problems in terms of the 'internal' linguistic competence of individual speakers or by examining the properties of individual speech acts. But the 'closure of meaning' of the polyvalent terminologies of everyday language achieved in discourse can be grasped only by studying the contextual ordering of whole conversations. (Giddens 1984:71)

In the last section Giddens's view of discourse as ordering everyday belief-claims (Giddens 1984:92) was used to introduce 'real-life' into the discussion. His description of the everyday character of discourse as fragmentary and dislocated might thus seem to support the evidence from the interviews of something 'outside' of such a discourse. However, if, as desired, Giddens's conception of discourse is replaced by that of this thesis, and this given access to his other psychic levels, then a new explanatory power of his ideas is liberated for use on the data. People whose 'real-life' irrupted in this way might be seen not happily to be waving within the sea of Giddens's fragmentary discourse, but to be drowning in the failure of his basic security system.

Giddens practical consciousness allows the rationalization of the dureé of day-to-day activity - the

knowledgeability it recalls is the same as the knowledge and resources of the four discourses suggested here. Such knowledge is manifested in a fragmentary way precisely because it is derived not from one discourse, but from four. The inclusion of symbols in discourse resolves Giddens's difficulty of the uncertain margin between practical and discursive consciousness; the way this thesis theorizes symbols allows their discursive use by agents in ways that they cannot put into words - symbols are shared, but meanings need not be.

The greatest value of this use of Giddens's conceptualization of the psychical apparatus (his 'stratification model') for this thesis, would be at the level of his 'basic security system'. However, this implies recall of the routines and schemata of the four discourses not just by practical consciousness, but also during the process of reflexive monitoring of action, and is a much more speculative proposition. However, if this is licensed, then Giddens ideas of the security that this system produces can be allied to those of Cohen, discussed in section 2.3, where: "people draw the conventions of community about them, like a cloak about their shoulders, to protect them from the elements" (Cohen 1985:63). It also allows the problem of the three-discourse person to be considered.

As already noted, Giddens's reflexive monitoring uses 'perceptual schemata' that are neurologically based, but he sees ontological security as grounded in routine, so schemata based in such routine - in the institution of a discourse -

are also implied. As Giddens observes: "there is a generalized motivational commitment to the integration of habitual practices across time and space" (Giddens 1984:64) - such integration is surely embodied in discourses of the sort posited here. People reflexively monitor the dureé of day-to-day action, and feel secure as long as it fits their routine discursive schemata - when it does not, they get anxious.

Considerable progress has been made in explaining how people can escape the anxiety that would result if they consciously perceived the conflicts and paradoxes between all the different discourses in which they are involved. They can use the resources of the discourses themselves to arm against such anxiety. The last chapter charted the stately progress of the 'longue dureé' of innovation through the four discourses. This chapter has arrived at the rapid recursive implication of those same discourses within the perceptual mechanisms of the people who innovate - a cyclic activity of their 'basic security mechanisms' within the dureé of their day-to-day lives.

However, one element in this continual reflexive monitoring process has not so far been considered, yet it is vital to the problems of this section. This element is the <u>frequency</u> of this continual cyclic activity. It is all very well to assert that different schemata allow different 'worlds' to be encountered without anxiety, but for this mechanism to 'work' the cycles must be sufficiently rapid to

preclude the untimely intrusion of another 'world' and the consequent reintroduction of paradox and anxiety.

At Giddens's level of neurological schemata (the eyes following a moving object say), a frequency of many cycles per second is implied. However at the level of day-to-day interactions between people, the level of their routine monitoring, Giddens draws upon Goffman's ideas of the 'bracketing' of social encounters (Giddens 1984:64-92). It is at this level that social learning and knowledgeability must be implicated, with practical consciousness recalling the appropriate routines from a discursive knowledge of how to behave. Here the monitoring cycle has two frequencies - that of bodily positioning and gestures that are so important in talk, and that of the beginning and ending of a conversation, or the turn-taking within it.

Durée can thus be 'bracketed' by a "reflexive moment of attention" when, as Giddens says:

> someone is asked by another to supply 'a reason' or 'reason' for, or otherwise to explicate, certain features of his or her activity (Giddens 1984:73)

The frequency of shifts of a person's 'attention' that this implies, potentially between disparate discourses, is hardly conducive to the ontological security of that person. It is, of course, possible to weave other mechanisms around encounters that might allow such security - in passing Giddens himself cites Durkheimian ritual separating sacred from profane as an example of bracketing (pp.73-74); a person's sense of the right clothes to wear (p.79) might be another example of discursive knowledge giving security, but examples

like these operate by excluding other discourses - they return to ideas like Cohen's community. This may well be what most people do most of the time, and Giddens brilliant project is certainly predicated upon just this aspect of social systems. But, this is not the aspect of social life that the data of this thesis highlight, and not the aspect that this section seeks to explicate.

The data of this research, even when revealing persistent bracketing of encounters in such as meetings, showed the active clashing of different discourses within the dureé of day-to-day innovative activity. Even at the level of the sentences and phrases of people's interviews all four discourses could be seen jostling for position, and yet they generally seemed unperturbed by this. The irruptions of 'real-life', even of mild anxiety, were certainly there - but they were exceptions to a much more prosaic - and secure - normality. Theorizing around, and bending, Giddens cyclic recall mechanisms provides a powerful explanation of how people survive day-to-day life, but these mechanisms are of entirely the wrong frequency to explain how innovators survive innovation. It is only at the frequency of Giddens's neurological schemata that a security mechanism might be found, and these are most certainly for him 'pre-discursive'. Indeed, even to push discourse to the level of his practical consciousness, and further into routine monitoring, committed a considerable act of violence to the fabric of his carefully embroidered ideas.

8.4.3 INTERMEZZO

The plight of the 'three-discourse person' remains - she may walk away from a meeting that denies her funds, go home and think about something different - but all the time an insistent cry continues within her moral discourse that "things don't have to be the way they are". The purposive actor wants to change things, but the way they are already imposes a moral counter to change - as Giddens says: "Tradition .... represents the moral command of 'what went before' over the continuity of day-to-day life", he adds:

> In this respect Lévi-Strauss is surely right that tradition is the medium of the reversible time linking the 'dureé' of daily life with that of the 'longue dureé' of institutions. (Giddens 1984:200)

The complete mismatch of the frequency of an innovator's activity with that of the innovative discourse itself is implicated in his problems - which again emphasizes the importance of the frequency of cyclical time in innovation.

The question remains: how does the innovator cope with all of the discourses that comprise innovation, at once? Lévi-Strauss's mechanism has already been mentioned (it is briefly considered in section 2.3) - a slated structure of changing versions of a myth that "progresses from the awareness of oppositions towards their resolution" (Lévi-Strauss 1963:224), but this has more power to explain the cyclic mechanisms of longue dureé than of dureé, let alone any unconscious cognitive security system.

However, Lévi-Strauss's resolute espousal of structuralist explanations, and the efforts of those who have followed in his footsteps, do hold out a hope of regaining the promised resolution that Giddens's formulation of a 'basic security system' seemed to offer. The insuperable problem that was reached in trying to amalgamate Giddens's ideas with the four-discourse model was that the most basic level at which his psychical mechanisms could access discourse was still too 'conscious' - its rate of recursion was too slow. A truly semiological structuralist approach does not limit discourse in this way, but neither, section 2.3 showed, does Sperber's cognitive approach to Lévi-Strauss's semiology.

In Sperber's careful development, the scent of a resolution to how innovators survive is very strong. Indeed, Sperber likens the way a smell can evoke memory to the way the symbolic mechanism works. His arguments will be reviewed again here because they clearly stand as a link between Giddens and Derrida, and so help elucidate the ideas of both.

Sperber's treatment of knowledge mirrors, in many ways, Giddens's practical and discursive consciousness (and resolves the difference between them perhaps). Knowledge is guaranteed by experience - seen as true, whereas the brain puts beliefs into 'quotes' as 'symbolic knowledge' - "the symbolic mechanism is the 'bricoleur' of the mind" (Sperber 1975:113). When an inexplicable occurrence, or a paradox, fails to fit into the mind's "conceptual categories of meaning", the symbolic mechanism focuses upon this failure, brackets it as symbolic, looks into the field of memory that this focus

delimits, and reconstructs that memory. In the language of this thesis - if something fails to fit into one discourse, the mechanism fits it into another. This cognitive symbolic mechanism is thus watching over perception, ready to grab and relocate anything inexplicable - anything, in Giddens's term, that threatens 'ontological security':

> the repetitive side of cultural symbolism is there to set the endless evocation periodically in motion again (Sperber 1975:145)

Sperber thus provides the intimate unconscious security mechanism that the four-discourse model needs. The problem with Sperber is that, in rejecting, like Giddens, the primacy of the structural linguistic heritage that Lévi-Strauss uses, he denies use of his ideas within the four-discourse framework. Nor is it quite so possible to re-introduce discourse to Sperber since, unlike Giddens, he rejects signification altogether - he sees his idea of 'evocation' replacing any notion of 'meaning'.

It would be possible to see discourse as Sperber's "repetitive side of cultural symbolism", but this would throw away all but the boundaries around the discourses. The shared views of the world that chapter six started by illustrating would have to be discarded for Sperber's much more localised view that "Cultural symbolism creates a community of interest but not of opinions" (p.137). This is a seemingly small point - this thesis (like Cohen, and even Derrida) has never claimed

that either meaning or 'opinion' need be shared in discourse[7] - but Sperber's crucial assertion here is that 'symbolism creates a community of interest'. This shows that he shares Lévi-Strauss's project to unearth the deep structures of the mind, but it also forces him into the teleological position that his universal cognitive mechanism actually creates social entities like community and, presumably, discourse. Thus discourse and cognition cannot be separated, or recursively linked, as this thesis, and Giddens, suggest they must be.

This is the point of Sperber's divergence from Derrida - Sperber implicitly sees language as a metaphor for the human use of symbols, and as such he rejects it as unnecessary. Derrida sees it, in his arche-writing, as very much more than a metaphor. Sperber slips towards solipsism, his world is left as a construct of the deep structures of the mind, which he himself admits, in an essay on Lévi-Strauss, are but "fleeting shapes and contours one can glimpse through the mist" (Sperber 1979:40). Derrida's world looks more like the 'real-life' of the interviews - its structural properties and discursive character are complicated, and may easily be traduced, but they are capable of explication, and they have been tested against the data that this research has presented.

It would be an act of folly to follow Sperber in rejecting the discourses that have proved so serviceable in discussion thusfar, but it would be unfortunate if his ideas had to be discarded outright. Discussion must therefore turn

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[7] As Derrida points out - not just the elements of the sign, but signification itself is arbitrary.

to Derrida, in order to see if his masterful explication of structuralism can retain the insights of Sperber within the context of the four-discourse model of innovation.

8.4.4 THE TIME OF WRITING

The problem that the would-be theorist of everyday life faces, stated baldly, is how to combine the recall of remembered discourse with the flow of dureé. Sperber provides a convincing mechanism, but rejects discourse; Giddens's mechanism seems hardly fast enough, but his whole discussion about the relations between discourse, memory, and structure has considerably aided the debate thusfar, and his refusal to see a 'present' in dureé points the way forward - the way that Derrida resolutely takes.

In fact, Derrida might well pick up the treatment of memory in order to shake Giddens's sociological discourse. Giddens and Sperber have conceptions of knowledge built up through past experience, and of belief recursively recalled and re-created, and both theorists thus invoke memory - Sperber says:

> conceptual representations are probably not extracted from passive memory, but rather reconstructed by means of the traces left by previous acts of construction. (Sperber 1975:141)

Giddens sees his practical consciousness as a recall mechanism, and refers to the need to distinguish:

> memory, as the temporal constitution of consciousness; and recall, as the means of recapitulating past experiences in such a way as to focus them upon the continuity of action. (Giddens 1984:49)

Giddens's familiar problem of structure versus process appears here - the past reified as 'knowledge' or 'memory' fitting ill within the flow, the dureé, of everyday life. The reversible time of dureé is a way of meeting this problem for Giddens, but it never quite goes away. It reappears even as Giddens expounds his carefully turned ideas of structure:

> Structure, as recursively organized sets of rules and resources, is out of time and space, save in its instantiations and co-ordination as memory traces, and is marked by 'an absence of the subject'. (Giddens 1984:25)[8]

Giddens, ever mindful of the need to resist any metaphysics of presence, still seems to require that memory remain doggedly present as an instance - presumably, moreover, present in the mind of a subject.

Thus, even a theorist like Giddens who dares to try and combine social structure with human cognition finds his Achilles's heel with memory - which has elements of both. Memory must contain the structural knowledge that Giddens's knowledgeable agent needs, but it is also part of the cognitive apparatus. Sperber gives up long before this point, and accepts only the cognitive mechanism, to which the social structure is merely subservient. Memory is thus the undecidable element in Giddens's text where Derrida might insert his deconstructive lever, but if this is to be allowed, then Derrida's own views of memory must now be considered.

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[8] Giddens is here summarizing his earlier discussion (Giddens 1984:17).

It is significant that both Sperber and Giddens refer not only to memory, but to memory traces - Derrida's idea of the disseminated trace is central to his theoretical development here. Derrida's complex ideas were discussed at some length in section 2.5 in order to provide the background that the present discussion needs to proceed. Background and foreground are not, of course, words of which Derrida would necessarily approve - all texts potentially implicate all others, and it is in the continual interplay of their rhythm and their weave, that Derrida catches what Giddens's would call dureé. Indeed, Derrida's own ideas demand repeated study, and the way in which they have already been woven into the fabric of this thesis begins to give some impression of what Derrida means by a trace. This section, therefore comes not to explicate Derrida's ideas, but to use them, even though it repeats some of section 2.5 in the process. It uses them to understand the people in the interviews; the converse also applies, the stories of the people in the interviews aid understanding Derrida.

Derrida explains his own project in a passage already quoted in section 2.5, but well worth repeating:

> The play of differences supposes, in effect, syntheses and referrals which forbid at any moment, or in any sense, that a simple element be present in and of itself, referring only to itself. Whether in the order of the spoken or written discourse, no element can function as a sign without referring to another element which itself is not simply present. This interweaving results in each 'element' - phoneme or grapheme - being constituted on the basis of the trace within it of the other elements of the chain or system . This interweaving, this textile, is the text produced only in the transformation of another text. Nothing, neither among the elements nor within the system, is anywhere ever simply present or

> absent. There are only, everywhere, differences and traces of traces. (Derrida (1987a:26)

The key to this passage (itself a very Derridan word - passage from where to where? What is passed as it passes?) is the phrase that each element is 'constituted on the basis of the trace within it'. This idea is reminiscent of Sperber's symbol evoking 'traces left by previous acts of construction', but it goes very much further. For Derrida, the trace is not simply within the mind, but within the element of discourse itself. Insofar as this evokes the memory of an individual, it also evokes very much more - it evokes all the discourses that the element implicates - the 'traces of traces'.

Of course, it could be pointed out here that these traces can be seen as lying in the 'knowledgeability' that Giddens's consciousness recalls, and thus introduction of Derrida is mere casuistry, or at best redundant. But, Giddens - whilst asserting the absence of subject, time, and space from any structure of knowledge - was still forced to locate memory within time and the mind of a subject, which does what he denies - fixes a centre for structure in time and space. Derrida does not do this, as the beginning of his essay on Lévi-Strauss (Derrida 1978:278-293) says: the Saussurean structuralist project began at "the moment when, in the absence of a center or origin, everything became discourse" (p.280).

Derrida views discourses in exactly the way that this thesis has sought to view them - as realms of local and relative truth. In discourse, Derrida sees the sign as

replacing the centre, the discourse moves as the trace of signification moves. This very clearly parallels Giddens's slow change of institutions in the reversible time of longue dureé - as an innovative discourse moves through the four discourses, its apparent meaning seems to change along the way - but, Derrida says, all the meanings are inscribed, outside of space and time, in its trace. This is why Derrida must be invoked to resolve Giddens's problems - because he rigorously theorizes the impossible 'outside' of discourse[9] in a way that Giddens does not:

> If there are structures, they are possible only on the basis of the fundamental structure which permits totality to open and overflow itself such that it takes on meaning (Derrida 1978:26)

Thus Derrida resolves Giddens's problem of a temporal memory-trace. Giddens asserts that "structures", or (as he prefers) "structural properties", are "out of time and space", nonetheless, he has to add that structure still "exists, as time-space presence" (Giddens 1984:17 - emphasis added), even though it exists only as "reproduced social practices", and "as memory traces orienting the conduct of knowledgeable human agents" (emphasis added). This is evidence of Giddens's duality of structure. Derrida's resolution provides an originary 'fundamental structure' that is out of time and space, outside the structure of Giddens's memory-trace, or the structures of discourse or knowledgeability. Yet this

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[9] "not simple by rejecting such ideas wholesale but by showing that they rest on a certain metaphysics, an ontology of language and being whose absolute reign must henceforth at least be open to doubt" (Norris 1987:53)

fundamental structure is not really outside of these local individual totalities, it is also inside of them - glimpsed when they 'open and overflow', and so 'take on meaning'. Giddens's dismissal of such a semiological "'retreat into code' - whence it is difficult or impossible to re-emerge into the world of activity and event" (Giddens 1984:32), denies him the 'spacing' of the fundamental structure where the paradoxical 'activities' of individuals and the 'events' of innovation can be understood.

This fundamental structure is Derrida's 'arche-writing', which shows through the fissures in the four discourses of this thesis. It is there that they are formed, there where their local truth peels away from the madness and excess that is also a part of 'real-life' - where:

> speech, confined to this temporal rhythm of crisis and reawakening, is able to open the space for discourse only by emprisoning madness (Derrida 1978:60-61)

Yet 'real-life' still sometimes irrupts in discourse, Derrida continues:

> crisis or oblivion perhaps is not an accident, but rather the destiny of speaking philosophy - the philosophy which lives only by emprisoning madness (Derrida 1978:61)

It irrupted in the data as was illustrated earlier, it also becomes peculiarly, and painfully, evident to the researcher - as Derrida says, "I philosophize .... in the <u>confessed</u> terror of going mad" (p.62). But investigation of the play of discourse, its excess, shows this fundamental structure, and allows the researcher to understand how the inventor innovates. Indeed, it allows the inventor to invent because

it is only in the double mark of spacing that new meanings can be forged, a "new speech" be made that can:

> at every instant liberate previous madness while enclosing within itself, in its present existence, the madman of the day. (Derrida 1978:61)

The arche-writing has to be writing (and not the 'speech', or 'speaking philosophy' of the logos that has traduced philosophy for millennia) because speech cannot be outside of time, whereas:

> in every silent or not wholly phonic spacing out of meaning, concatenations are possible which no longer obey the linearity of logical time, the time of consciousness or preconsciousness, the time of 'verbal representations' (Derrida 1978:217)

This quotation is taken from an essay of Derrida's on Freud (Derrida 1978:196-231), and it is here that Derrida comes closest to solving the problems of innovative action as they appear in the present research.

The basic problem that still remains in this investigation of innovation - the relation of ideas to action - is how the people who innovate can, with apparent ease, resolve the paradoxes between the four very different ways of speaking that innovation seems to require. Derrida's mention of a new speech 'at every instant' promises, not only that the clashes between all four discourses might be resolved, but also that that resolution is quick enough to explain the evidence of the innovators in this thesis (in Giddens's terms, to explain how they maintain their ontological security). Derrida notes that Freud had to resort to the metaphor of writing in describing the psyche, but, for Derrida, writing is not metaphor but the fundamental

structure, thus he asks "what must the psyche be if it can be represented by a text?" (Derrida 1978:199).

If psyche and fundamental structure are inextricably bound together in this way, then Derrida's trace of meanings, of 'dissemination' - permanent and outside of time and space - must be compatible with perception. Moreover, it is fundamental, so not only does it come before the time of discourse, it also comes before the time of consciousness, and of perception. The continual refreshing of consciousness from the dureé of day-to-day life must go by way of this writing - thus Derrida shows that the wax of the mystic pad, the unconscious, memory, are all written before perception - all have access to the originary space of writing. In his development of Freud's 'mystic writing pad' metaphors, Derrida observes at this point:

> Until now, it has been a question only of the space of writing .... But there is as well <u>a time of writing</u> (Derrida 1978:225)

He adds:

> Temporality as spacing will not only be the horizontal discontinuity of a chain of signs, but also will be writing as the interruption and restoration of contact between the various depths of psychical levels: the remarkably heterogeneous temporal fabric of psychical work itself. (Derrida 1978:225)

Freud adds, to the simple machine, a hand to peel the layers apart, to interrupt and restore their contact, thus "Traces .... are constituted by the double force of repetition and erasure" (p.226). This provides the mechanism that allows perception rapidly to oscillate between all of the four discourses, for meanings to change so quickly that the

multiple meanings that innovation seems to have in people's minds can coexist without apparently troubling them. It is only the researcher's insistence upon playfully throwing the deconstructive spanner of an undecidable meaning into the works that slows the process of thought, opens up the generative void where all the discourses can come together with the madness of their excess, and so shows up the paradoxes of innovation.

The problem of articulating individual action, innovation, and the social structures of discourse, is nearer to resolution here than at any point in this thesis. However, no simple resolution is possible; accepting the logic of arche-writing, of 'spacing' as originary, means:

> That spacing is the impossibility for an identity to be closed on itself .... The irreducibility of spacing is the irreducibility of the other. (Derrida 1987a:94)

In a way, Freud acknowledges this by needing an invisible hand to operate the mystic pad - in so doing the simple idea of perception itself 'existing', of the mystic pad providing a simple metaphor for it, is vitiated:

> pure perception does not exist: we are written only as we write, by the agency within us which always already keeps watch over perception, be it internal or external. The 'subject' of writing does not exist if we mean by that some sovereign solitude of the author. The subject of writing is a system of relations between strata: the Mystic Pad, the psyche, society, the world. Within that scene, on that stage, the punctual simplicity of the classical subject is not to be found. (Derrida 1978:226)

This, then, allows understanding of why it is necessary to consider not only the individual agent in innovation, but also the discourses that surround and comprise innovation, as

well as the human psyche itself. But this also allows understanding of why it is impossible simply to aggregate unitary views of individual, or structure, or psyche into a model where a simple subject could still be held responsible for innovation - as Derrida says: "We must be several in order to write, and even to 'perceive.'" (p.226). Difference, the basis of language and writing, needs an other to be 'different from', just as life needs death. The mystic pad is a dead machine, it needs Freud to operate it - the traces in its wax can only be memory traces as part of a system that comprises individuals and their discourses. This is very close to Giddens duality of structure and agency, but, for Giddens, structure - although outside of space and time - is still secondary to the dureé of action, to temporality. For Derrida, the fundamental structure of writing generates not only time and the possibility of discourse itself, but also the 'time of writing' that is a basis for individual perception and innovative action. The subject of innovation is a system of individuals and their discourses.

> Innovation is truly a trace of traces, a text where:
>
> What I call text is also that which 'practically' inscribes and overflows the limits of such a discourse. There is such a general text everywhere that .... this discourse and its order .... are overflowed, that is, everywhere that their authority is put back into the position of a mark in a chain (Derrida 1987a:59-60)

Each discourse has its own authority over sense and truth, but there is also an excess where other meanings are forged - "the realm of ethical discourse .... which exceeds all given

conceptual structures" (Norris 1987:224). An interval can open in discourse to allow people to think new thoughts - between the double marks of this interval, innovation can happen.

8.5 The Trace of a Thesis .... A Summary

> A metaphor is a vice that attracts the dull mind by reason of its aptness and repels the too serious mind by reason of its falsity and danger so that, after all, there is something to be said, nothing voluminous perhaps, but at least a word of concession for that class of society which in literature as in everything else goes always with its four feet on the ground. (Joyce 1991:92)

The last section found a way of integrating structure and process, of combining the structures of discourse with the actions of individuals, within the social world of innovation. This is not an easy task, as the organizational literature shows. Open systems and contingency ideas have pointed the way that this research has taken, with their emphasis on systemic diversity and the need for a focus outside of organizations. Multiple paradigm approaches have held out a promise of understanding through varied structural metaphors, but their promise has largely been unfulfilled because no attempt has been made to theorize how their various paradigms might be articulated. It is as insufficient as it is unsatisfying to offer vague endorsements of the value of being multiply sensitized - it is also what gets interpretive and qualitative research a bad name.

This research has been interpretive and it has been qualitative, it has drawn upon a long anthropological and sociological ethnographic tradition, but it has sought to show that such a methodological approach does not merely benefit from, it demands, a rigorous and rational orientation to the development of both analysis and theory. Rigorous and rational theory in this area is to be found, most usefully for the present concerns, in the challenging sociology of Giddens and philosophy of Derrida. Their ideas have been woven together with the body of data that the research has collected, and the result has been an enhanced understanding of data, method and theory.

If this thesis has resolved some of the problems that its data raise, this has not been achieved lightly - it has been achieved at the expense of a considerable complexity and depth of philosophical discussion. This has demanded that less heed be paid to disciplinary boundaries than is customary in a management thesis. To take theoretical resolution further would involve a deeper and unwarranted trespass into the domain of philosophy; all that remains to be done here is to summarize the story so far.

The model developed over the last three chapters has been shown to be capable of empirical illustration and of heuristic use. Structures have emerged that have been called discourses - seen synchronically in Saussure's terms - 'langue'. A process of innovation was first posited in which an idea, in order to grow, progressively jumps from discourse to discourse

- diachronically - 'parole'. These discourses have been broadly conceived at a social level, but they have grown out of interviews with individuals, and the case-studies show that it is through the agency of individuals that the changes of gear between the four emergent discourses are brought about.

The cracks in this version of the model began to show (even though it retains some utility as a tool for understanding the broad character of innovation). The idea moves not by some stately progress through first one discourse, then another - it exists in all of them all of the time. The model simply shows that, at any one point, an idea merely presents a few more paradoxes to one discourse than to the others, and if it needs support in the language of that discourse, then all of its supporters have to try to speak that language - <u>but at the same time they go on talking all the other languages</u>. The idea creates, in effect, its own discourse, composed out of the other four; as it grows, they are all renewed. Giddens's carefully crafted duality of social structures and individual agency within social systems provides a theoretical underpinning for such a model.

Thus, for the people who support the idea, the people with their feet in different discourses, it is not a case of simply shifting their rhetoric as the project changes - it is not even a case of getting up in the morning and deciding which discourse to adopt for the day - it is a case of talking in all of them at once. Explaining how they can do this without being crippled by anxiety at the paradoxes involved, became the last pressing challenge in formulating an adequate

model of the innovative process. Giddens provides a start here, his security mechanisms against the rigours of day-to-day life are convincing, but the mechanisms that might reassure the would-be innovator seem to operate too slowly, and at too conscious a level.

The relevance of thought in the Saussurean semiological tradition, and particularly French Structuralism from Lévi-Strauss (and his non-semiological follower Sperber) to Lacan and Derrida, becomes apparent here. That a few people have begun to see the value of such ideas for investigating management and organizations - such as Cooper (1989), or Vargish (1991), gives some encouragement in attempting to use them.

Derrida's ideas seem particularly relevant, especially because of their adamantine rigour, not to say hardness. A Derridan approach seeks to tease out the loose ends of the fabric of social life - to unravel the textile of discourse in which people cloak themselves - to deconstruct the text that their speech writes in the pages of research. Deconstruction serves as a way out of the problems of the innovator caught betwixt four discourses. It is a philosophy of investigation rather than a method - and a rigorous and reasonable one. The core of these ideas is that ideas cannot have a core, and it is precisely because people believe in ideas having a core (that they have a 'presence', a 'being') that ideas get stuck in discourses.

The position of a philosopher like Derrida is thus precisely the same as that of the supporter of new ideas.

Faced with having to live in a ruling discourse that does not even realize its own limitations, both have to work from within to try and make those limits discernible. To do this, the symbols, the double meanings, the paradoxes, that show the end of one discourse and the start of another, offer purchase to the lever of deconstruction - discourses deconstruct themselves. But far from showing a border to be crossed, or privileging a new way of doing things, such switch-points mark out a space or interval in which the whole trace can be seen of what an idea has been, and what it is becoming, and of all the meanings it has had, or could have - in short, the void where innovation happens.

This excess of process beyond the limits of the structure of discourse is what the original model, and other models, lack. Challenging and difficult to understand, it is, nonetheless, a far surer basis of thought in which to investigate new ideas. It also fits the data of this research - people clearly saw occasional glimpses of this excess within their own discourses. Derrida opens out the spacing within the discourses of many other thinkers, including Lévi-Strauss and Freud, and it his extended discussion of Freud's metaphor of perception that finally offers a way of understanding how people can think in four discourses at once. A virgin surface for conscious perception (a problematical idea, dangerously close to metaphysics says Derrida) is continually refreshed, written upon, by the trace a stimulus has already broached in the unconscious - in memory. Different intersections of the trace of innovation with the traces of the different

discourses can thus be perceived in rapid succession. The chameleonic changes in the thought of innovators that their words betray can be understood.

The price that such understanding demands lies in the undecidable questions: whose trace? whose innovation? even, whose thought? Human action, agency even, is not denied, but causality is a concept of discourse and Derrida shows that the excess that supplements every action makes agency forever undecidable. Derrida allows a glimpse of the nowhere where the chain of innovation is forged - a hint, perhaps, to the innovator of how to be a part of the system of relations between people and their discourses that results in innovation.

This chapter began with a quotation from T.S. Eliot, a wistful lament that ideas and action result in knowledge of motion and speech, not of stillness and silence. Eliot spoke true perhaps, but his lament was misplaced - silence and stillness cling to the knowledge of dead structures - the thought of the twentieth century has provided a knowledge of motion and speech that allows such structures to be peopled, and it should be embraced and applauded. In Eliot's day the powerful and fragmented visions of such as Picasso and Joyce evidenced this thought. A new modernism arose - a reaction against the old modernism of Saint-Simon, and the ideas of progress through industrialism, and the excesses wrought in

their name like the social aberrations of 'scientific management'.

Successors of Saussure, Durkheim and Freud, like Giddens and Derrida, have - through their knowledge of speech, and of motion, and of becoming - provided this research with at least some understanding of the continuing process of technical innovation. This understanding has shown that the problems that beset the present-day successors of Parsons and Fisher are remarkably similar to the problems from which they too suffered. Across a century of change, the management of technology within industrial and financial discourse seems little altered; it is still characterized by stillness and silence.

This thesis has shown, no doubt in a lame and halting fashion, that new theory is available - new theory of tremendous relevance and promise for studying the continuing problems of managing innovation. Its wider application is long overdue.

CHAPTER 9 : SOME POSSIBLE CONCLUSIONS

That was a way of putting it - not very satisfactory:
A periphrastic study in a worn-out poetical fashion,
Leaving one still with the intolerable wrestle
With words and meanings. The poetry does not matter.
It was not (to start again) what one had expected.
What was the value of the thing long looked forward to,
Long hoped for calm, the autumnal serenity
And the wisdom of age? ....
(T.S. Eliot 1974:198-9 - East Coker)

9.1 Introduction

Throughout this thesis, theory, data and method have exercised debate by turns; each one has informed understanding of the other two. This might be the first conclusion of this research - theory, data, and method themselves have something of the character of the discourses that emerged in this research. The 'conventional' choice made in this thesis, of consigning them to separate chapters, has emphasized this - but allowing them to intertwine has also emphasized that, just like the discourses of innovative action, they cannot be kept apart. To confine research narrowly to methodological, or to theoretical concerns is as absurd as pretending innovation is solely a moral, or an industrial, or financial enterprise. The 'real-life' of research and innovation alike includes all the discourses within which their practitioners speak, write and act.

This short chapter maintains the separation of the three discourses of research. The canons of thesis writing are well tried and convenient - moreover, it is a pleasing paradox that scholarly research, whose business it is to transgress the boundaries of what a discourse already knows, should be

constrained by scholarly convention in this way. It is a paradox, perhaps, that stands as a token for the process of innovation itself.

Three conclusions each are offered upon method, data and theory. More detailed conclusions could no doubt be drawn from the previous eight chapters, but to do so would merely repeat much of their material. The important point is not that these conclusions are fixed or final, but that they demonstrate how a fusion of theory, data and method can allow the understandings of each other that outcrop, within their discourses, as conclusions. The real conclusion is that conclusion is as much a process as a structure.

9.2 The Method

The first observation upon the method of this thesis has already been fully developed in chapter three - investigation of innovation seemed to require a qualitative and interpretive method, and the choice of such a method has been vindicated by the present research. The reasons are simple - innovative activity deals in things that cannot be fully predicted beforehand, thus where to turn the focus of research needs continually to be investigated during the course of research - this demands interpretation of as broad a spectrum of innovative social artefacts as possible. Above all it demands talking to the people who innovate, or reading their letters, not counting things.

This leads to the second important point. These people are often not neatly arrayed in organizations, the research very soon found that they spanned many organizations and groups. The innovations considered here as often proceeded in spite of organizations, as they did because of organizations. Research needed to be sensitive to all sorts of social collectivities, but it also needed to consider the individuals who were involved, and even their thought processes as well. The idea of a unitary level-of-analysis was not one that this research could afford the luxury of embracing.

The final methodological observation is possibly the most important of all, but it will be briefly made. If this thesis, and particularly its appendices, have not already demonstrated this point, then no amount of discussion can avail. The point is that, because qualitative data is inherently unstructured[1], and because any satisfying interpretation emerges and cannot be imposed, analysis of this data demands more order and rigour than quantitative data, not less. The interpretive researcher can never be an outside unbiased observer; he or she lives, filters and records each and every interview. Herein lies a true rigour - little in this research encounter is discarded, the data is rich, but to preserve its promise demands that every bit of it be sifted and considered. The analysis method developed and used here

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[1] Beyond the fact that it was, here, conversational English, neatly divided up into the transcripts of separate interviews, or into letters.

met this requirement. The appendix to this chapter develops some of these points in a little more detail.

9.3 The Data

This thesis, and the theoretical works it has used and cited, have all spent far too long emphasizing the problems that models and metaphors have when they try to reify 'data', for any neat encapsulation of data to be offered here. All that can be offered as conclusions are one important observation about where useful data were found, followed by two more about the model of innovation that was developed. This model is, perhaps, the real conclusion of this research.

Where to look for data is a pressing problem in management research, and this prompts, perhaps, the most far-reaching and useful conclusion of this thesis. This is that sensitive use of 'historical' material can yield data that are directly comparable with free-format research interviews. A personal letter from the past is just as much a primary source as the transcript of an interview. Of course, letters demand a working knowledge of the worlds of their writers, accessible through critical use of secondary sources, but this is precisely the same as the 'cultural competence' that interviews require of the researcher. The use of such material opens up a vast and largely untapped resource of case-study material for interpretive analysis and management scholarship - a resource whose study, moreover, is more easily

and cheaply accomplished than comparable study of management in today's organizations. Interpretive business history is a discipline waiting to happen.

The second point about data relates to the model that has been developed in this thesis. Difference is central to this model - in many ways innovation was theorized as a process of creating difference within its four different discourses. The structural linguistic ideas upon which the model drew are themselves based upon difference - what is more, upon arbitrary difference. It is hardly surprising, then, that the data from which the model emerged was itself characterized by difference - inventors versus financiers, UK versus US, past versus present. But the model has shown that these elements that look irreconcilable at first, have a deeper similarity, and an underlying consonance. Moreover, concentrating upon these similarities threw the differences into a strong symbolic contrast that often greatly aided understanding of the innovative process. The conclusion is that diverse and dissimilar data is not something to be avoided in management research, but something to be sought.

The final observation about data concerns the way that the model of this thesis combines structure and process. The two terms are problematical throughout the social sciences, and this research has demonstrated that they must be considered together. Most important, they can never be divorced from the actions of people - it is through human

activity that process continually recreates, and is modified by, structure. Innovation itself emerged from the data as a compound of structure and process, of action and discourse. The model of the last three chapters went through several iterations, and no doubt needs many more, but even though the balance within it of structure, process, people, and social collectivities may have varied as it progressed - it always needed all of these. Like Derrida's idea of a sign containing the trace of its construction within it, an adequate model must <u>be</u> the data that it explains.

9.4 The Theory

The first important theoretical conclusion has just been made, but it deserves re-emphasizing. The theory to which this thesis led was theory that refused to privilege either structure or process, but sought to incorporate both. Giddens theory of structuration, with its duality of structure and agency, has provided a peculiarly comprehensive framework upon which to hang innovation. However, even this framework is too restrictive. Derrida's ideas have had to be added in order to allow innovation the freeplay it needs. Derrida does not so much combine structure and process, as allow an interval to be glimpsed where the difference between them has no meaning - the void where meanings are created. Derrida defies summary as much as he denies conclusion, a word he would probably delight in using to deconstruct any text that employed it. The way in which the text of this thesis deconstructs itself

through words like 'conclusion', will therefore have to stand as a concluding example of the power of Derrida's philosophical ideas here.

Comment on theoretical matters would not be complete without a clear acknowledgement of the debt the theory used here pays to the semiological structural linguistic 'tradition' started by Saussure. This broad tradition has been used here to tease the linguistic and symbolic artefacts out of people's words, and to interpret the structures and processes that they suggested. If this thesis has aided understanding of innovation by doing this, then it points to the relevance of linguistic ideas (whether developed by French structuralists or radical sociologists) to analysis of the problems of organization and management.

The final theoretical word has to rest with the people who innovate. Understanding innovation is understanding them. This thesis has used the words of people who were generous enough to allow the researcher access to them. To interpret their words it turned, in the end, to Derrida - it was only his complex philosophical discourse that allowed an understanding of how <u>they</u> can come to terms with, indeed not even notice, the multiple paradoxes of <u>their</u> complex discourses. In the end the researcher can only describe innovation; he is left to marvel that some of the people to whom he talked, aided or impeded by their discourses, actually innovated.

No thesis can ever be complete, no research can ever be concluded, but, like human life, it has to end. The space of writing of this thesis has now closed, but the trace of this research remains within it. Only one fragment of the discourse in that trace is the researcher's, fragments of the discourses of others are also there in profusion. In this thesis there is an echo of all their words and all their ideas; it is only right that it should end with words dedicated to the memory of the one who was foremost amongst them - they stand as a motto for all scholars who are 'Valiant-for-Truth':

> I do not repent me of all the trouble I have been at to arrive where I am. My sword I give to him that shall succeed me in my pilgrimage, and my courage and skill to him that can get it. My marks and scars I carry with me

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APPENDIX I - INTRODUCING THE RESEARCHER

> I could chronicle the emotional strain of the researcher as he attempts to live with all his beliefs in phenomenological 'brackets'. I could discuss the loneliness one feels after spending months with an ethnographic record. In all these ways and many others, faith that ultimately the research will be of interest is a necessary support for the interpretive researcher. (Wenger 1985:319)

Human beings go about their day-to-day lives within all sorts of different social contexts. This raises a problem for social theorists - should they look at an individual or at some social collectivity?. Much of this thesis is intimately involved with answering this question, the arguments will not be repeated here, suffice it to say that, very broadly speaking, it tries to do both. The research tries to look at both the individual inventor or financier or whoever, and also at the organizations, groups, communities, associations, and suchlike, that seem to be important to them.

This raises another problem - whatever it is that links individual to collectivity has to be theorized as well. To attack this problem, the researcher has to enter the participants' worlds and share their visions of their social situations. This involves being trusted by them, being 'like them' - not in an attempt to dissemble or imitate, but rather in the spirit of not pre-supposing what is there.

This is the basis of the interpretive ethnographic method used in this thesis. The researcher, to use a rather clichéd phrase, is the research instrument. This being the case, it is only proper that the research instrument be described. This appendix sets out to do so; it then concludes with a few remarks on the other appendices of this thesis and on what is included in them.

Wenger (1985:307) speaks of the interpretive researcher having "a moral responsibility" to provide guidance to his successors in the painful art of accomplishing an ethnographic

record. His guidance was valued in the research described here - it is therefore hoped that chapter three on methods, along with this more intimate appendix, will take up the torch from him - however lame the attempt in comparison with his fleetness.

The researcher, like the individuals interviewed, has a voice - his voice - for I am a he (the masculine form is therefore used for the researcher where appropriate in this thesis) - his voice certainly runs throughout this thesis, but scarcely in a dominant or particularly animated way. The people interviewed had voices more noticeable in this text, but their words were delivered to the researcher's face in full knowledge of his credentials, such as they are. When I contemplated a study of HOTOL, therefore, I felt I would be aided by having been myself a 'member' of the aerospace industry. I had in fact worked, as a chartered engineer, for both Rolls-Royce and British Aerospace. Indeed, my seven years with Rolls-Royce, immediately after graduating with an engineering degree, had started with that most intense of socializing experiences - a graduate trainee-ship. We learnt Rolls-Royce, and more significantly our particular part of it (the erstwhile Bristol Siddeley), with an intensity of which any ethnographer would be proud. My next job was two years in a small company (somewhat akin to those described in some of the finance interviews), where size and urgency rendered it necessary to break some of the taboos that no engineer dare transgress in a large company. Five years in British Aerospace followed - encountering the same taboos and rich rituals, but emphasized in subtly different ways, and even some of the same people. When I talked to those involved in HOTOL, their moral careers bore striking resemblance to my own.

The aerospace industry has its own symbolic language - I could understand that a thing like HOTOL is real to an engineer when it is inscribed in engineering drawings and calculations. The final realization in metal merely confirms

its presence. A real metal aeroplane is, nonetheless, a potent symbol of another kind - it is an end-point that unites all those in the industry. The close association with such creatures that I enjoyed was unusual for an engineer, but it provided another useful credential for an aspiring student of the industry. My final credential that seemed useful, not in the HOTOL study but in gaining access to the world of the City, was gained after leaving the world of aerospace - membership of an old Oxford College.

Such credentials undoubtedly have value in interpretive research, but they have to be used with discretion - their value is as a cloak of invisibility that confers transparency upon the researcher. But if such credentials are a cloak, perhaps the key to success as a researcher is not so much ownership of such a garment through a chance of his past, but rather knowledge that such magical garments exist, and that they can, if needed, be borrowed from the very people he interviews without their realizing it. As my fellow engineer, aviator, and Oxford student - Mike Wenger - observes, echoing Goffman: "Qualitative methods demand a constant 'presentation of self'" (Wenger 1985:310).

The researcher has been summoned to appear as a 'presence' in this introductory appendix in the hope that, since he is implicated so inextricably in his text, his 'absence' from the rest of it will at least be noted by his readers, and the very notion of presence and absence thereby brought into question. This thesis is about how the voices of innovators are heard above the babble that surrounds them - it is thus appropriate to approach their stories listening, in this way, for other unexpected and feint voices in their texts.

If the researcher is 'absent' from the research text, then so is much else. Some of this material is traditionally included in appendices, and this expedient is adopted in this thesis. Appendices have been added where it is considered

that the inclusion of additional material might aid the reader's understanding of the main body of the thesis. Such material consists in supplementary discussion, supplementary analysis, supplementary data, or data analysed in a supplementary way. This material is not arranged as the usual arbitrary series of appendices, but by chapter in order to give some pointer to each bit's area of relevance. There is an appendix to each chapter, and whilst each is complete in itself, it is conveniently considered alongside the chapter to which it refers.

APPENDIX II - THE PROVIDERS OF THEORY

The method used in this thesis was an interpretive one, as chapter three describes; the data presented in chapters four and five is qualitative and ethnographic. The theoretical literatures considered in chapter two are therefore themselves of a qualitative and interpretive persuasion. This appendix to chapter two acknowledges this imbalance of theoretical consideration, without apologizing for it - it is both inevitable and necessary. However, ideas and data from other disciplines are relevant throughout this thesis, and whilst they are not important enough to its arguments to warrant inclusion within the main text, they do deserve a limited explanatory discussion.

The rest of this appendix provides such discussion - it briefly considers two areas of research. The first is the way that economists have treated the question of innovation, the second is the growing literature that considers venture capital. Innovation is the subject of this thesis, and, rightly or wrongly, venture capital has become associated with innovation in both the popular and scholarly minds. Venture capital was investigated in some depth in the interviews of this thesis.

In describing both the economics and venture capital literatures no attempt at comprehensiveness has been made; however, the economic texts found useful for this thesis have been those that review the innovation literature, and these give at least a flavour of its size and breadth. Several disciplines are clearly interested in venture capital - this thesis comes from a management tradition, but a geographical slant is considered below - an interesting, and conflicting, alternative to the economics of innovation. The discussion here is also relevant to the consideration of venture capital in chapter five, particularly its reference to a few sources of venture capital data and statistics.

II.I ECONOMIC ASPECTS OF INNOVATION

The ways in which economists have treated innovation are described in two recent review articles - those of Dosi (1988) and of Cohen & Levin (1989). Both note the tradition of (and are, perhaps, responses to the inadequacies of) industrial economics. This thesis looks at innovation from the activities of individuals and their inventions onwards, but industrial economics focuses on the later stages in the innovation process (if process and stages there be). Nonetheless, a microeconomic industrial frame of reference still has very clear implications for strategic and policy considerations, and even for macroeconomic management. It also has a very clear and challenging interface with welfare economic questions, and with the management literature. It is a broad frame of reference, and allows a critical perspective upon management allied, in different ways, to the research of this thesis.

To start with, two points of continuing debate might be extracted from the economic literature - they are useful reference directions in charting a course through it. First is the question whether innovating firms respond to what is possible, or to what people in a market want - the 'technology push' versus 'market pull' argument. The present research indicates that perhaps a richer fabric encompassing both is needed - the inventors in the case studies were certainly pushing their technology, but its uncertain adoption by industrial organizations obviously also reflected those firms' perceptions of market conditions (not to mention very active lobbying from all and sundry).

The second point is the debate about how patterns of innovation develop - how differences between industries, and across geographical areas arise. In the broadest terms this section considers differences between industries, the next section between regions - in both, but especially in the

second, the debate centres around the hoary old questions of the adequacy and relevance of evolutionary and diffusionary ideas. Once again, the present research, admittedly because of its initial focus on specific cases, undercuts these concerns by teasing out the relationships between the people who innovate. In so doing it shows how specific phenomena that become labelled as evolution and diffusion occur, without the need to make the claims as to causality that these concepts do - claims that lie at the heart of this, perhaps artificial, debate.

No introductory consideration of economic ideas on innovation, such as that attempted here, would be complete without some discussion of Schumpeter. Cohen & Levin (1989) provide an excellent way to do this - they review empirical investigations of the:

> two hypotheses associated with Schumpeter:
> (1) innovation increases more than proportionately with firm size and (2) innovation increases with market concentration. (Cohen & Levin 1989:1060)

They chart a very wary course through the vast literature on this, searching for 'robust facts'. This section will attempt to catch at least a flavour of this debate, but their conclusions on Schumpeter are worth noting first.

They describe the daunting obstacles to measurement in this area, and then move on to consider the empirical studies directed at each hypothesis in turn. They note that, in the first, Schumpeter's dyadic distinction between large and small has been extended into a continuous spectrum by many writers, concluding that:

> The most notable feature of this considerable body of empirical research is its inconclusiveness. (Cohen & Levin 1989:1069)

With the second hypothesis, likewise, they find:

> results leave little support for the view that industrial concentration is an independent, significant, and important determinant of innovative behavior and performance. (Cohen & Levin 1989:1078)

Their overall conclusion is that: "The empirical results concerning how firm size and market structure relate to innovation are perhaps most accurately describes as fragile" (p.1078). They therefore move on to a consideration of many of the factors that the investors interviewed in the present research mention, which they summarize under the headings of: "product market demand, technological opportunities, and appropriability conditions" (p.1079).

Dosi provides an extensive and eclectic review of these characteristics of the innovative process, and in doing so is perhaps closer than Cohen & Levin to management concerns. Dosi's rather labyrinthine article will, therefore, be taken as the basis for discussion here, augmented by Cohen & Levin's commentary. The approach taken will be eclectic rather than comprehensive. Dosi starts with a definition of innovation:

> In the most general terms, private profit-seeking agents will plausibly allocate resources to the exploration and development of new products and new techniques of production if they know, or believe in, the existence of some sort of yet unexploited scientific and technical opportunities; if they expect that there will be a market for their new products and processes; and, finally, if they expect some economic benefit, net of the incurred costs, deriving from the innovations. (Dosi 1989:1120)

Dosi's model of corporate man is clear from this, as are some of his areas of concern, like search processes, decision making, and profit seeking - also the useful distinction of process from product innovation. A certain divergence from the real people of interpretive research is also to be discerned (Cooper 1983 is relevant here). This divergence is the most abstract at the most general level, where innovation is simply linked to research and development spending. But even this link, as Dosi makes clear is a very problematic one (he notes the OECD 'Frascati' attempt at least to standardize the definition - Dosi 1988:1123). Cohen & Levin also consider this point, observing:

> it is heroic to assume that even a properly measured representation of R&D stock or flow can fully summarize a firm's effort devoted to technological innovation. (Cohen & Levin 1989:1065)

Dosi stems the divergence in levels-of-analysis between firms and individuals, by reintroducing the salient questions: "what do people actually do?", and what determines the "'propensity to innovate'"? (p.1125).

Dosi's answer to these questions starts by looking at the 'search process'. Innovation involves 'ill structured' problems that need solutions, and solutions need knowledge. Knowledge is a wide term, and technology, in turn, is much more than knowledge (or mere information[1]) - it encompasses both the universal public knowledge of science and the often undefinable and tacit competences of individuals and groups - including their 'models' and procedures. Dosi introduces the idea of 'technological paradigms' here (p.1127), asserting that: "each technological paradigm involves a specific 'technology of technical change'" (p.1128). Dosi's assertion is a useful one for this thesis, especially his summing up, although he appears to be insensitive to its implication that his own ideas, and those of the economic community, also comprise just one paradigm amongst many; he sums up:

> It quite often happens that prototypical problem-solving models, rules on how to search and on what targets to focus, and beliefs as to 'what the market wants' become the shared view of the engineering community. A paradigm is economically exploited over time and reproduced over time also through the development of institutions that train the would-be practitioners in methods for the improvement of basic exemplars, and peer's judgements are also based on the success achieved in the refinement and the use of these methods (Dosi 1988:1128)

The seminal legacy of Kuhn (1970) is very clearly apparent here.

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[1] Dosi's comments here are interesting - particularly his distinctive conception of 'technology' and its relation to knowledge and information - Dosi (1988:1130). He further develops these ideas later (p.1146) and critiques analyses that tend to see knowledge in commodity terms.

Dosi goes on to introduce another useful concept - the 'technological trajectory' (Dosi 1988:1128-1130, also described by Cohen & Levin 1989:1086-1088). Search is not random, covering the whole 'characteristic space', dipping into a common 'pool' of knowledge - it depends upon what has gone before, and upon what a firm already knows[2] - an 'avenue' of innovation is followed. Dosi adds later that such movement along a trajectory is irreversible (Dosi 1988:1144).

However, a point more relevant to the sorts of innovation that this thesis considers, is that some innovations create a new trajectory, a new paradigm. Here, Dosi notes:

> a continuous tension between efforts to improve the capabilities of doing existing things, monitor existing contracts, allocate given resources, on the one hand, and the development of capabilities for doing new things or old things in new ways. This tension is complicated by the intrinsically uncertain nature of innovative activities (Dosi 1988:1133)

Dosi thus distinguishes two types of uncertainty - the 'familiar' one: "defined in terms of imperfect information about the occurrence of a known list of events", and "strong uncertainty whereby the list of possible events is unknown" (p.1134). Dosi (1988:1141-1142) goes on to assert that this development of his paradigm approach resolves the push-pull argument. 'Normal' progress within a paradigm , and constrained to a trajectory, allows little room for market demand to exert a pull; on the other hand, 'market pull' does direct the 'extraordinary' progress involved in looking for new paradigms. Nonetheless, an equally good case might be noted here, supporting an argument exactly opposite to Dosi's - a market cannot pull an innovation that it does not yet know exists. Cohen & Levin catch the nature of this when they write:

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[2] Dosi continues his discussion of the differences between technology and knowledge in this respect, and, in a fascinating footnote, notes research that has drawn out analogies with linguistics here (Dosi 1988:1131).

> A vastly more difficult problem is posed by major innovations that introduce an entirely new product (e.g. the television, the automobile). In such cases, there is no straightforward way to characterize latent demand from data on existing products, particularly if one acknowledges that tastes themselves may change as a consequence of a major innovation.
> (Cohen & Levin 1989:1082)

They later note market structures completely overturned in such cases (p.1088). Dosi himself goes on to acknowledge a counter argument that sees technology as proactive and not reactive to the market - he writes:

> Whether market signals change or not, firms try to perfect their products and processes, by trial-and-error mechanisms of search and imitations of the results already achieved by other firms, motivated by the competitive edge that innovations are expected to offer.
> (Dosi 1988:1142)

This is a point, Cohen & Levin (1989:1078) suggest, that is endorsed by recent research evidence.

The problems that seem to beset the older traditions of economic debate here clearly derive from the elision of two types of innovation; both writers attempts to separate what Dosi terms 'normal' from 'extraordinary' progress testify to this. Yet the problems seem to remain - the research of this thesis suggests that greater insights are obtained once the individual level-of-analysis is incorporated - people, not firms, innovate, and to ascribe motivation to an aggregate of individuals, as Dosi does, is to deny the richness of wide areas of contemporary social science research. Nonetheless, the impressive catalogue of the characteristics of innovation at firm-level and above, that both writers provide, does provide a useful check upon the problems of too behavioural an approach, by reminding that such approaches should not theorize organizations away.

Both writers consider the mutual relationship between technology and science here. Cohen & Levin also introduce the idea of spillovers from other areas, something that Dosi catches in his more general debate about sectoral differences

and taxonomies of innovation throughout his article. Cohen & Levin say:

> Just as a close link to science and the availability of engineering heuristics affect an industry's technological opportunity, so does the contribution of technical knowledge from sources external to the industry: suppliers, customers, universities, technical societies, government, and independent inventors.
> (Cohen & Levin 1989:1088)

This thesis very clearly describes just such links as these, so much so that the assertion that they merely 'contribute' seems a little understated.

Both Cohen & Levin and Dosi move on to consider the central importance of appropriability, an area of relevance from classical economics through to modern ideas of business strategy. Basically, it is asserted that a new idea, however good, however productive of public welfare, will not be taken up unless a barrier exists to prevent other agents doing the same. Cohen & Levin cite the 1623 Statute of Monopolies and the American Constitution as embodying recognition of this (Cohen & Levin 1989:1090-1091). The patent is the theoretical solution to this dilemma, but many other mechanisms exist such as distinctive sales and service capabilities, a head start, an ability to learn quickly, secrecy, or difficulty of imitation (p.1093). Patents play a part in the data of this thesis, and economic research provides a useful caution here in the sometimes counter-intuitive character of its evidence. Cohen & Levin observe:

> In fact, industries differ widely in the extent to which patents are effective. The evidence suggests that patents are regarded as a necessary incentive for innovation only in a few industries.
> (Cohen & Levin 1989:1091)

They cite pharmaceuticals as one of these few, noting, on the other hand, that the effectiveness of patents in electronics is negligible. Dosi (1988:1139) notes much the same. Cohen & Levin (1989:1093) make an allied observation, of some relevance to technology transfer, that imitation itself demands very considerable investments in R&D (generally more,

and very often much more, than half the cost of the innovation imitated).

> Dosi sums up his argument at this point, saying:
>
> My general point, however, is that the observed sectoral patterns of technical change are the result of the interplay between various sorts of market inducements, on the one hand, and opportunity and appropriability, on the other. (Dosi 1988:1141)

He develops the theme of sectoral differences, noting its interface with regional studies (and thus the geographical approaches mentioned in the next section); he further discusses taxonomies of innovation and type of industry.

Dosi moves on from these lofty heights to consider the intra-industry differences that such aggregate approaches tend to gloss over - differences between firms themselves. He first critiques the Schumpeterean approach considered at the start of this section, and goes on to discuss differences within industries. Two final, and somewhat contradictory, points arise here that are useful in the argument of this thesis. These are Dosi's clear evolutionary tendencies, and yet his contradictory endorsement of less positivistic, more qualitative, approaches as well.

Dosi's (1988:1156-1157) discussion is measured, but he uses an evolutionary lexicon to describe his "loose biological analogy", where ideas of "'fitness'", of "technological variety" and "behavioural variety", explain the differences in innovation between firms through "a competitive process which selected survivors". Dosi describes various other economists' attempts to "formalize this process in an evolutionary perspective". He elaborates on such a process:

> evidence on the dynamics of industries and technologies highlights complex and varied learning processes whereby firms explore specific domains of perceived technological opportunity (Dosi 1988:1158)
>
> Note also in this dynamics, technological asymmetries and technological and behavioural variety <u>are both the outcome and a driving force</u> of technological and organizational change. (p.1158)

Thus, such a perspective suggests that market structure follows technology, and to account for observed differences: "one should 'map' the varied characteristics of innovation .... into empirically recognizable classes of evolutionary processes" (p.1158).

In contradistinction to Dosi's clear receptiveness to an evolutionary perspective is his acknowledgement, after further discussion of the ideas of industrial economics, of the problems of positivistic empiricism:

> Certainly, from an empirical standpoint, the concepts emphasized in this work (such as opportunity and appropriability) do not have obvious and objective counterparts, because they are not directly measurable and empirical studies are still difficult and uncertain; however, in my view, statistical difficulties do not detract from their crucial interpretive importance. (Dosi 1988:1161-1163)

Thus, Dosi concludes, there is scope for a "very promising link" between: "the domains of the 'economics of innovation' - on the one hand, and the .... domain of industrial economics - on the other" (p.1162). Dosi, therefore acknowledges the need to step beyond a strictly biological evolutionary model (p.1163).

The prescriptive conclusions of both articles are interesting, not to say heartening. Dosi notes the great range and number of economic studies of innovation over the last twenty years, but also says that:

> Progress in this area is often constrained by scarcity of the relevant data, but possibly also by the 'vision' and approach to empirical analysis of economists who are generally trained to consider technology among the preanalytical data of their models. (Dosi 1988:1163-1164)

He sees a "new view" trying to remedy this. He adds:

> My impression is that there is a significant gap between the wealth of findings by economic historians, students of technology, applied industrial economists, on the one hand, and the (more limited) conceptualization of these findings in economic theory, on the other. Clearly there will always be a difference between 'empirical stories'

> and the 'analytical stories' of the theoreticians. (Dosi 1988:1164)

The challenge, however, is to make evidence and theory compatible. Cohen & Levin (1989:1096&1097) also make this point, although their approach seems even more responsive to the loose and changeable processual character of innovation than Dosi's:

> Although tastes, technological opportunity, and appropriability conditions themselves are subject to change over time, particularly in response to radical innovations that alter the technological regime, these conditions are reasonably assumed to determine the interindustry differences in innovative activity over relatively long periods. (Cohen & Levin 1989:1095)

Cohen & Levin conclude that:

> much of our empirical understanding of innovation derives not from the estimation of econometric models, but from the use of other empirical methods. As we have illustrated with examples, the case study literature provides a rich array of insights and factual information. (Cohen & Levin 1989:1098)

Dosi ends by saying that "the domain of innovation" is: "a major - and still largely unexplored - frontier of economic analysis" (Dosi 1988:1165).

This thesis is largely case-study based. Whilst it does not attempt to set its observations within an economic framework, it would indeed show a lack of 'vision' not to acknowledge the mutual relevance of many economic ideas - the more so because economic writers like Dosi and Cohen & Levin are themselves at pains to acknowledge the insights that more interpretive methods and case-study based approaches provide for their work.

II.II THE VENTURE CAPITAL LITERATURE

It is scarcely necessary to reiterate the place of venture capital in the debate of this thesis - chapter five considers this in some detail, and presents the findings of a wide range of interviews with venture capitalists. Nevertheless, a quick review of some other threads of research in this area, together with brief mention of a few useful sources of information, is appropriate in this appendix.

It is useful to start with a working definition - that of Mason & Harrison is broadly consistent with the tenor of the research in this thesis:

> We define venture capital as an activity by which corporate investors support entrepreneurial ventures with finance and business skills to exploit market opportunities and thus obtain long-term capital gains. (Mason & Harrison 1991:203)

The Venture Capital literature is reviewed by McNaughton (1991). He looks mainly at empirical spatial studies, especially those of economic geographers, which he classifies under the four headings of locational studies, market taxonomies, market specialization and economic impact. The last of these is a clear point of contact with the ideas described in the last section, and McNaughton's comments here allow the underlying differences of theoretical standpoint to be glimpsed. He says:

> The modern view of entrepreneurship in economic theory can be traced to two sources: (1) the work of those economists who have been influenced by the writings of Joseph Schumpeter and (2) the Austrian School of Economics. (McNaughton 1991:9)

He characterizes the two, very broadly, in the following way:

> Each addresses an important component of technological change. Schumpeter's entrepreneur causes disequilibrium in the market by introducing a new innovation. The Austrian's entrepreneur takes advantage of the uneconomic prices that result from this initial monopoly through imitation. (McNaughton 1991:11)

He discusses various ways of adding the venture capitalist to such models - for example:

> As the central actors in this system, venture capitalists lend structure and coherence to the dynamic process posited by entrepreneurial economic theory (McNaughton 1991:12)

McNaughton also briefly considers the business literature on venture capital, which he says is "substantial" (p.13). The definition of risk is a major problem he notes here. He describes three threads in the research, and comments upon its limitations:

> Business researchers have traditionally focused on (1) concerns for the investment decision-making behaviour of venture capitalists, (2) the evaluation of the performance of venture capital portfolios, and (3) the availability and cost of venture capital. The result has been a rather static interpretation of venture capital, based largely on analogies with larger financial markets. (McNaughton 1991:15-16)

The geographer's spatial model contrasts sharply with such an interpretation, and with economic perspectives - McNaughton cites the old 'conventional' economic model as seeing simple diffusion of supply leading to equilibrium, whereas the new 'geographical' model is spatially constrained and constantly shifting. He endorses the latter, summarizing them thus:

> The conventional model predicts that venture capital will eventually be equally available in all regions, while the geographical model holds that some areas will never be supplied. (McNaughton 1991:16)

Others of the essays in the collection in which McNaughton's is published illustrate the very clear regional differences of venture capital that are seen in reality (for example, Mason & Harrison (1991) consider the UK, and Perry (1991) the extremely idiosyncratic rise and fall of venture capital in New Zealand). One further essay deserves comment here - that of Thompson (1991), since in some ways its aims echo those of the present research:

> there has so far been little systematic analysis of the key personnel at the very heart of this important business: the individual professional venture capitalists themselves. (Thompson 1991:134)

As he says: "Rarely, if at all, ... is the term 'venture capitalist' used to refer to the actual individuals" (p.136). Thompson analyses the entries in a who's who of venture capitalists, and compares education with work location with age. As he is the first to admit, this is a difficult task, and the results are undeniably fraught with various assumptions and tantalizing inadequacies in the data, but it is a most promising first attempt to use such data, and it does challenge very strongly the economic diffusion model discussed above. As Thompson says:

> It uses the empirical results from analyses of biographical data .... to address hypotheses which flow from the conventional model of the venturing space-economy. The drawbacks to the data-set require that these results be viewed as suggestive rather than confirmatory, and that any conclusions be stated very tentatively. Nevertheless, given the blind enthusiasm with which the trickle-down theories have sometimes been adopted to justify hands-off policies, this caution can be seen as a strength and virtue of the approach and results, rather than a weakness. (Thompson 1991:157-158)

Thompson sees the use of data about individuals as a rich way of investigating the many questions surrounding venture capital - an opinion that is strongly endorsed in the present thesis.

It is interesting, as an aside, to note Thompson's comments about New York, where some of the present interviews were conducted - he says of US venture capitalists (admittedly in 1984) that: "New York City alone has .... 21 per cent of the nation's total, and this is almost double the number in the second ranked city, Boston" (pp.145-146).

Thompson's work is interesting not simply because it teases so much out of relatively little data, but because it uses 'soft' biographical data in a disciplined and orderly way - a method that this thesis also tries to demonstrate. He illustrates that purely quantitative approaches do not have the monopoly of rational and rigourous analysis - such analysis can equally well serve qualitative, even interpretive, data. As Thompson suggests, the best model is

one that does not, in a flight to numbers, disown the true complexity of its subject matter; such a model maintains its explanatory power:

> It achieves this because it does not merely see the supply of venture dollars, the surface over which they are spread, and the individual venturers themselves as neutral components of a system governed by physical or geometric laws. Rather, it allows individuals to be willful actors who are influenced by institutional and behavioral constraints, and who exist in an economic landscape made of real and uneven places and contexts. (Thompson 1991:159)

It is interesting, in the present context, to note Thompson's comment in a footnote (p.161), that his present research is "pursuing some of these questions through personal interviews of venture capitalists and recipient firm founders".

It only remains in this appendix to cite those references that were found useful in considering venture capital during this research - they represent a considerable resource of data. The two primary directories used were Lucius Cary's "The Venture Capital Report Guide to Venture Capital in the UK", and "Pratt's Guide to Venture Capital Sources" for the US - both are regularly published, the fourth edition of the VCR Guide (Cary 1989), and the thirteenth edition of Pratt's Guide[3] (Morris & Isenstein 1989) being used here. The British Venture Capital Association also publishes a small directory of its members.

In the UK, the periodical reports by the Bank of England also provide useful economic overviews (Bank of England 1982, 1984, 1990). The essay by Mason & Harrison (1991), referenced earlier in this section, is a good critical review of both the UK venture capital industry's methods and economics, as well as the wider issues of debate surrounding it - the essay provides an excellent introduction to all these aspects. A

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[3] Pratt's Guide is published by 'Venture Economics' who also publish venture capital journals in both the US and the UK.

final book that deserves mention is Wilson (1985) - it presents the history of the recent rise of venture capital in the US - despite its popular and positive style, it provides considerable insights into how venture capital is organized and operates.

APPENDIX III - THE METHOD

The methodological arguments surrounding the use of the historical case study that forms part of the present research are outlined in the main body of this thesis. However, the choice of using historical data was in some ways novel, this appendix therefore provides a little more discussion of it.

The key concern in choosing an appropriate subject for the case study was the problem of material for analysis. On the basis that the survival of material ought to be inversely proportional to its age, the choice of a study around a century old made sense, but what to analyse remained the main question. Interviews yield personal opinion - mediated no doubt by the person's attitude to the researcher, but not filtered by anyone else's interpretation. The most similar historical documents are personal letters - their content might depend on the recipient, but they are still a primary product of the mind of the person of interest. Some hint of their possible utility for analysis had been gained from the limited use made of verbatim evidence (to the House of Lords) in the HOTOL study, and this provided the incentive to proceed. The rôle of Charles Parsons and Admiral Fisher in the introduction of the marine steam-turbine, in the first years of the twentieth century, was chosen for study.

However, before discussing the use of these men's letters in more detail, one more point arising from the interview experience needs comment. In conducting an interview, the researcher enters the world of the person interviewed. The context, its everyday details and concerns, and its 'feel', are all absorbed, and they in turn play a considerable part in interpretation. At the most basic level, if a certain topic, or even name, arises in an interview, its wider importance is likely to be perceived by the suitably attuned researcher. The need for this sort of contextual facility raises more difficulty with words from another age. The hermeneutic

problem of the contemporary researcher is that today's perceptions may be projected onto material from the past. This has to be countered in two ways. First a degree of sceptical eclecticism is needed - the more past perceptions that are collected, then the more rounded the picture. Second, full use must be made of secondary material - Parsons, Fisher, and their fellows indulged in lectures, papers, pamphlets, and enquiries - just as people involved in HOTOL do today. These artefacts are directly comparable, and provide a further check on too partisan a reading of the past.

The big question, however, was what suitable correspondence concerning the marine steam-turbine is available. Considerable time - probably too much - was expended looking at this, and at whose correspondence might be useful. With HOTOL a wide range of people were interviewed, including both the technical innovators themselves, and those in Government and elsewhere who were instrumental in the story. The same double approach was desirable for marine steam-turbines. The 'inventor', Charles Parsons, was obviously the technical innovator to look at. As a token 'facilitator', a good choice seemed to be the one man associated (rightly or wrongly) in popular historical imagination with founding a turbine Navy - Admiral of the Fleet Lord Fisher of Kilverstone - 'Jackie' Fisher.

The choice of Fisher may appear superficial, but it is consonant with the aims of this thesis - as the interview data shows, popular opinion plays a major part in innovation (however misguided it may appear to those involved) so the subjects it lights upon deserve, perhaps demand, study. Moreover, Fisher was most certainly involved, so his study might be expected, at the least, to inform some of the issues involved.

Primary Parsons material exists in the Science Museum Library (who hold a collection of his papers), and in the

Rosse Papers in the Northern Ireland Public Records Office, and catalogues were obtained. In addition a visit was made to the Tyne and Wear Archives in Newcastle, who hold some relevant early material, and were getting more. Fisher material exists in many places (see the prefaces to Marder (1952, 1956, 1959) for more details), and a few were investigated (the National Maritime Museum, the Ministry of Defence Whitehall Library).

In the end the very wealth of material demanded either a thesis dedicated to it, or a re-thinking of its place in the present thesis. This wealth highlighted a subsidiary task for the analysis - if successful, the analysis might demonstrate the usefulness of management research based upon such rich resources of historical data. In essence, the possible benefits can be cast in the form of a question: why make the enormous effort involved in collecting good and deep contemporary management data, if another source is more easily available? The published primary data relevant to steam-turbines looked a very convenient source with which to investigate this question. It was also a suitable basis for the historical case-study that the thesis needed.

The expedient of using only published primary material was chosen to contain analysis. Here, Fisher has been particularly well served by Marder (1952,1956,1959) whose three volumes of edited correspondence contain no fewer than 1,225 letters (although a few more letters are published in the biographies and suchlike mentioned elsewhere), in addition, Kemp (1960) has published many of Fisher's papers. Parsons's correspondence is liberally quoted by his rather over-enthusiastic biographer Appleyard (1933) (who also includes a few Fisher letters to Parsons, not in Marder). Again a few other sources of Parsons's letters exist (Clarke (1984) and a few short extracts in the catalogue of the Rosse Papers), together with his published lectures and suchlike (see Appleyard for a list, and his nephew's collection - Parsons (1934); also Kemp (1960)). Relevant passages from

all of these were chosen, and analysed in the same way as the research interviews.

The appendix to chapter four describes the analysis of Parsons and Fisher in greater detail, together with HOTOL and the other technologists. That to chapter five does the same for the finance interviews.

APPENDIX IV - THE PROVIDERS OF IDEAS

The interviews dealing with technology were both the first and the last to be obtained and analysed. Broadly speaking HOTOL came first, then an overview of the material available on Parsons and Fisher, then the finance interviews of chapter five, then the other technology interviews, and then the choice and analysis of Parsons's and Fisher's letters.

The analysis developed throughout this period - the emergent categories from HOTOL aided analysis of finance, and both of these aided treatment of the historical material. Chapter three describes the basic methodology, the appendix to chapter five covers the underlying questions in more detail, but this appendix covers the specific problems in the interpretive analysis of technological ideas.

The problem underlying the technological data is that, whilst the common categories that are the stuff of this analysis emerge quite easily, they do not have the coherence inherent in the finance interviews. Finance could be treated as one community - a snapshot in time - and whilst broad, the community still bounded what the people interviewed said. The people involved in technological innovation, however, were telling a story - all played different parts. Thus, whilst great consensus emerged about the important issues (the topics of the analysis), everyone had a different perspective (indeed they were often chosen for that very reason) - dissonance and difference were built in. Chapter four thus tells several stories, chapter five presents a more conventional ethnography.

This appendix is split into three parts - the first deals with treatment of the historical data, which, since it was analysed last, also included some of the most developed reflections on the analysis. The second part describes the detailed analysis of HOTOL, and the third part does the same for the other contemporary data on technologists. All parts

give details of how the categories emerged, and how they were treated. The potency of these details is ephemeral, and tied to a transient stage in the preparation of the thesis. They are so intimately intertwined with the researcher and the process of writing, that it is only their husks that can be presented here - outside of that process. They are of little more than passing interest, and certainly do not warrant inclusion in the main part of this thesis. They are presented, therefore, in this appendix merely as commentary on the discussion of the main body of the thesis.

IV.I HISTORICAL DATA

This section discusses some detailed points concerning the analysis of the historical data. The hope was that this data would be as rich as a live interview, whilst at the same time allowing the long-term success or failure of the innovation it described to be known. Surviving documents are much more limited than an interview, and much more liable to tampering motivated by personal vanity, sycophantic eulogizing, or whatever. Nonetheless, letters, and particularly the letters of an age when such correspondence seemed more widespread, do bear a striking resemblance to the texts transcribing the contemporary interviews. On this basis, analysis was attempted.

The first thing that became very apparent was the profusion of people, and even places, mentioned in the letters and other material. Some of these names were familiar, most were not, and this was a recipe for considerable complexity and confusion - especially when added to the span of time over which the letters were written (an element not, of course, so noticeable in the contemporary interviews). The solution was simply to prepare a card index of these people. This was continually augmented as new people appeared in the literature. Also useful was a table of who was where when, particularly at the Admiralty - where four Sea Lords and a

civil First Lord, all continually changing, defied ready comprehension. This index and table were effective in mimicking the everyday cultural competence in this area that is a by-product of face-to-face interviews.

In an interview the researcher chooses questions, which the person interviewed may follow or not. With a letter the researcher chooses the fragments that seem relevant to the problems of innovative technology (thereby adding to the inherent vagaries of documentary survival). But the resulting multiply-distilled discourse of Charles Parsons and 'Jackie' Fisher seemed little different to analyse than HOTOL or finance interviews. The following is a list of the categories that emerged:

TOPICS RAISED IN LETTERS OF CHARLES PARSONS (P) & LORD FISHER (F) & ROUGH QUALITATIVE ASSESSMENT OF DEGREE THEY WERE DISCUSSED

	P	F	TOPIC	P%	F%	Both%
A?			PERSONAL HISTORY	6.1	5.7	5.9
B?			INNOVATION HISTORY	12.1	0.0	5.9
C?			PATENTS, LICENSING	6.1	0.0	2.9
D?			MARKETS, MARKETING, SALES	6.1	2.9	4.4
E?			LOCAL PROBLEMS	6.1	8.6	7.4
F?			FINANCE, RETURNS	6.1	0.0	2.9
G?			RISK	3.0	0.0	1.5
H?			FASHION	3.0	0.0	1.5
I?			AIMS	6.1	5.7	5.9
J?			ORGANIZATION, BUSINESS	9.1	2.9	5.9
K?			METHODS	12.1	11.4	11.8
L?			LABOUR, PEOPLE	3.0	5.7	4.4
M?			PUBLICITY	3.0	11.4	7.4
N?			WRITING, WORDS, EPIGRAMS	3.0	11.4	7.4
O?			GOVERNMENT INVOLVEMENT	3.0	2.9	2.9
P?			INVENTORS, INVENTION	9.1	5.7	7.4
Q?			OVERALL PROBLEM	3.0	2.9	2.9
R?			PERSONAL METHODS	0.0	11.4	5.9
S?			JOB OFFERS	0.0	5.7	2.9
T?			FOREIGN COMPETITION	0.0	5.7	2.9
			ORGANIZATIONS and INNOVATIONS			
A			STEAM TURBINES			
B			SHIPS			
C			BOILERS			
D			SCREWS			
E			OTHER INVENTIONS			
F			ARMSTRONG'S etc.			
G			CUNARD			
H			OIL, GAS TURBINES			
I			GUNS			
J			DOCKYARDS etc.			
			PEOPLE			
1			PARSONS			
2			FISHER			
3			LORD RAYLEIGH			
4			Laurence PARSONS			
5			John Fletcher MOULTON			
6			LORD KELVIN			
7			Sir Alfred Fernandez YARROW			
8			Sir William Henry WHITE			
9			LORD ARMSTRONG			
10			Sir Andrew NOBLE			
11			Sir Phillip WATTS			
12			Viscount ESHER			
13			Rollo APPLEYARD			
14			Winston CHURCHILL			

LEGEND

- Topic mentioned
- Brief discussion of topic
- More detailed discussion of topic
- Topic discussed in considerable detail

As with the other analyses below, the category names are fairly arbitrary; multiple names were applied to each category to catch the full nuances of the material included. The following provides an index of all the historical category names, for ease of reference:

INDEX OF CATEGORIES TO EMERGE FROM PARSONS AND FISHER LETTERS

AIMS . . . . . I?
APPLEYARD, Rollo 13
(1867-1943)
ARMAMENT FIRMS . . . . . F
ARMSTRONG, LORD William, G. 9
(1810-1900)
ARMSTRONG'S & OTHER ARMAMENT FIRMS . . . . . F
BOILERS C
BUSINESS . . . . . J?
CHURCHILL, Winston 14
(1874-1965)
COMPETITION, FOREIGN . . . . . T?
CUNARD . . . . . G
DOCKYARDS etc. . . . . J
EPIGRAMS . . . . . N?
ESHER, Viscount (Reginald Baliol BRETT) . . . 12
(1853-1930) . . . . . .
FASHION . . . . . H?
FINANCE . . . . . F?
FISHER of KILVERSTONE, BARON John Arbuthnot . 2
(1841-1920)
FOREIGN COMPETITION . . . . . T?
GAS TURBINES . . . . . H
GOVERNMENT INVOLVEMENT . . . . . O?
GUNS I
HISTORY, PERSONAL . . . . . A?
HISTORY of PARTICULAR INNOVATION . . . . . B?
INNOVATION HISTORY . . . . . B?
INVENTION . . . . . P?
INVENTIONS, OTHER . . . . . E
INVENTORS . . . . . P?
JOB OFFERS . . . . . S?
KELVIN, LORD (William Thomson) 6
(1824-1907)
LABOUR . . . . . L?
LICENSING . . . . . C?
LOCAL PROBLEMS . . . . . E?
MARKETING . . . . . D?
MARKETS . . . . . D?
METHODS . . . . . K?
METHODS, PERSONAL . . . . . R?
MOULTON, John Fletcher, QC 5
NOBLE, Sir Andrew 10
(1831-1915)
OFFERS of JOBS . . . . . S?
OIL . . . . . H
ORGANIZATION . . . . . J?
OTHER INVENTIONS . . . . . E
OVERALL PROBLEMS . . . . . Q?
PARSONS, Sir Charles Algernon 1
(1854-1931)
PARSONS, Laurence (4th Earl of Rosse) . . 4
(-1908)
PATENTS . . . . . C?
PEOPLE . . . . . L?
PERSONAL HISTORY . . . . . A?
PERSONAL METHODS . . . . . R?
PROBLEMS, LOCAL . . . . . E?
PROBLEMS, OVERALL . . . . . Q?
PUBLICITY . . . . . M?
RAYLEIGH, LORD (John Strutt) 3
(1842-1934)
RETURNS . . . . . F?
RISK . . . . . G?
SALES . . . . . D?
SCREWS . . . . . D
SHIPS . . . . . B
STEAM TURBINES . . . . . A
TURBINES, STEAM . . . . . A
TURBINES, GAS . . . . . H
WATTS, Sir Phillip 11
(1846-1926)
WHITE, Sir William Henry 8
(1844-1913)
WORDS . . . . . N?
WRITING . . . . . N?
YARROW, Sir Alfred Fernandez 7
(1842-1932)

The categories are very similar to those that emerge in the three sets of contemporary interviews. The broad division

used there was maintained - into 'topics' (opinion, symbol, idiosyncrasy, etc.), 'organizations' (more factual comment, basically on groups bounded by common membership), and 'people'. The choice of topic remained fairly free, though some attempt was made to resist the proliferation of small topics to which the finance analysis had fallen prey. Only one new, and useful, departure was made here - 'organizations' were taken to include bodies of fairly factual comment on aggregations of ideas as well as people. This is self explanatory from the category names ('ships', for example), and includes discourse on what new designs incorporated, on the engineering arguments for certain innovations, on when things happened and suchlike. In some measure this also reflected the new historical dimension - letters came from a wide span of time - there was more scope for sheer historical comment and reflection. This is a useful addition - a place for more factual comment was sometimes needed in earlier analyses, allowing the 'topics' to be kept clear for much more personal and uncertain comment - as the question mark in their arbitrary designating letter (the order they emerged) tries to imply.

Included in the list of categories above, is a very rough estimation of the depth in which each category was treated. This illustrates how Parsons and Fisher differed in what they saw as important. This, of course, assumes that their surviving letters are representative in this matter. Here, there is some comfort in the distributions of the categories; Parsons seems more concerned with engineering and business aspects, Fisher with his own ways of influencing opinion and events - both fit the rôles that other evidence suggests they played in naval engineering innovations at the turn of the century.

Of particular note here (and in the other analyses below) is the prominence of the 'methods' topics (particularly 'personal methods' for Fisher, and 'innovation history' - i.e. how turbines happened - for Parsons). On these cards there

emerged a coherent picture of their day-to-day activities (or at least the more colourful and salient ones). The other topics often seem to define either what the symbolic inputs and outputs of these activities were, or their assessment of how the inputs impinged upon this daily round, and how the outputs were mediated. Indeed the extended 'organization' categories were in many ways the concrete equivalents of these symbolic 'topics'. The over-riding observation is that the emergent categories are all very different in character, feel, mode of thought - and yet they all exist quite easily within the same text. Moreover, whether that text is the transcribed speech perceived in an interview, or the transcribed thoughts of an old letter, the discourses that they represent (and that the categories distill further aspects of) still seem to emerge.

One final mechanical point is worthy of comment. Analysis demands time for thought, it also demands expansive access to all the categories at once. Until the historical case-study, analysis had been undertaken by manually cutting up texts, and gluing them on to physical coloured cards. With Parsons's and Fisher's letters the data available for analysis was at least four times that of even the longest interview. This (and the acquisition of a new printer) led to the decision to print the text for analysis in print so fine that physical cutting became rather difficult. Also, the data spanned decades - ideas, opinions, practices, all changed - and clear dating of fragments was essential. The upshot was that the 'cutting' was done by computer, and the 'cards' put together, with dates attached to each fragment, in a word processor file. In practice this was easier than had been feared - it took about the same time as the physical alternative - time for thought was maintained. Nonetheless, for use in writing the thesis, the aggregated text for each category was still printed out, and glued to coloured file cards, entirely compatible with those from the interview analysis.

IV.II HOTOL

The study of HOTOL was the first research of this thesis, and many of the interview and analysis techniques used in later interviews were first tried with the HOTOL data. Chapter three covers the more general methodological aspects fully; they are not repeated here. These appendices are reserved for more specific comments on the development of the analysis. HOTOL represented only the starting point for this development, that finance, the historical material (just considered), and the technology interview analysis, continued. Discussion here is therefore limited to a simple presentation of those elements of analysis specific to HOTOL. Fuller discussion of the overall development of interview analysis is left to the appendix to the finance chapter.

The first unique characteristic of HOTOL is, of course, its context. If a new world had to be learnt for the analysis of the Parsons data, a precisely inverse problem faced the researcher into HOTOL. The slang, the acronyms, even the ephemeral soubriquets, were, if anything, too familiar to the researcher, who had himself worked in the aerospace industry for over a decade, including both of the companies involved in HOTOL. To redress this (and to provide the reader with the same sort of aide-mémoire that the researcher constructed for himself in the case of the Parsons material), the following glossary is included, and after that a list of those interviewed (augmented with basic statements of their organizations and positions).

HOTOL GLOSSARY

AERE	Atomic Energy Research Establishment
Ansty	RR site at Coventry associated with rocket work
Ariane	Family of joint European rocket developments
BAe	British Aerospace PLC
BIS	British Interplanetary Society
BNSC	British National Space Centre
CNES	Centre National d'Études Spatiales (France)
Columbus	European elements of NASA-ESA Space Station
Culham	AERE establishment
DGEng	Director General of Engines (MoD PE)
DTI	Department of Trade and Industry
ESA	European Space Agency
Farnborough	An RAE (DRA), venue for major two-yearly air show
Filton	BAe site, associated with Concorde and other civil aircraft work
Hermes	European proposal for a space shuttle
HOTOL	Horizontal Take-Off & Landing
ITN	Independent Television News
MoD	Ministry of Defence
NASA	National Aeronautics & Space Administration (USA)
NASP	National AeroSpace 'Plane (USA) - first prototype designated X30
NGTE	National Gas Turbine Establishment
Patchway	RR site at Bristol
PE	Procurement Executive (MoD)
Platform	Space platform - part of NASA-ESA space station
PV	Private Venture
Pyestock	National Gas Turbine Establishment (near Farnborough)
RAE	Royal Aerospace (originally Aircraft) Establishment - recently renamed Defence Research Agency (DRA)
RAeS	Royal Aeronautical Society
RB545	RR designation of HOTOL engine
RFP	Request For Proposals
ROF	Royal Ordnance Factories (now privatized)
RR	Rolls-Royce PLC
SABRE	Synergetic Air-Breathing and Rocket Engine (renamed and re-matched SATAN engine)
Sänger	German proposal similar to HOTOL
SATAN	Swallow with Alternative Thermodynamics and Adapting Nozzles (Alan Bond's new engine - see also SABRE)
Space Station	Joint NASA-ESA project (see also Columbus, and Platform)
SSTO	Single Stage To Orbit
Stevenage	BAe site - centre of space and communications activities
Swallow	Early HOTOL designation, referring in particular to the engine
UKAEA	United Kingdom Atomic Energy Authority
Warton	BAe site associated with military aircraft - centre for later HOTOL work

INTERVIEWS WITH THOSE INVOLVED IN HOTOL
(In order of interviewing; titles as at time of interview.)

Date	Person		Organization	Position
12- 7-88	B.R.A. BURNS		BAe Warton	Project Manager
12- 7-88	Alison WAKE		BAe Warton	Project team member
12- 7-88	Steve FURNISS		BAe Warton	Project team member
15- 7-88	Alan BOND		AERE Culham	HOTOL Originator
26- 7-88	Dr. R.C. PARKINSON		BAe Stevenage	HOTOL Originator
2- 8-88	Ken LAVENDER		AERE Culham	Worked for Alan Bond
3- 8-88	Sid WALMSLEY		BAe Warton	Project team member
3- 8-88	Nick DALE		BAe Warton	Project team member
3- 8-88	Steve GILKES		BAe Warton	Project team member
3- 8-88	Alec DAVIES		BAe Warton	Project team member
4- 8-88	John SCOTT-SCOTT		RR Ansty	HOTOL Originator
9- 8-88	A.C.NICHOLAS / A.HICKS		BNSC	Cover HOTOL within BNSC
11- 8-88	Dr. Brian LOWRIE		RR Bristol	Chief Engineer, High Speed Propulsion
2- 9-88	Dr. Tony HEWWIT		RR Bristol	Project Manager
5-10-88	Roy GIBSON	*	BNSC	Ex Director General
13-10-88	Gordon LEWIS		RR Bristol	Ex Director
16-11-88	Sir Richard NORMAN		MoD	Ex Chief Scientific Adviser to MoD
1-12-88	Sir Geoffrey PATTIE	*	Government	Ex Minister at DTI
10- 1-89	Sir Francis TOMBS	*	RR	Chairman
20- 1-89	Clive LEYMAN		BAe Filton	Chief Engineer
6- 2-89	Peter CONCHIE		BAe Stevenage	Director of Business Development
3- 3-89	Ivan YATES	*	BAe	Deputy Chief Executive (engineering)
25- 5-90	Frank MILES		ITN News	Ex Head of Science
24- 9-90	Sir Austin PEARCE		BAe	Ex Chairman
23-10-91	Alan BOND		Reaction Engines Ltd.	[REPEAT INTERVIEW]

* Interviewed by House of Lords Select Committee - transcripts used in analysis (evidence of Kenneth Clarke - Minister DTI - also used. Peter Conchie also provided evidence to the Committee).

The people interviewed all had some connection with HOTOL, some tenuous, but mostly strong, and in the cases of the originators seminal. There were a few others who could have added to the picture, but it is fair to claim that the body of interview data included the great majority of those instrumental in HOTOL. However, in the research each new interview, and each new reflexive pass of the analysis, changed the interpretation - diverse elements associated themselves in unexpected ways, hanging questions resolved themselves, other questions emerged. In addition, HOTOL is still going on. So, despite the breadth of data, no

interpretation could ever be comprehensive; nonetheless, it is hoped that the interpretation that emerged within the main text of this thesis may still be of relevance beyond HOTOL itself.

The following table is a list of the topics used in this interpretation of HOTOL. The percentage figures alongside the topics are a rough and qualitative guide to the amount of comment each received in the discourse of the interviews (the appendix to the next chapter discusses this more fully); for conciseness the interviews have been aggregated into very broad organizational groupings for this purpose[4]. It is apparent that many topics demanded several names - as they grew out of the analysis they bent and contorted to include subtly different shades of comment. An index of the topics is therefore also included for convenience, covering all these ephemeral names. The table following the index presents the topics, and who discussed which, in an even more detailed visual form.

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[4] The second interview with Alan Bond was not included in these figures since its discussion was deliberately more directed, being biased by topics raised in other parts of the research - its analysis is included in the later table, however.

TOPICS TO EMERGE IN HOTOL INTERVIEWS & ROUGH QUALITATIVE ASSESSMENT OF DEGREE THEY WERE DISCUSSED

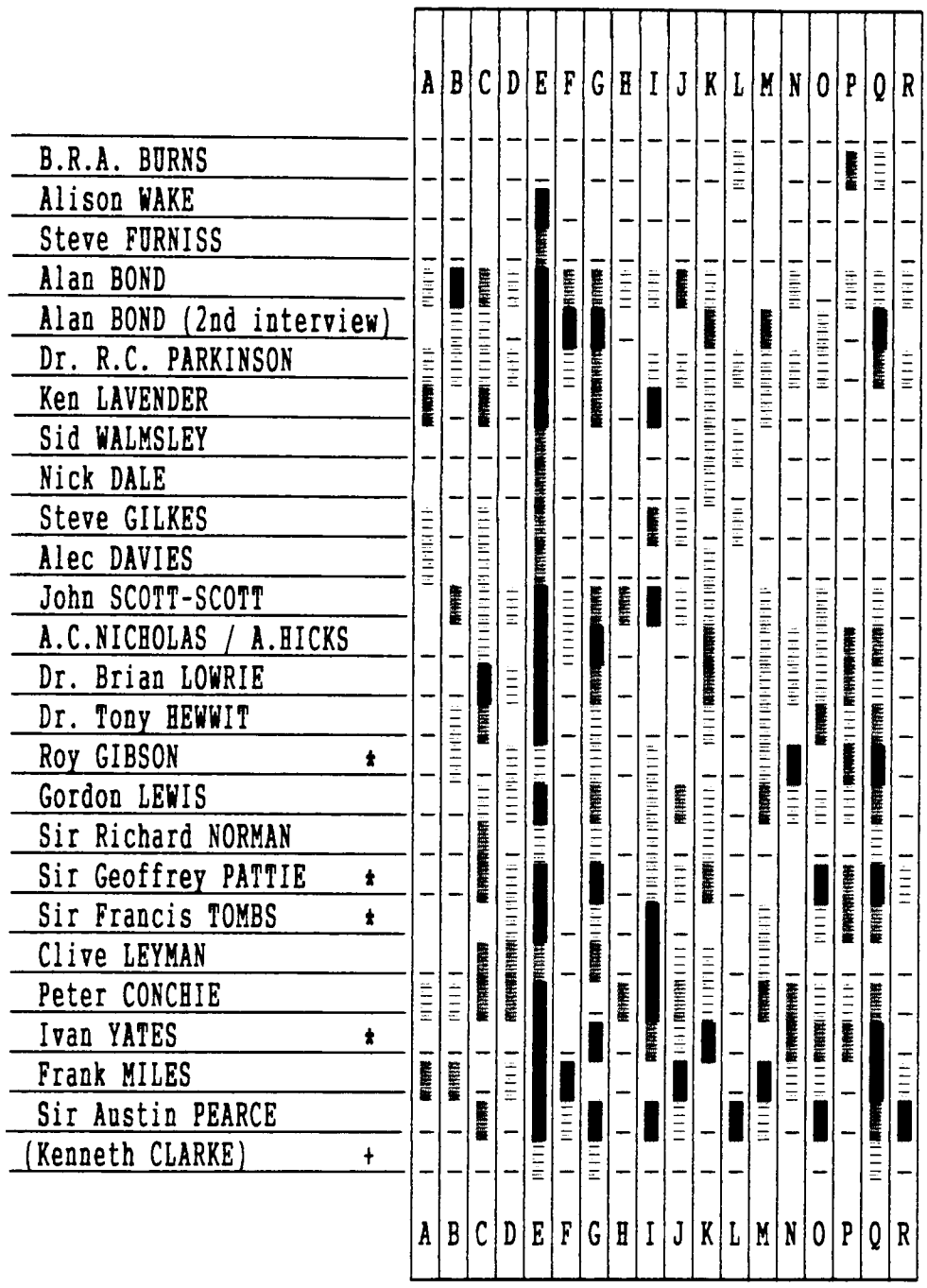
	TOPIC	Originators* %	BAe %	Govt. %	R-R %	Journalists %	Others %	OVER ALL %
A?	MOTIVATION	4.4	3.5	0.0	0.9	8.1	14.3	3.2
B?	PERSONAL TURNING POINTS, FEELINGS	7.9	1.5	1.8	2.6	8.1	0.0	3.3
C?	MANAGEMENT SYSTEMS, POLICY	6.1	7.9	7.0	8.7	0.0	14.3	7.3
D?	PROJECT TURNING POINTS, UNEXPECTED EVENTS	5.3	4.0	4.4	6.1	5.4	0.0	4.6
E?	WAY THEY WORK, SORT OF PEOPLE THEY ARE	10.5	16.8	12.3	13.9	10.8	19.0	13.9
F?	HISTORY, CONTROLLABLE EVENTS	6.1	2.0	3.5	0.9	10.8	0.0	3.3
G?	FUNDING	7.9	6.4	12.3	8.7	2.7	14.3	8.3
H?	SIZE AND SCALE	5.3	2.0	0.0	0.9	0.0	0.0	1.8
I?	CREATIVITY, INNOVATION	7.0	9.9	6.1	6.1	2.7	19.4	7.8
J?	NATIONAL PUBLICITY	6.1	5.4	2.6	3.5	10.8	0.0	4.8
K?	EXPERTISE, EXPERIENCE, KNOWLEDGE	5.3	7.4	7.0	7.0	2.7	9.5	6.6
L?	SPECIALIZATION, PROFESSIONS	3.5	5.4	0.0	0.9	0.0	0.0	2.7
M?	WORDS	4.4	6.4	5.3	7.8	10.8	9.5	6.5
N?	WHAT THE PROJECT IS	4.4	3.0	7.9	5.2	5.4	0.0	4.6
O?	BASIC PROBLEMS	3.5	4.5	6.1	7.8	5.4	0.0	5.1
P?	FUTURE	2.6	5.0	8.8	8.7	0.0	0.0	5.5
Q?	FOREIGN INTEREST, FOREIGN PRACTICE	6.1	6.9	13.2	9.6	10.8	0.0	8.5
R?	POLITICS	3.5	2.0	1.8	0.9	5.4	0.0	2.2

[* does not include second interview with Alan Bond]

INDEX OF TOPICS TO EMERGE FROM ANALYSIS OF HOTOL INTERVIEWS

BASIC PROBLEMS . . . . . O?
CONTROLLABLE EVENTS . . . . . F?
CREATIVITY . . . . . I?
EXPERIENCE . . . . . K?
EXPERTISE . . . . . K?
FEELINGS . . . . . B?
FOREIGN PRACTICE . . . . . Q?
FOREIGN INTEREST . . . . . Q?
FUNDING . . . . . G?
FUTURE . . . . . P?
HISTORY . . . . . F?
INNOVATION . . . . . I?
KNOWLEDGE . . . . . K?
MANAGEMENT SYSTEMS C
MOTIVATION . . . . . A?
NATIONAL PUBLICITY . . . . . J?
PERSONAL TURNING POINTS . . . . . A?
POLICY . . . . . C?
POLITICS . . . . . R?
PROFESSIONS . . . . . L?
PROJECT TURNING POINTS . . . . . D?
SIZE AND SCALE . . . . . H?
SORT OF PEOPLE THEY ARE . . . . . E?
SPECIALIZATION . . . . . L?
UNEXPECTED EVENTS . . . . . D?
WAY THEY WORK . . . . . E?
WHAT THE PROJECT IS . . . . . N?
WORDS . . . . . M?

TOPICS RAISED BY THOSE INTERVIEWED INVOLVED IN HOTOL



* House of Lords evidence included in analysis
\+ Only House of Lords evidence used

Topic mentioned
Brief discussion of topic
More detailed discussion of topic
Topic discussed in considerable detail

Numbers and tables make qualitative data look quantitative, tentative interpretation look like hard fact. This is not the intention - it is simply an artefact of the prevailing analytical canons, of the currently priveleged discourses in management studies. Simply to de-privilege that discourse, however, would deny the utility of numerical and graphical presentation to qualitative data. Much more seriously, it would deny rigour and robustness to the analysis of that data - the appendices to this thesis are, in part, a sort of inoculation against these ills. Nonetheless, the mixing of analytical 'styles' attempted here makes it even more necessary continually to emphasize that the topics are not concrete entities, but shifting mental categories in the researcher's head. A repeat of the analysis would yield different topics - indeed, experience of HOTOL helped the analysis of finance and the technologists to do just that. Analysis of the last few HOTOL interviews (after the finance ones, and incidentally using the Parsons computerized technique discussed above) prompted yet further reflection. In short - there is no equilibrium in this productive reflexive cycle, but comparison of the successive cycles (that these appendices set down on paper) is not only instructive, it is of first importance to the analysis itself - as the appendix to chapter six will show.

A final thread of the analysis emphasizes its multiple 'styles'. The topics stuck onto on the yellow cards were not the only categories - references by people to other people, and to organizations, were also noted and recorded on the green and red cards. The following table shows who cross-referenced whom and what (note that the letters indicate people, not topics, in this table). The result is extremely suggestive of the close, and important, networks of contacts that surrounded HOTOL.

LINKS BETWEEN THOSE INTERVIEWED INVOLVED IN HOTOL

		A	B	C	D	E	F	G	H	I	J	K	L	M	N	O	P	Q	R	S	T	U	V	W	X	Y	*	+	#
A	B.R.A. BURNS	o	w	w		P		w	w	w	w		P			O					O	O				P			
B	Alison WAKE	w		w		O		w	w	w	w											O							
C	Steve FURNISS	w	w					w	w	w	w																		
D	Alan BOND (both ints.)	P	O	O	o	P	O	O	O	O	O	P	O	P		O			P	P	O		P	P		P	P	O	O
E	Dr. R.C. PARKINSON	P	O	O	P	o		O	O	O	O	P					P		P		O	P				P	P	O	O
F	Ken LAVENDER				P		o																						
G	Sid WALMSLEY	P	w	w					w	w	w											P							
H	Nick DALE	P	w	w				w		w	w																		
I	Steve GILKES	O	O	O				O	O	o	O																		
J	Alec DAVIES	w	w	w		O		w	w	w												O							
K	John SCOTT-SCOTT				P	P								P			P		P	P	P	P		P		P	P	O	O
L	A.C.NICHOLAS / A.HICKS	O	O	O				O	O	O	O		o			O		O										O	
M	Dr. Brian LOWRIE				P	P						P	O		P	P	P			P								O	
N	Dr. Tony HEWWIT			P	P	P								w						P									
O	Roy GIBSON												O			o		O	P	P						P			
P	Gordon LEWIS				P	O						P	O	P		P		P	P	P		O		P			P		
Q	Sir Richard NORMAN																	o										O	
R	Sir Geoffrey PATTIE				P											P		O		P						P	P	O	
S	Sir Francis TOMBS				P																								
T	Clive LEYMAN	O	O	O		P		O	O	O	O	P						O			o	O	P				P		
U	Peter CONCHIE	O	O	O	P	P		O	O	O	O	P							P	P	P			P			P		O
V	Ivan YATES																										P		
W	Frank MILES				P	P						P	P				P	O	P	P		P		o		P	P	O	O
X	Sir Austin PEARCE					O													P	P		O							
Y	(Kenneth CLARKE)				P																								
		A	B	C	D	E	F	G	H	I	J	K	L	M	N	O	P	Q	R	S	T	U	V	W	X	Y	*	+	#

LEGEND: (in descending order of precedence)

- P Person mentioned by name
- O Person's specific organization mentioned (i.e. more specific than just BAe, DTI or R-R)
- o Person mentions their own organization
- w People work directly with one another

BOLD letters indicate both people mention each other

- * This column refers to Sir Raymond Lygo, BAe Chief Executive, who was widely mentioned.
- + This column refers to the Blue Streak project, also widely mentioned
- # This column refers to the British Interplanetary Society, again widely mentioned

IV.III OTHER SMALL CASE STUDIES

The interviews with technologists were amongst the last to be conducted in this research, and they were analysed last. Their position in the time history of the research is therefore interesting - the people approached were chosen to complement both the HOTOL and the finance material. The following is a list of the four people who provided these 'vignettes':

INTERVIEWS WITH TECHNOLOGISTS AND INVENTORS

Date	Person	No.	Organization	Position
18- 6-90	Heinz FRICK	1	Ex BAe	Skyhook Originator
26- 6-90	Sir Clive SINCLAIR	2	Sinclair Research	
28- 6-90	Peter USHER	3	Vosper Thornycroft	Chairman
30-10-90	Dr. Louis NISBET	4	Xenova Ltd.	CEO

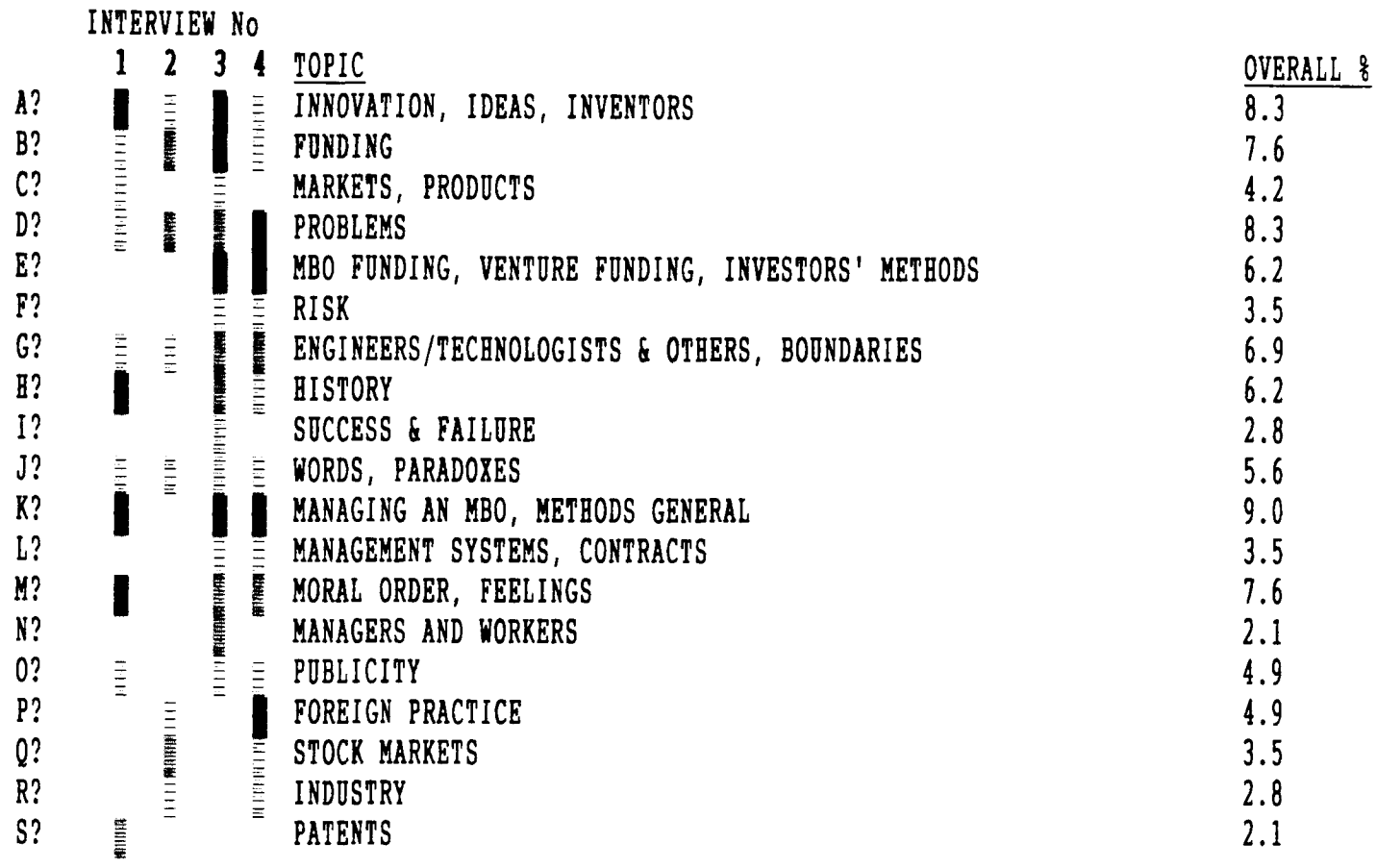
Each of these people can, fairly, be represented as the originator or developer of innovative ideas - in this they sit alongside Parsons and those involved in HOTOL. Indeed the choice of these people often reflected the advice and comment of others interviewed or contacted earlier in the research. The people themselves, and of course the researcher, sometimes mentioned these others, providing a satisfying link between the technology and finance material, and reflexive closure to the data as a whole.

This small set of four interviews raised the problem of whether to produce a new set of analysis categories, or to 're-use' one of the other sets. This is a relatively minor problem, but it is worth discussing because it emphasizes that analysis is not fixed, but very much a matter of researcher choice, although some choices are better than others, and some seductive solutions may even be methodologically inadmissable. The four interviews were diverse, they ranged from Clive Sinclair - a strong 'inventor stereotype' - through a failed attempt to innovate within BAe, to two examples of venture capital funding for technology - one of a new company, the other of an old one. The categories of discourse in these

interviews reflected those in all the other interviews, so re-use of their analysis was tempting. The HOTOL categories seemed the best fitted and their use was certainly considered, however, using them would have vitiated the methodological intention that categories be emergent and not externally imposed. Of course, in principle the list of categories remained 'open', so an extended set could have been allowed to develop, containing sub-sets applicable to each set of interviews (indeed categories did seem to be widely shared across interviews). However the most convenient course, and the one most consonant with the previous analyses, was to allow another new set of categories to emerge for the four technologists, and this was taken.

The more general point inherent in this - that if people share discourses, then emergent categories might also be shared - is very relevant to the discussion in chapter six. The appendix to that chapter takes up this discussion, and compares the similarities and differences in all the category sets to emerge - from Parsons, from HOTOL, from finance, and from the technologists. The following table and index presents the topics that did emerge from the interviews with technologists; they shows considerable consistency, despite their not inconsiderable differences of experience, outlook, and position:

TOPICS RAISED IN TECHNOLOGY INTERVIEWS & ROUGH QUALITATIVE ASSESSMENT OF DEGREE THEY WERE DISCUSSED



INDEX OF TOPICS TO EMERGE FROM ANALYSIS OF TECHNOLOGISTS INTERVIEWS

BOUNDARIES . . . . . G?
CONTRACTS . . . . . L?
ENGINEERS & OTHERS . . . . . G?
FAILURE . . . . . I?
FEELINGS . . . . . M?
FOREIGN PRACTICE . . . . . P?
FUNDING - MBO . . . . . E?
FUNDING . . . . . B?
FUNDING - VENTURE . . . . . E?
HISTORY . . . . . H?
IDEAS . . . . . A?
INDUSTRY . . . . . R?
INNOVATION . . . . . A?
INVENTORS . . . . . A?
INVESTORS' METHODS . . . . . E?
MANAGEMENT SYSTEMS . . . . . L?
MANAGERS AND WORKERS . . . . . N?
MANAGING AN MBO . . . . . K?
MARKETS, STOCK . . . . . Q?
MARKETS . . . . . C?
MBO - MANAGEMENT OF . . . . . K?
MBO FUNDING . . . . . E?
METHODS GENERAL . . . . . K?
METHODS - INVESTORS' . . . . . E?
MORAL ORDER . . . . . M?
PARADOXES . . . . . J?
PATENTS . . . . . S?
PROBLEMS . . . . . D?
PRODUCTS . . . . . C?
PUBLICITY . . . . . O?
RISK . . . . . F?
STOCK MARKETS . . . . . Q?
SUCCESS . . . . . I?
TECHNOLOGISTS & OTHERS . . . . . G?
VENTURE FUNDING . . . . . E?
WORDS . . . . . J?
WORKERS AND MANAGERS . . . . . N?

APPENDIX FOUR

The cards detailing the organizations and people mentioned by these four technologists were, of course, specific to each person. However, even here a few surprising similarities with other interviews were seen, and some of these are reflected in the ethnographic vignettes in the main body of the thesis.

APPENDIX V - THE PROVIDERS OF FINANCE

Interviews were conducted to investigate the way in which ideas are funded. The following list shows the people and organizations interviewed for this part of the research:

INTERVIEWS WITH THOSE INVOLVED IN FINANCE

(In order of interviewing; titles as at time of interview.)

Date	Person		Organization	Position
23- 1-90	Peter TERRY		Midland Montagu	Ex Director
23- 1-90	Tim HOLLAND-BOSWORTH		Kleinwort Benson	Corporate Finance Director
25- 1-90	John MORTON	*	BTG	Company Secretary
25- 1-90	N.STREET & J.DAWSON		Rothschild Ventures	
30- 1-90	Anthony BROOKE		Warburg Securities	
30- 1-90	Andrew BENNS		London Wall Securities	
14- 2-90	Gary S.WATSON	+	Hill Samuel	Director
26- 2-90	Dr. Richard SUMMERS		3i	Director
6- 3-90	Sir Peter CAREY		Morgan Grenfell	Chairman
8- 3-90	Chris BENJAMIN		DTI	Engineering Markets Division
8- 3-90	David GEMMILL		Chartered West LB	Deputy CE
12- 3-90	Humphrey BATTCOCK		Charterhouse Venture Funds	
12- 3-90	Roger LLEWELLYN		Mercury Asset Management	Vice Chairman
16- 3-90	Sue BUDD		County NatWest Ventures	
22- 3-90	Antony COSTLEY-WHITE		Oxford Seedcorn Capital	Director
19- 4-90	Don CARSE	NY	Bankers Trust	
19- 4-90	Carey CALLAGHAN	NY	Shearson Lehman Hutton	
20- 4-90	P.KRON & B.BARCLAY	NY	Citicorp	Project Finance
20- 4-90	Barbara LUNDBERG	NY	Kidder Peabody	
23- 4-90	Tom GITHENS	NY	Morgan Lewis Githens & Ahn	
23- 4-90	John CASSIS	NY	Salomon Brothers	
23- 4-90	Tom SMITH	NY	Chase Manhattan	
24- 4-90	Stuart FEINER	NY	Inco Venture Capital	President
24- 4-90	Richard MORGAN	NY	Wolfensohn Associates	
27- 4-90	Denis FILER		Engineering Council	Director General
30- 4-90	David LEATHERS		Abingworth Management Ltd.	
3- 5-90	G.ANDERSON & M.PRATT		Bank of England	
14- 5-90	Jeremy DOLPHIN		Deutsche Bank	Project Finance
11- 6-90	Keith MALLINSON		Korda & Co	
12- 6-90	Derek HARRIS		Birmingham Technology	Finance Director
13- 6-90	Lucius CARY		VCR / Seed Capital Ltd.	Founder
20- 6-90	Stephen JONES		Prelude Tech. Investments	Director
26- 6-90	Alex KORDA		Korda & Co	Joint MD
3- 7-90	David CHEESMAN		3i	High Technology Unit
3- 7-90	Dr. Peter ENGLANDER		Alan Patricof Associates	Director
5- 7-90	Roman KADRON		Citibank	VP Financial Institutions
22- 8-90	David COOKSEY		Advent	Chairman
19- 9-90	Charles BATCHELOR		Financial Times	Small Business Correspondent

* Second interview conducted 1-5-90.
\+ Telephone interview.
NY Interviews conducted in New York.

This appendix provides a brief description of these interviews in general, and of their analysis in particular. Chapter three describes the methodology used to guide conduct and analysis of each of these interviews. The present appendix supplements this methodological discussion, and presents the actual results obtained from the finance interviews. It also repeats and augments the comments in the appendix to chapter four about the emergent categories of the analysis.

The basic analysis procedure was to cut up copies of each interview and paste like comments onto cards, thereby building up a set of categories. These were subdivided into more factual material, and the softer and more speculative 'topics'. No attempt was made before analysis to guess what these topics might be, they emerged as analysis proceeded. The first interview analyzed was deliberately chosen to be one of the longest and most wide ranging, in order that most topics should emerge as early as possible, for convenience. In fact, only one topic was added after this first interview, however the topic titles were augmented to accommodate comments in later interviews where the emphasis was slightly different, but still covered by the broad sense of the topic.

The topics were each assigned an arbitrary letter code simply to assist reference. The list that results is therefore arbitrarily ordered, and reflects both the interpretive judgement of the researcher, and the concerns of the people who were interviewed. The substance of each topic also varies in subtle ways - they contain ethnographic data, symbols, stories, and suchlike, but they do not contain simple uncritical description. Any analysis that simply took people's words at 'face value' would grossly under-represent the richness of their words. The following list presents the topics that emerged, in the order in which they emerged; it also gives a taste of the rudimentary content analysis discussed later:

TOPICS TO EMERGE IN FINANCE INTERVIEWS & ROUGH QUALITATIVE ASSESSMENT OF DEGREE THEY WERE DISCUSSED

	TOPIC	UK VCs %	NY VCs %	UK Banks %	NY Banks %	I/mediaries %	OVER ALL %
A	PERSONAL BACKGROUND	1.6	1.7	1.9	2.7	1.9	1.8
B	ACCOUNTANTS, ACCOUNTANCY PROCEDURES	0.9	1.7	1.1	0.9	2.5	1.1
C	FINANCE GENERAL, BANKS, FINANCIERS, V.CAPITAL	4.9	6.9	5.6	8.4	5.0	5.7
D	COMPARISONS OF UK WITH USA	4.7	7.8	3.7	5.3	1.9	4.6
E	ENGINEERS	0.9	0.0	0.4	0.0	3.8	0.9
F	FINANCIAL STRUCTURE, CONTRACTS	1.6	1.7	4.5	4.4	1.3	2.5
G	ENTREPRENEURS, FOUNDERS	3.2	4.3	3.4	4.4	0.0	3.2
H	ACADEMICS, ACADEMIC INSTITUTIONS	2.6	0.0	1.5	1.3	1.3	1.9
I	MARKETS	2.7	2.6	1.1	0.4	1.9	2.0
J	FAILURE, SUCCESS	2.7	4.3	0.7	1.3	0.0	2.0
K	INVESTMENT, AMOUNTS INVESTED	3.0	0.9	2.2	0.4	1.9	2.2
L	IDEAS, INNOVATION	3.3	1.7	4.9	4.0	6.9	3.9
M	RISK	2.0	0.0	3.7	2.2	1.3	2.1
N	WORDS, EPIGRAMS	2.6	2.6	1.9	3.1	1.9	2.5
O	MANAGEMENT	4.2	3.4	3.4	1.8	3.1	3.5
P	RETURNS	0.9	3.4	0.0	1.8	0.0	0.9
Q	PRODUCTS, TECHNOLOGY, INTELLECTUAL PROPERTY	3.7	3.4	4.1	4.4	2.5	3.7
R	METHODS, POLICIES	7.4	9.5	8.6	8.0	6.9	7.8
S	FUNDS, FINANCIAL INSTITUTIONS	3.1	5.2	1.5	3.1	1.3	2.8
T	PURPOSE, MORAL ORDER	3.0	0.0	1.1	1.8	4.4	2.4
U	TIMESCALE, SHORT-TERMISM	2.1	2.6	1.9	1.3	4.4	2.2
V	COMPANIES, INDUSTRY	3.8	5.2	5.6	4.4	6.9	4.6
W	EXIT ROUTES	2.7	3.4	0.0	0.9	0.6	1.8
X	STARTUP	3.9	6.9	2.6	3.6	3.1	3.8
Y	INVENTORS	2.6	3.4	4.9	1.8	1.3	2.8
Z	PUBLICITY, MEDIA, PRESS	2.6	0.0	1.9	0.0	5.0	2.2
AA	PATENTS, LICENSING, PROTECTION	1.2	0.0	1.1	0.0	0.0	0.8
BB	JAPAN	1.8	0.0	1.9	3.6	5.7	2.3
CC	GOVERNMENT, GOVERNMENT FUNDING	3.3	0.0	4.1	3.1	7.5	3.6
DD	SEEDCORN	4.3	2.6	1.9	2.7	2.5	3.4
EE	EUROPE	3.0	0.9	4.1	4.0	5.7	3.4
FF	ANGELS, HIGH NETT WORTH INDIVIDUALS	2.2	0.9	1.5	1.8	1.3	1.8
GG	INTERMEDIARIES, CONTACTS, NETWORKS	3.2	6.0	3.4	5.3	3.1	3.7
HH	STOCK MARKETS	1.1	3.4	1.9	0.9	0.0	1.3
II	MANAGEMENT BUYOUTS, TAKEOVERS	1.8	3.4	1.9	1.3	1.9	1.9
JJ	DEVELOPMENT (FUNDING)	1.1	0.0	2.2	1.3	0.0	1.1
KK	PROJECTS	0.4	0.0	4.1	4.0	1.3	1.6

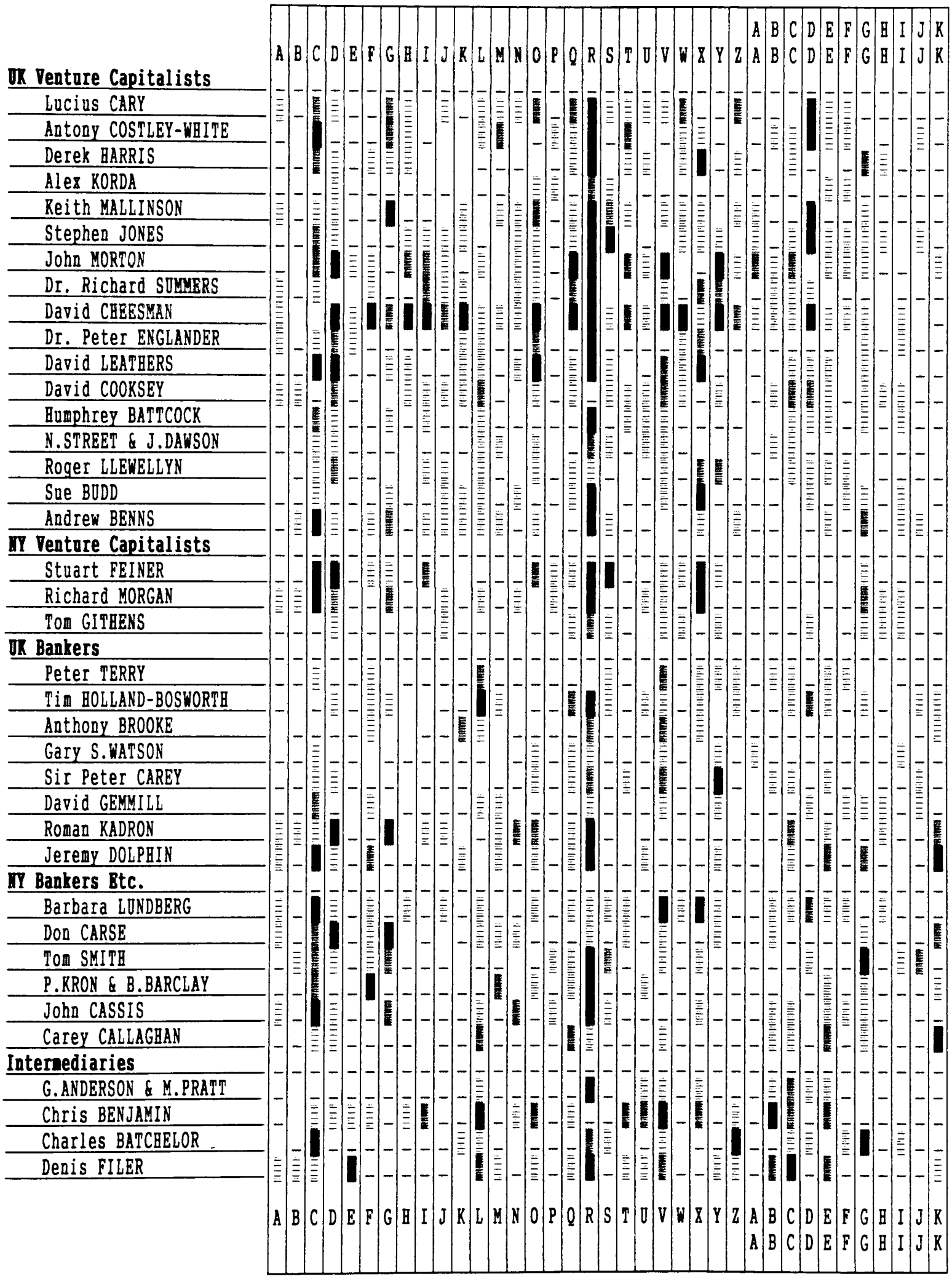
The percentage figures given for each category are extremely rough estimates of the degree of discussion each received - they will be considered in a little more detail below. First, for ease of reference, the following list is presented - it shows all of the category titles or descriptors in alphabetical order:

INDEX OF TOPICS TO EMERGE FROM ANALYSIS OF FINANCE INTERVIEWS

ACADEMIC INSTITUTIONS . . . . . H
ACADEMICS . . . . . H
ACCOUNTANCY PROCEDURES . . . . B
ACCOUNTANTS . . . . . B
AMERICA (COMPARISON WITH UK) . D
AMOUNTS INVESTED . . . . . K
ANGELS . . . . . FF
BANKS C
BUYOUTS II
COMPANIES V
CONTACTS . . . . . GG
CONTRACTS . . . . . F
DEVELOPMENT (FUNDING) . . . . . JJ
ENGINEERS . . . . . E
ENTREPRENEURS . . . . . G
EPIGRAMS . . . . . N
EUROPE . . . . . EE
EXIT ROUTES . . . . . W
FAILURE . . . . . J
FINANCE GENERAL C
FINANCIAL STRUCTURE . . . . . F
FINANCIERS C
FOUNDERS . . . . . G
FUNDING (FROM GOVERNMENT) . . . CC
FUNDS (FINANCIAL) . . . . . S
GOVERNMENT CC
GOVERNMENT FUNDING CC
IDEAS L
INDIVIDUALS-(HIGH NETT WORTH) . FF
INDUSTRY V
INNOVATION L
INSTITUTIONS (FINANCIAL) . . . S
INTELLECTUAL PROPERTY . . . . . Q
INTERMEDIARIES . . . . . GG
INVENTORS . . . . . Y
INVESTMENT . . . . . K
JAPAN . . . . . BB
LICENSING . . . . . AA
MANAGEMENT . . . . . O
MANAGEMENT BUYOUTS II
MARKETS I
MEDIA . . . . . Z
METHODS . . . . . R
MORAL ORDER . . . . . T
NETWORKS . . . . . GG
PATENTS . . . . . AA
PERSONAL BACKGROUND . . . . . A
POLICIES . . . . . R
PRESS . . . . . Z
PRODUCTS . . . . . Q
PROJECTS . . . . . KK
PROTECTION . . . . . AA
PUBLICITY . . . . . Z
PURPOSE . . . . . T
RETURNS . . . . . P
RISK . . . . . M
SEEDCORN . . . . . DD
SHORT-TERMISM . . . . . U
STARTUP X
STOCK MARKETS . . . . . HH
STRUCTURE (FINANCIAL) . . . . . F
SUCCESS . . . . . J
TAKEOVERS II
TECHNOLOGY . . . . . Q
TIMESCALE . . . . . U
USA (COMPARISON WITH UK) . . . . D
VENTURE CAPITAL C
WORDS . . . . . N

The following table shows whom of those interviewed referred to each topic, and the degree to which they discussed it. The letters refer to the categories listed above:

TOPICS RAISED BY THOSE INTERVIEWED INVOLVED IN FINANCE



In the table the interviews are grouped into venture capitalists, bankers, and intermediaries - a somewhat arbitrary subdivision, but one found useful for the discussion of this thesis. The table shows a great diversity in the interviews, and demands further comment regarding their analysis, lest too much quantitative weight (and too little qualitative understanding) be placed upon these intermediate 'results'.

First of all, the topics are emergent, but not neutral. They reflect what the person interviewed said, but an interview is a dialogue, and the interviewer also influences it. Previous interviews, literature surveyed, and popular hearsay, all raise questions in the researcher's mind that guide the conduct of the interview - sometimes consciously, sometimes not. In general, as little prompting as possible was used in interviews, but specific questions in a few areas (such as perceptions of differences in other countries) were asked. This is necessary, not to force discussion, but rather to limit it - after all, this thesis seeks understanding of people's views on the support of new technological ideas, not of their views on life in general. This degree of bracketing on the part of interviewer (and interviewee - people only readily discussed what interested them) imposes a degree of consistency on the material before analysis, without denying the idiosyncratic or strange a place either.

The second point to bear in mind is that this research is not based upon a single interview, far from it. Even the initial choice of people to interview was deliberately broad, and thereafter it was the advice and comment in the interviews themselves that directed subsequent selection. Some of the resultant assemblage of people had direct involvement in financing ideas, some did not, but almost all of them had interest, knowledge, expertise, and views, relevant to the area. Again, the eclectic nature of the interpretive methods used adds robustness to the results.

A third and final point illustrates this robustness. The above observations could be summarized by saying that, once a research area is chosen, qualitative research is bounded in a very active way. If the researcher succeeds in that active process, then the texture of the results might be expected to have a certain regularity - the emergent topics forming a sort of regular and bounded, yet still diverse, set. It does seem that this is the case with the interviews discussed here. The table above shows regularity in that a wide range of people discussed almost all of the topics, and in the fact that most people raised many different topics; yet differences are also present. The analysis therefore appears to have been successful, more successful perhaps than that of the interviews with technologists. This reflects, in some measure, the fact that the emergence of topics involves a degree of conscious choice on the part of the researcher - as it should, since the researcher is part of the process. In the present research the experience of analysing the HOTOL data undoubtedly aided the approach to analysing the finance data.

The result is a visual presentation of what seemed important, with regard to financing ideas, to people inside the financial community itself. Such things cannot be quantified - the data records people's descriptions, feelings, and opinions, and to quantify these would have appeared absurd to them. But this is not to say that numbers are not useful as ways of visualizing the relative importance of various things to people, so long as they are used in a suitably impressionistic way. This is what has been done in the percentages ascribed to each topic in the list of categories presented above. The percentages were calculated by simply ascribing rankings from one to four to the degree of discussion in each interview shown in the table. This has two benefits - firstly it allows aggregation into groups of the topics discussed by venture capitalists, bankers and intermediaries, on either side of the Atlantic. Secondly, it

tends to smooth out the fact that some interviews were longer than others - no topic could rank more than four, regardless of how many cards the discourse of an interview filled, yet even a passing mention in a short interview would still be accounted for.

The emergent topics were a key device used in writing this thesis, but they are only useful in so far as they help interpretation. There is no right or wrong way to analyze the words, nor is it only the words - the written record of spoken interviews - that are important. Other analyses, and other aspects could, equally, be considered. A final example will serve to show that no one form of analysis should be privileged. The interactive interviews aimed to seek out the wider community that shared the discourses they revealed. It is the people, and the way they saw and interacted with one another, that are important. Thus an interesting sidelight comes from an analysis of peoples' references to each other, and to each other's organizations - the more factual emergent categories. The final table of this appendix to chapter five shows the network of contacts that this hints at (note the letters refer to the people cross-referenced this time, not to the categories discussed above):

LINKS BETWEEN THOSE INTERVIEWED INVOLVED IN FINANCE

	A	B	C	D	E	F	G	H	I	J	K	L	M	N	O	P	Q	R	S	T	U	V	W	X	Y	Z	AA	BB	CC	DD	EE	FF	GG	HH	II	JJ	KK	LL
UK Venture Capitalists																																						
A Lucius CARY		**O**		O	O	**O**		O	O	O																												
B Antony COSTLEY-WHITE	**P**							O	O																													
C Derek HARRIS										O																												
D Alex KORDA		O																**O**																		P	
E Keith MALLINSON						O												**J**																				
F Stephen JONES	**P**			O	O			O	O																													
G John MORTON							o																															
H Dr. Richard SUMMERS								o	o																											O		
I David CHEESMAN								o	o																													
J Dr. Peter ENGLANDER																																						
K David LEATHERS																																						
L David COOKSEY																																						
M Humphrey BATTCOCK							O																															
N N.STREET & J.DAWSON							O																															
O Roger LLEWELLYN							O	P	O																													
P Sue BUDD		O						O	O	O			O			o																						
Q Andrew BENNS							O	O	O							O																							
NY Venture Capitalists																																						
R Stuart FEINER				**J**	**J**																																	
S Richard MORGAN																																						
T Tom GITHENS														O																								
UK Bankers																																						
U Peter TERRY							O	O	O																													
V Tim HOLLAND-BOSWORTH							O							O																								
W Anthony BROOKE								O	O					O	O								o															
X Gary S.WATSON																																						
Y Sir Peter CAREY							O	O	O			O		O																								
Z David GEMMILL																																						
AA Roman KADRON																											o											
BB Jeremy DOLPHIN							O																															
NY Bankers Etc.																																						
CC Barbara LUNDBERG										O																	O					O						
DD Don CARSE																						O																
EE Tom SMITH								O	O																													
FF P.KRON & B.BARCLAY																																						
GG John CASSIS																																						
HH Carey CALLAGHAN																																						
Intermediaries																																						
II G.ANDERSON & M.PRATT																																						
JJ Chris BENJAMIN								O	O																													
KK Charles BATCHELOR	P																																					
LL Denis FILER																																						
	A	B	C	D	E	F	G	H	I	J	K	L	M	N	O	P	Q	R	S	T	U	V	W	X	Y	Z	AA	BB	CC	DD	EE	FF	GG	HH	II	JJ	KK	LL

P Person mentioned by name
O Person's organization mentioned
o Person mentions their own organization
J Jointly funded deal mentioned
BOLD letters indicate both people mention each other

As has been said, the topics and their further analysis, were a key used to unlock the interpretation that this thesis represents. Other analyses could, no doubt, unlock other doors. In this, it is vital to remember that in any interpretation it is not the topic headings that are important, but the words from the interviews that are written beneath each heading. The bulk of this thesis considers these words; this appendix has merely tried to show that disciplined and structured analysis was needed to allow the underlying texture of the discourse to emerge, or - rather - to re-emerge, in the main body of the text.

This re-emergence flowed directly from the words on the cards. In writing the thesis certain section headings, certain ways of organizing, were chosen - these too seemed to emerge, both from the interviews experienced and from the literature read by the researcher. The topics then seemed to fit more or less into one section (or sections) better than into others. Thus the cards for each relevant topic were re-read, and often quoted, in the process of writing the sections. In practice, this meant working on a number of sections at once, the fabric of all of them slowly hanging itself around a growing framework of quoted discourse, some of it falling away again as time went on. Chapter five with its ethnography of finance is, then, a dynamic creation - the final version is final only because a selection of keystrokes told a printer to operate at a particular time, and a photo-copier then prepared a set of copies for the reassuringly timeless calm of an Oxford bookbinder's shop. The thesis is thus arrested in time in the process of becoming another thesis that still could have been an interpretation of the world of financing ideas, of the process of innovation. For every reader, should there be any, it will become something different. As such, it is itself a metaphor of the very process of innovation. It is right that it should be so.

APPENDIX VI - FOUR DISCOURSES

Chapter six shows that what people said in the interviews clearly suggested a number of different world views. These were fitted into four distinct classes of discourse, designated: moral, industrial, financial and public. Each of these seemed distinctive in its own way, and putting them together allowed a subtle framework within which to consider innovation. It proved useful to think in terms of these four discourses - four orders of meaning.

The interviews suggested these orders of discourse, and there was an abundance of examples from their texts to provide empirical qualitative support for them. Nonetheless, it would be perverse to pretend that the interviews, already carefully analysed into constituent categories (as described in the appendices to previous chapters), had suddenly spawned some new epistemological 'meta-categories' called discourses. This was not the case - the idea of four discourses arose during, and from, the earlier analysis itself. However, two corollaries are inescapable if the freely emergent topics (topics that differed between the four sets of interviews that were analysed), when taken together, suggested only four discourses. Firstly, these different sets of topics must be linked in some way, and secondly they ought, themselves, to be capable of being grouped under the four discursive headings.

This appendix describes the way in which both of these corollaries were demonstrated. This was done as a check upon the robustness of the four-discourse model that later chapters will use to investigate innovation. In practice, little difficulty was found in aligning the categories from one set of interviews with congruent categories from the others. This is hardly surprising - the previous appendices have described the very considerable learning process that linked them, leading on from one analysis set, on to the next. In

addition, many of the categories from different sets had even been given similar, or identical, names.

Of the three types of category used in the analysis, topics alone were compared here, since the organizations and individuals named were clearly likely to be specific to each set of interviews (although some very interesting cross-references amongst these categories are considered in the appendix to the next chapter). A comparative table of topics across the sets of interviews was compiled. This proved quite easy - the numbers of topic for each group of interviews were different, so it involved aggregating individual topics in the larger sets (particularly finance) to match the broader topic categories in the other sets. Unsurprisingly, the set requiring the least aggregation was the last to be analysed - that from the technology interviews. The following table presents the resulting cross-comparisons; for convenience each new aggregation of topics is headed with a new topic-group name:

COMPARISON OF TOPICS FROM EACH SET OF INTERVIEWS

TECHNOLOGY	FINANCE	HOTOL	PARSONS & FISHER
-ideas-			**-ideas-**
A? INNOVATION, IDEAS, INVENTORS	G? ENTREPRENEURS, FOUNDERS L? IDEAS, INNOVATION Y? INVENTORS	I? CREATIVITY, INNOVATION	P? INVENTORS, INVENTION
-finance-			**-finance-**
B? FUNDING I? SUCCESS & FAILURE	C? FINANCE GENERAL, BANKS, FINANCIERS, V.CAPITAL J? FAILURE, SUCCESS K? INVESTMENT, AMOUNTS INVESTED P? RETURNS S? FUNDS, FINANCIAL INSTITUTIONS FF? ANGELS, HIGH NETT WORTH INDIVIDUALS	G? FUNDING	F? FINANCE, RETURNS
-product-markets-			**-product-markets-**
C? MARKETS, PRODUCTS	I? MARKETS		D? MARKETS, MARKETING, SALES H? FASHION
-problems-			**-problems-**
D? PROBLEMS	U? TIMESCALE, SHORT-TERMISM	O? BASIC PROBLEMS	E? LOCAL PROBLEMS Q? OVERALL PROBLEM
-methods-			**-methods-**
E? MBO FUNDING, VENTURE FUNDING, INVESTORS' METHODS K? MANAGING AN MBO, METHODS GENERAL	R? METHODS, POLICIES X? STARTUP DD? SEEDCORN II? MANAGEMENT BUYOUTS, TAKEOVERS JJ? DEVELOPMENT (FUNDING)	E? WAY THEY WORK, SORT OF PEOPLE THEY ARE	K? METHODS
-risk-			**-risk-**
F? RISK	M? RISK		G? RISK
-professions-			**-professions-**
G? ENGINEERS / TECHNOLOGISTS & OTHERS, BOUNDARIES	E? ENGINEERS H? ACADEMICS, ACADEMIC INSTITUTIONS GG? INTERMEDIARIES, CONTACTS, NETWORKS	L? SPECIALIZATION, PROFESSIONS	
-history-			**-history-**
H? HISTORY		F? HISTORY, CONTROLLABLE EVENTS D? PROJECT TURNING POINTS, UNEXPECTED EVENTS	B? INNOVATION HISTORY

TECHNOLOGY	FINANCE	HOTOL	PARSONS & FISHER
-odd-words			**-odd-words-**
J? WORDS, PARADOXES	N? WORDS, EPIGRAMS	M? WORDS H? SIZE AND SCALE	N? WRITING, WORDS, EPIGRAMS
-management-systems			**-management-systems-**
L? MANAGEMENT SYSTEMS, CONTRACTS	B? ACCOUNTANTS, ACCOUNTANCY PROCEDURES F? FINANCIAL STRUCTURE, CONTRACTS O? MANAGEMENT	C? MANAGEMENT SYSTEMS, POLICY	J? ORGANIZATION, BUSINESS
-feelings			**-feelings-**
M? MORAL ORDER, FEELINGS	A? PERSONAL BACKGROUND T? PURPOSE, MORAL ORDER	B? PERSONAL TURNING POINTS, FEELINGS A? MOTIVATION	A? PERSONAL HISTORY R? PERSONAL METHODS S? JOB OFFERS
-management-of-people			**-management-of-people-**
N? MANAGERS AND WORKERS			L? LABOUR, PEOPLE
-publicity			**-publicity-**
O? PUBLICITY	Z? PUBLICITY, MEDIA, PRESS	J? NATIONAL PUBLICITY	M? PUBLICITY
-foreign-comparisons			**-foreign-comparisons-**
P? FOREIGN PRACTICE	D? COMPARISONS OF UK WITH USA BB? JAPAN EE? EUROPE	Q? FOREIGN INTEREST, FOREIGN PRACTICE	T? FOREIGN COMPETITION
-stock-markets			**-stock-markets-**
Q? STOCK MARKETS	W? EXIT ROUTES HH? STOCK MARKETS		
-industry			**-industry-**
R? INDUSTRY	V? COMPANIES, INDUSTRY KK? PROJECTS		
-'intellectual-property'			**-'intellectual-property'-**
S? PATENTS	Q? PRODUCTS, TECHNOLOGY, INTELLECTUAL PROPERTY AA? PATENTS, LICENSING, PROTECTION	K? EXPERTISE, EXPERIENCE, KNOWLEDGE	C? PATENTS, LICENSING
-visions			**-visions-**
		N? WHAT THE PROJECT IS P? FUTURE	I? AIMS
-government			**-government-**
A GOVERNMENT*	CC? GOVERNMENT, GOVERNMENT FUNDING	R? POLITICS	O? GOVERNMENT INVOLVEMENT

* Note: Government was considered as an 'organization' category, not a '?topic?' in the technology interviews - so concrete and obvious an entity no longer seemed to justify the speculative treatment a '?topic' implies - it is included here, and in future references to this table, for completeness.

These new topic-groups suggest few real problems - certainly there are a few gaps, but even these are largely explicable. For example, it is hardly surprising that the financiers did not mention '?history', since they were the only group without a case-study to describe the history of; Parsons did not mention '?stock markets' because his companies were private, not public, companies. Different, and maybe more condensed, aggregations could no doubt have been suggested, but, as with all the analysis presented in this thesis, what is suggested here reflects the researcher's interpretation of his own data - too much should not be read into mere topic names. What is important is that the topic-groups link the different sets of people - <u>what was salient for one group seemed salient for all</u>.

The next consideration is whether these shared topic-groups can neatly be subsumed by the four discourses. Perusal of them confirms that indeed they can, given two rather obvious qualifications. Firstly, it is not reasonable to include either the '?methods' or '?history' topic-groups here - they describe the methods used within, or the progress of an innovation through, any of the discourses - as categories they were not specific to just one discourse. The second qualification is that some of the topic-groups are associated not with one particular discourse, but with the boundaries between them.

Given these observations, the following table ascribes to each discourse in turn, the topic-groups that best seem to fit it. The topic-groups that describe boundaries are also listed, with a suggestion of the specific boundaries to which they are most relevant:

APPENDIX SIX

TOPIC GROUPS RELEVANT TO EACH DISCOURSE

MORAL DISCOURSE:
- ?Ideas
- ?Feelings
- ?Visions

INDUSTRIAL DISCOURSE:
- ?Management Systems
- ?Management of People
- ?Industry

FINANCIAL DISCOURSE:
- ?Finance
- ?Stock Markets

PUBLIC DISCOURSE:
- ?Product Markets
- ?Foreign Comparisons
- ?Government

BOUNDARIES:	M-I	I-F	M-F	M-P	I-P	F-P
?Problems	*	*	*	*	*	*
?Odd Words	*	*	*	*	*	*
?Risk	*	*	*			
?Professions	*	*	*			
?Intellectual Prop.	*	*	*			
?Publicity			*	*	*	

This appendix has illustrated the way in which the topic categories resulting from analysis of the interview data support the utility of a four-discourse model, just as the contents of the interviews - of the topics - are seen to support it in the main body of chapter six. Whilst they may or may not share each others' discourses, the people involved in innovation today, and those so involved one hundred years ago, all very clearly share knowledge of all those different discourses.

APPENDIX VII - CROSSING BOUNDARIES

This appendix looks at what can be gleaned from the interviews to show the inter-relationships between people and between their organizations. The evidence is suggestive rather than concrete, but what it suggests is of great interest to chapter seven's description of the way innovation happens. There are two ways of looking at the data - who talked about whom, and where their organizations were situated in relation to others; this appendix looks at each in turn.

VII.I INTERPERSONAL NETWORKS

The appendix to chapter six brought together the topics - one of the three categories that grew out of the interview analysis; this appendix brings together the other two categories - the people and the organizations that were talked about. Spotting these two categories in the text of the interviews is straightforward, and, although their simple occurrence tells little of what people thought about them, a careful sifting of these two categories is still a productive exercise, especially when a large sample of interviews is available as was the case with the HOTOL and finance data.

The following table re-orders the data already presented in appendix four on the HOTOL interviews. As will be seen they have been broadly re-grouped into the two companies, government, other individuals, and originators:

LINKS BETWEEN THOSE INTERVIEWED INVOLVED IN HOTOL

		ORIGIN -ATORS			BRITISH AEROSPACE												GOVERN -MENT					ROLLS -ROYCE						
		D	K	E	U	T	A	B	C	G	H	I	J	V	X	*	L	R	O	Y	Q	P	M	N	S	W	F
ORIGINATORS																											
D	Alan BOND (both ints.)	o	**P**	**P**		0	P	0	0	0	0	0	0	P		P	0	P	0	P			P		P	P	**O**
K	John SCOTT-SCOTT	**P**		P	P	P										P		P		P		P	P		P	P	
E	Dr. R.C. PARKINSON	**P**	P	o	P	**O**	P	**O**	0	0	0	0	0	**O**		P		P		P		P					
BRITISH AEROSPACE																											
U	Peter CONCHIE	P	P	P		P	**O**	**O**	0	0	0	0	0	0		P		P							P	P	
T	Clive LEYMAN		P	P	**O**	o	**O**	0	0	0	0	0	0	P		P					0						
A	B.R.A. BURNS			P	**O**	**O**	o	w	w	w	w	w	w				P		0	P							
B	Alison WAKE			**O**	**O**		w		w	w	w	w	w														
C	Steve FURNISS						w	w		w	w	w	w														
G	Sid WALMSLEY						P	w	w		w	w	w	P													
H	Nick DALE						P	w	w	w		w	w														
I	Steve GILKES						0	0	0	0	0	o	0														
J	Alec DAVIES			**O**	**O**		w	w	w	w	w	w															
V	Ivan YATES															P											
X	Sir Austin PEARCE			O	0													P							P		
*	(Sir Raymond LYGO)																										
GOVERNMENT																											
L	A.C.NICHOLAS / A.HICKS					**O**	0	0	0	0	0	0	0				o		**O**		0						
R	Sir Geoffrey PATTIE	P														P			P	P	O				P		
O	Roy GIBSON																**O**	P	o	P	O				P		
Y	(Kenneth CLARKE)	**P**																									
Q	Sir Richard NORMAN																				o						
ROLLS-ROYCE																											
P	Gordon LEWIS	P	**P**	**O**	0											P	0	P	P		P		**P**		P	**P**	
M	Dr. Brian LOWRIE	**P**	**P**	P													0		P			**P**		P	P		
N	Dr. Tony HEWWIT	P		P				P															w		P		
S	Sir Francis TOMBS	**P**																									
JOURNALISTS																											
W	Frank MILES	**P**	**P**	P	**P**											P	P	P		P	0	**P**			P	o	
OTHERS																											
F	Ken LAVENDER	**P**																									o
		D	K	E	U	T	A	B	C	G	H	I	J	V	X	*	L	R	O	Y	Q	P	M	N	S	W	F

LEGEND: (in descending order of precedence)

- P Person mentioned by name
- O Person's specific organization mentioned (i.e. more specific than just BAe, DTI or R-R)
- o Person mentions their own organization
- w People work directly with one another

BOLD letters indicate both people mention each other

* This column refers to Sir Raymond Lygo, BAe Chief Executive, who was widely mentioned.

This data shows some obvious things, and some that are not so obvious. It is obvious that most of the references each group makes are to themselves, it is also obvious that there is a scattering of references to other groups. Various points arise from the general character of these references. BAe made little reference to Rolls-Royce, and vice versa; Government made little reference to either. Of these, BAe can be looked at in a little more detail - there were more interviews in this company than elsewhere, and they covered four different company sites. They show that the Warton site, where the people interviewed were mostly involved in the day-to-day realization of HOTOL, made the least outside reference. The other BAe people - the 'link-men' at Stevenage (Conchie), Filton (Leyman), and also at Warton (Burns) made some reference (and that largely personal) outside their sites, as did those at Head Office (Yates and Pearce).

It would be easy to read too much into all this, but it is fair to say that it mostly shows the general character that might be expected of a complicated project in modern industrial organizations. What the table adds, however, is the rôle of the originators - seen in some of the interviews as an unorthodox element. These three men referred to almost everyone else, generally in person, and were often referred to by them in turn. The same can be said of the journalist who 'blew the story'. This is very supportive of the view of how innovation happens advanced in the main body of this thesis.

The next table presents the finance interviews in the same form as that used for HOTOL. It is very similar to the table already included in appendix five, but it has been opened up a little for clarity, and the second interview with Alan Bond - which largely discussed finance - has been added:

LINKS BETWEEN THOSE INTERVIEWED INVOLVED IN FINANCE

	A	B	C	D	E	F	G	H	I	J	K	L	M	N	O	P	Q	R	S	T	U	V	W	X	Y	Z	AA	AB	CC	DD	EE	FF	GG	HH	II	JJ	KK	LL
UK SEED CAPITALISTS																																						
A Lucius CARY		**O**		O	O	**O**		O	O	O																												
B Antony COSTLEY-WHITE	P							O	O																													
C Derek HARRIS										O																												
D Alex KORDA		O																**O**																		P		
E Keith MALLINSON						O												J																				
F Stephen JONES	P			O	O			O	O																													
UK VENTURE CAPITALISTS																																						
G John MORTON							o																															
H Dr. Richard SUMMERS								o	o																										O			
I David CHEESMAN								o	o																													
J Dr. Peter ENGLANDER																																						
K David LEATHERS																																						
L David COOKSEY																																						
M Humphrey BATTCOCK							O																															
N N.STREET & J.DAWSON							O																															
O Roger LLEWELLYN							O	P	O																													
P Sue BUDD		O						O	O	O			O			o																						
Q Andrew BENNS							O	O	O							O																						
NY VENTURE CAPITALISTS																																						
R Stuart FEINER				J	J																																
S Richard MORGAN																																						
T Tom GITHENS														O																								
UK BANKERS																																						
U Peter TERRY							O	O	O																													
V Tim HOLLAND-BOSWORTH							O							O																								
W Anthony BROOKE								O	O					O	O							o																
X Gary S.WATSON																																						
Y Sir Peter CAREY							O	O	O				O		O																								
Z David GEMMILL																																						
AA Roman KADRON																										o												
BB Jeremy DOLPHIN							O																															
NY BANKERS Etc.																																						
CC Barbara LUNDBERG									O																	O								O				
DD Don CARSE																					O																	
EE Tom SMITH								O	O																													
FF P.KRON & B.BARCLAY																																						
GG John CASSIS																																						
HH Carey CALLAGHAN																																						
INTERMEDIARIES																																						
II G.ANDERSON & M.PRATT																																						
JJ Chris BENJAMIN								O	O																													
KK Charles BATCHELOR	P																																					
LL Denis FILER																																						
HOTOL																																						
Alan BOND (2nd Int.)							O	O	O				O	O			O					O																

	A	B	C	D	E	F	G	H	I	J	K	L	M	N	O	P	Q	R	S	T	U	V	W	X	Y	Z	AA	AB	CC	DD	EE	FF	GG	HH	II	JJ	KK	LL

P Person mentioned by name
O Person's organization mentioned
J Jointly funded deal mentioned
o Person mentions their own organization
BOLD letters indicate both people mention each other

The groupings here are a little arbitrary, but as chapter five shows they do reflect something of the way people seemed to classify different areas of finance. The next section of this appendix suggests a little further sub-division, but - given the sparsity of the data - the broader divisions are more appropriate here. It is immediately observable that there are far fewer references to others in the finance interviews than with HOTOL, although in part this undoubtedly reflects the nature of the interviews - they were not, after all, telling a story.

However, two interesting points do come out in the table. The first is that outside of venture capital, few people even referred to their own group. This perhaps reflects the gregarious, syndicated, nature of much of venture finance, although this is also true of banks, so the observation may be of no consequence. The second observation is more suggestive - every group seemed to mention venture capital. In a set of interviews where the intention was to investigate innovation, this does seem to hint that the finance community sees innovation as the province primarily of venture capital - a result of some interest to the discussion of this thesis.

VII.II GEOGRAPHICAL NETWORKS

The observation that 'The City' is a synonym for the British finance industry is significant for research that attempts to be sensitive to the symbolic nature of day-to-day life. Indeed, Giddens, reviewed in chapter two, looked at the City as one of his examples (Giddens 1984:319-327). Chapter five touched upon the history of some City financial institutions, and showed something of the informal character of their origins. These origins were in trade, and there seems little reason to doubt the accuracy of the conventional picture of today's financial service industry growing out of the activities of the 'business angels' of past centuries who frequented the old wharves of a major sea-port like London.

Such people dealt informally with those they funded, and therefore their geographical propinquity was essential.

The symbolism attached to 'The City' today is thus far from arbitrary - it is historically based. However, the interviews in the City hinted that close personal contact between those in financial institutions was still a characteristic feature today. Such an observation is, of course, consistent with the model developed in this thesis, of people interacting within and across different language communities. This leads to the intriguing question - if the geographical logic that applied to the financial denizens of the seventeenth century coffee houses applies to their successors in the City today, then this should still be evident in the way they cluster together - modern telecommunications notwithstanding. In other words: is the synonym 'The City' symbolic at all, or is it purely descriptive.

This is a fairly obvious question, and the very obvious concentration of finance within a small area of London seems to answer it. However, symbolism often <u>seems</u> obvious - that is how it works - and a scholarly approach in this area has to be wary of such things. This section of the appendix, therefore, tests this by looking at the institutions where interviews were conducted. The following three tables include their addresses; the subdivision is a little finer than in earlier tables, in order to investigate the full texture of the data:

(A) - VENTURE CAPITALISTS INTERVIEWED

UK VENTURE CAPITALISTS	
Independent Seedcorn	
Venture Capital Report / Seed Capital Ltd	Henley
Oxford Seedcorn Capital Limited	Oxford
Birmingham Technology (Venture Capital) Ltd.	Birmingham
Korda & Company Limited [Two interviews conducted]	Farringdon Lane
Prelude Technology Investments Limited	Cambridge
Technology Transfer	
British Technology Group	Elephant & Castle
Independent Later Stage	
3i plc	Waterloo Road
3i High Technology Unit	Waterloo Road
Alan Patricof Associates Limited	Upper Brook Street
Abingworth Management Ltd.	St. James Street
Advent Ltd.	Buckingham Gate
Funds With Links to Merchant and Other Banks	
Charterhouse Venture Fund	Hertford Street
Rothschild Ventures Limited	New Court
Mercury Asset Management	King William Street
County NatWest Ventures Limited	Bishopsgate
London Wall Investments	Copthall Avenue
NY VENTURE CAPITALISTS	
Corporate	
Inco Venture Capital	One New York Plaza
Independent	
Wolfensohn Associates	599 Lexington Avenue
Morgan Lewis Githens & Ahn	767 Fifth Avenue

(B) - FINANCIAL INTERMEDIARIES ETC. INTERVIEWED

INTERMEDIARIES	
Bank of England - Industrial Finance Division	Bank
DTI - Engineering Markets Division	Victoria Street
Financial Times	Southwark Bridge
Engineering Council	Maltravers Street

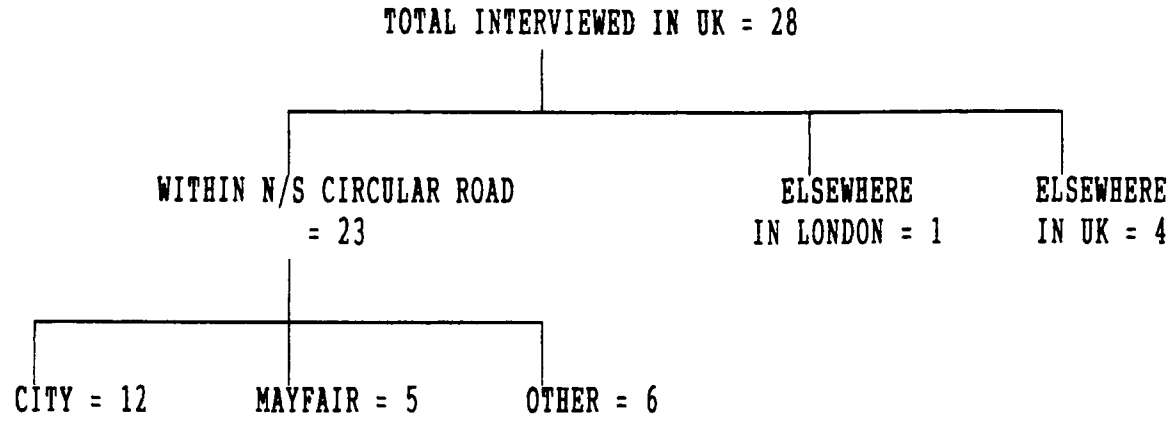
(C) - BANKERS INTERVIEWED

UK BANKERS	
Merchant Banks	
Midland Montagu	Cannon Street
Kleinwort Benson	Fenchurch Street
Warburg Securities	King William Street
Hill Samuel	Wood Street
Morgan Grenfell	Great Winchester St.
General & Commercial Banks	
Chartered West LB	Gracechurch Street
Citibank	Lewisham
Bank Project Finance Activities	
Deutsche Bank	Bishopsgate
NY BANKERS ETC.	
Merchant Banks	
Kidder Peabody	10 Hanover Square
General & Commercial Banks	
Bankers Trust	280 Park Avenue
Chase Manhattan	1 Ch. Manhattan Plaza
Bank Project Finance Activities	
Citicorp	399 Park Avenue
Investment Banks	
Salomon Brothers	One New York Plaza
Brokers	
Shearson Lehman Hutton	AmEx Tower

Looked at in this way, this simple data is remarkably revealing. Considering the City of London first, the addresses show that all the merchant banks interviewed were resident there, together with all but one of the 'captive' venture capital funds (indeed, two were co-located with their parent banks). The fund not in the City, together with the three smaller independent funds, were all in Mayfair/Victoria area. The two large independent funds (both 3i), and the other Government sponsored venture operation (BTG) were all just south of the Thames. In addition, all of the intermediaries, and two of the three other banks, were within these same relatively small central areas of London. The other bank (the only American one interviewed in the UK) was in south-east London.

The most striking feature of all was that out of five seedcorn capitalists, only one was in London at all. The

overall 'score', then, of UK based financial institutions interviewed was as follows:



Of course, there was a considerable element of researcher choice in this - London was recognized as the prime financial centre in the UK, and interviews were therefore solicited from London-based organizations. Equally, when the desirability of interviewing seedcorn people became evident, then the interviewer had to go where they happened to be located.

None of this, however, reduces the interest of the results for this thesis. The geographical fact remains that, in the sample of financial institutions interviewed, the older banks still congregated in the City, and the newer 'higher risk' venture funds had addresses in the more raffish and fashionable areas around Mayfair. The two large operations set up by Government had slipped south of the river, and only one of the seedcorn people was to be found in London at all.

The symbolic and the practical both seem to emerge from all this. At a practical level, these institutions maintained a tradition of personal contact. The very fact that seedcorn is located in 'the provinces' supports a thread of discussion in the interviews that its practitioners feel the need to be closer to their investee businesses than to finance (it also supports the feeling that seedcorn is not a regular part of finance). But at a symbolic level, despite all the institutions keeping within easy distance of one another in central London, it is tempting to see the choice of address as

a statement of the type of business the financiers were in. Funds attached to the old banking houses conservatively stay in the City, 3i and BTG buck the trend by choosing an unfashionable address south of the river for their new types of finance, and then, as venture capital becomes fashionable in the 1980s, the newer funds choose fashionable West London addresses.

It is also tempting to see the one American bank's choice of unfashionable Lewisham in a similar light, although such a conclusion looks rather shaky given the fact that they had another London address in the Strand. The New York interview with the same corporation was on Park Avenue. The New York data, notwithstanding the paucity of the sample of nine interviews, suggests a trend similar to that seen in London. All those interviewed were either around Wall Street (like 'The City', another geographical symbol of finance) in south Manhattan, or in the fashionable areas of mid-Manhattan. Moreover, the two smaller venture capitalists interviewed had the latter address, whilst the one merchant bank was on the southern tip, hard by the wharves of the old port.

The evidence of location, although a very small part of this research, does thus run parallel to the theoretical thread of the main body of this thesis. It suggests that, whilst narrow positivistic interpretations of the activity of finance and industry do have some explanatory power, there is still a very potent symbolic (and historical) undercurrent that cautions against any uncritical, or indeed unitary, acceptance of their conclusions.

APPENDIX VIII - DIFFERENCES

Previous appendices have described how the texts of the interviews can be broken down into categories, and how these - particularly the category that has been called 'topics' - seemed to catch something of the way people talked about their worlds, and what was important to them. Chapter six showed how the 'world-views' of people could very broadly be grouped into discrete language communities or discourses. It was clear that, since topics and discourses had both emerged from the same set of interview data, they ought to be related, and at the end of the appendix to chapter six a set of associations between them were suggested. Topics were grouped under the discourse to which they seemed most appropriate, or under the supplementary heading of boundaries.

Boundaries have been explored in chapter seven in considerable detail. The content of those topics in this boundary area covered more than one discourse, and usually the relationships between them were mentioned. The activity of crossing boundaries emerged as a critical part of the process of innovation, but chapter seven hinted at a few problems with this processual view. One important problem is that even when boundaries were not being talked about, discussion was seldom purely rooted in just one discourse - even at the finely divided level of 'topics', other discourses often crept in around the edges of the dominant one.

Therefore, it seems a fruitful exercise to extend the analysis of appendix six to investigate this. The shared topics are, of course, still associated with the individual people interviewed, and whilst some of these were clearly active in innovation (the 'three-discourse people' suggested at the end of chapter seven), others seemed firmly rooted in activities in which just one discourse predominated. If these people only spoke about that one discourse, and the innovators spoke about all of them, then this would support a simple model of innovation growing in stages - starting in one

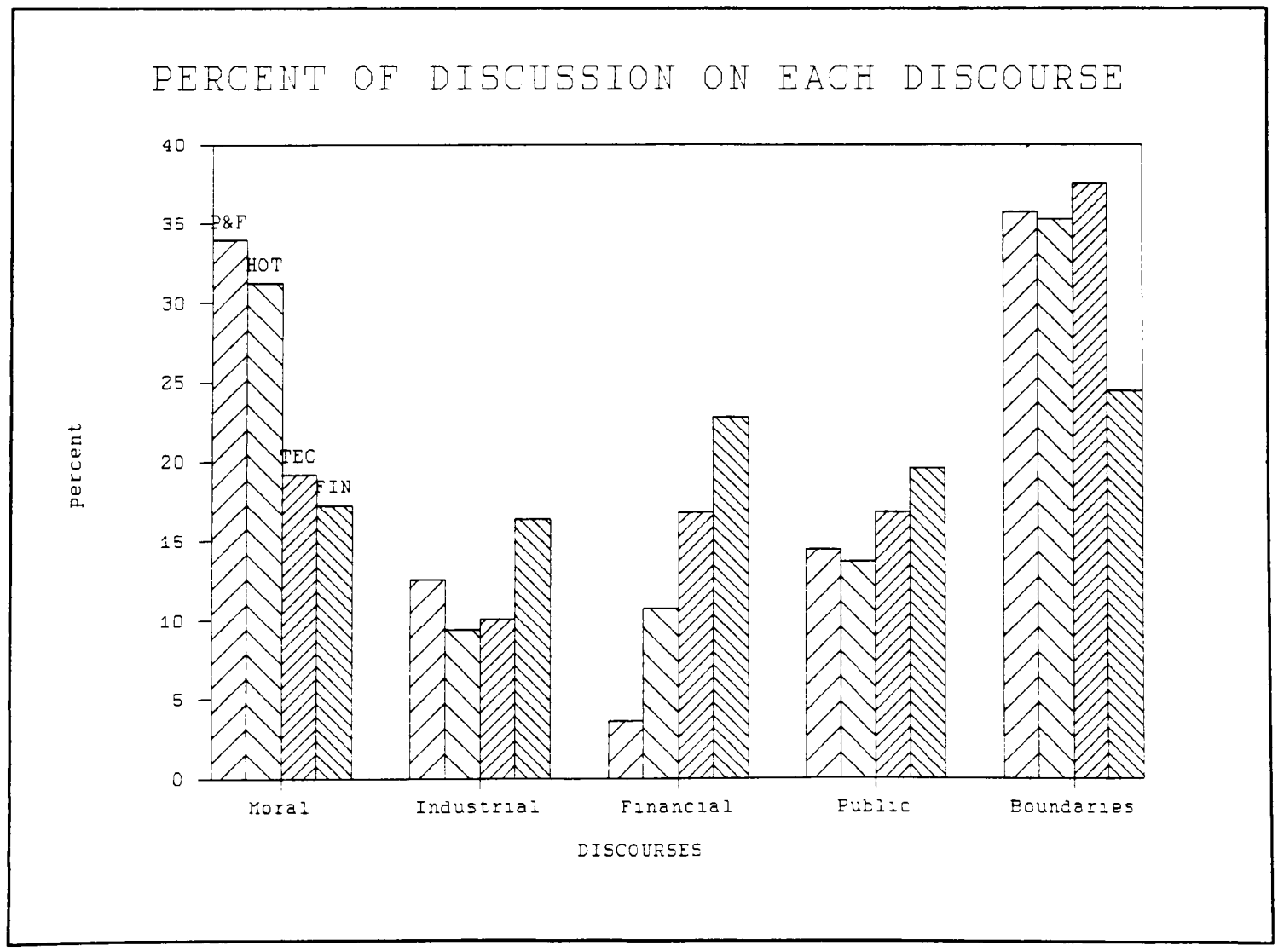
discourse, then crossing a boundary and growing in another, then another, and so on. If, as the problems that began to arise in chapter seven suggest, a staged model is not adequate, then a more complex pattern in the association of topics and people might be expected.

The following table and histogram present the data in this way. Once again the data looks more quantitative than it is - as previous appendices have fully explained, the seemingly precise percentages are based upon impressionistic estimates of the extent to which each topic was discussed (paying due attention to the differences in length of interview and suchlike). Nonetheless, they do allow the histograms to be drawn which in turn aid discussion of the questions just raised.

First of all, the following table and histogram lump all the data together to give an overview. The histogram plots the figures for the four sets of data - 'history' (Parsons & Fisher), 'HOTOL', 'technology' and 'finance' - these are aggregates of the finer divisions (presented below the headings in the table, and plotted in the later histograms of this appendix - note that these are not simple averages of these sub-groups, but weighted according to the numbers of interviews in each).

AMMOUNT OF DISCUSSION RELATED TO EACH DISCOURSE

	Moral Discourse %	Industrial Discourse %	Financial Discourse %	Public Discourse %	Boundaries %
HISTORY	33.9	12.5	3.6	14.3	35.7
Parsons	28.0	16.0	8.0	16.0	32.0
Fisher	38.7	9.7	0.0	12.9	38.7
HOTOL	31.2	9.3	10.6	13.6	35.2
Originators	33.7	7.9	10.1	12.4	36.0
BAe	29.5	10.3	8.3	11.5	40.4
Government	30.8	8.8	15.4	18.7	26.4
R-R	29.7	11.0	11.0	13.2	35.2
Journalists	33.3	0.0	3.7	22.2	40.7
Others	41.2	17.6	17.6	0.0	23.5
TECHNOLOGY	19.2	10.0	16.7	16.7	37.5
FINANCE	17.2	16.3	22.7	19.4	24.4
Seed	16.0	9.1	31.1	15.1	28.8
UK VCs	17.2	15.4	22.4	21.0	24.0
NY VCs	14.4	15.6	36.7	14.4	18.9
UK Banks	19.4	22.5	16.2	18.0	23.9
NY Banks	17.6	18.7	22.5	19.8	21.4
Intermediaries	16.9	17.6	11.8	26.5	27.2

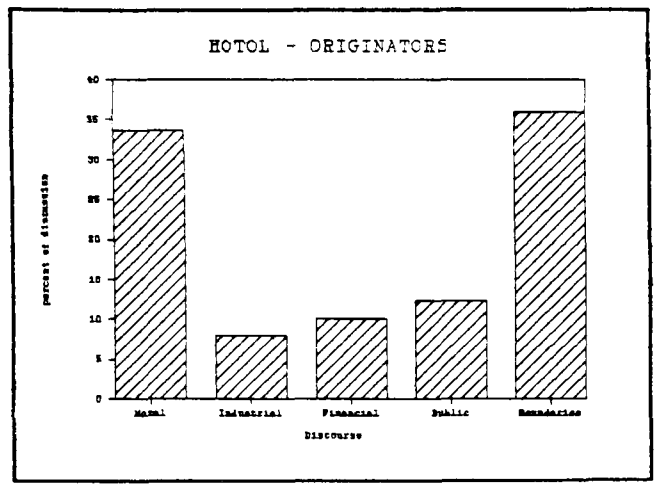


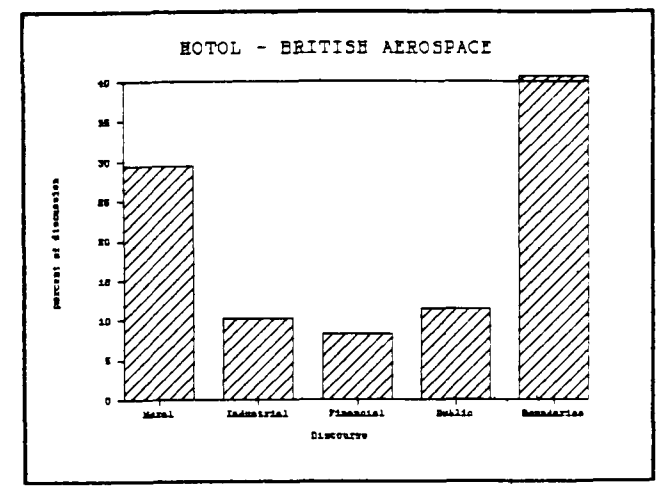
The summary histogram only divides the interviews into the four broad groupings under which the material was analyzed - the historical material consisting of the letters of Parsons and Fisher, the HOTOL interviews, the interviews with those in finance, and finally the four interviews with those who have been labelled technologists. The first point that is immediately apparent is that everyone spoke about all the discourses. Of the four, moral discourse seemed to attract the most comment, although all were well represented.

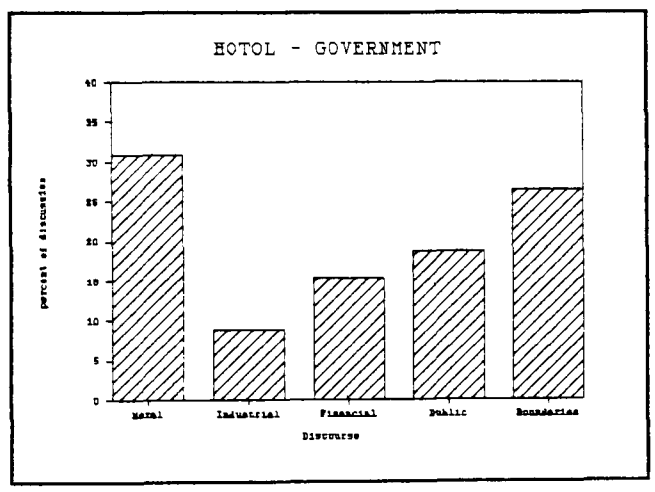
Looking in a little more detail, the texture of the results fit in with what the research of this thesis might predict - Parsons and Fisher, and those concerned with HOTOL, spoke in the language of moral discourse more than the others, and the financiers spoke more financial discourse, and less about boundaries, than anyone else. This suggests that the results are something more than the artefacts of a random set of observations. It also suggests that, since everyone used all of the discourses, the boundary-crossing model is a little too simple to catch the full complexity of innovation.

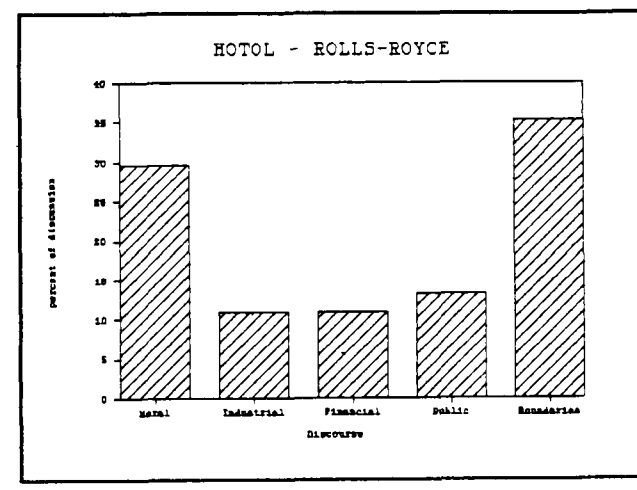
However, there are also puzzling features of this histogram - the financiers spoke more industrial discourse than anyone else for example. It must also be said that the groupings are too broad to answer the original question - in both finance and HOTOL, innovators rub shoulders with dyed-in-the-wool representatives of their industries for instance. The histograms below, therefore, present the data separately for each of the specific groups of people in the table. These groups are a little arbitrary, but do, it is hoped, make fine enough division to be useful in contemplating the questions, without swamping them with the excessive detail of individual interviews.

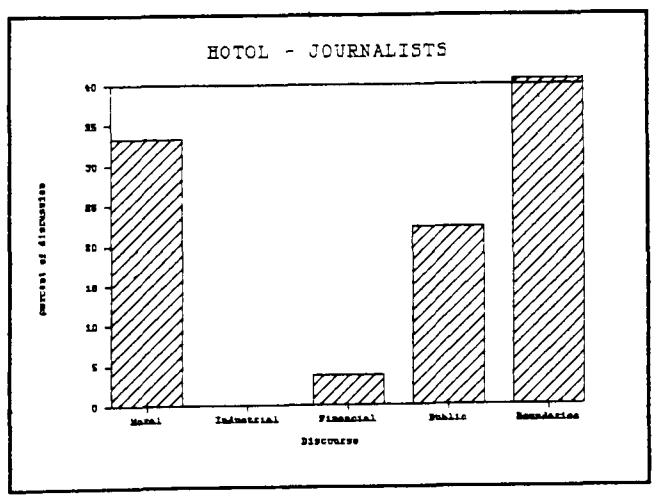
HOTOL

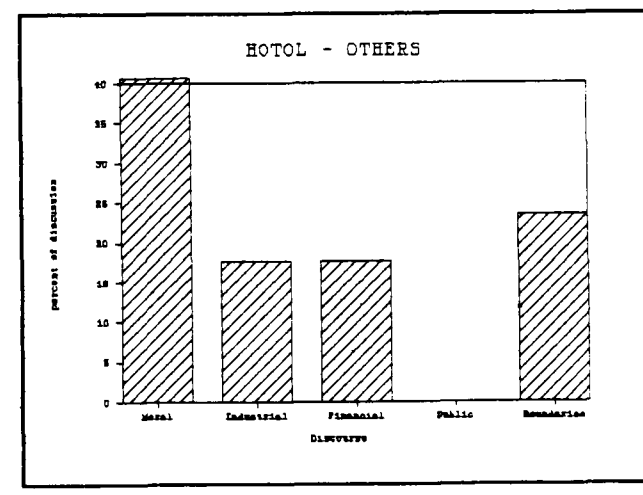

HOTOL - ORIGINATORS
percent of discussion
0
5
10
15
20
25
30
35
40
Moral
Industrial
Financial
Public
Boundaries
Discourse


HOTOL - BRITISH AEROSPACE
percent of discussion
0
5
10
15
20
25
30
35
40
Moral
Industrial
Financial
Public
Boundaries
Discourse

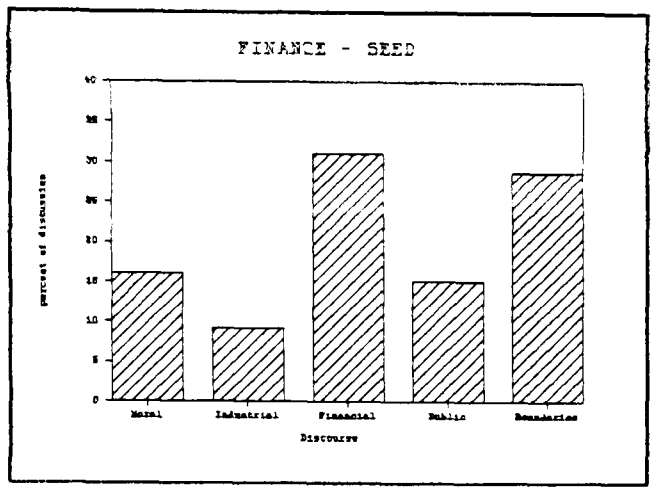

HOTOL - GOVERNMENT
percent of discussion
0
5
10
15
20
25
30
35
40
Moral
Industrial
Financial
Public
Boundaries
Discourse

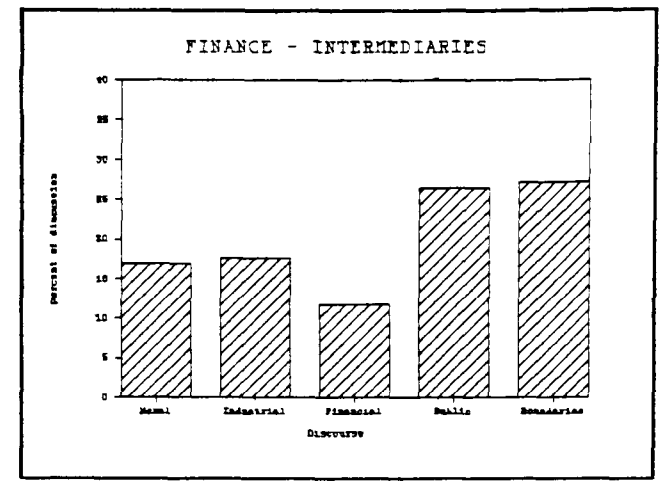

HOTOL - ROLLS-ROYCE
percent of discussion
0
5
10
15
20
25
30
35
40
Moral
Industrial
Financial
Public
Boundaries
Discourse

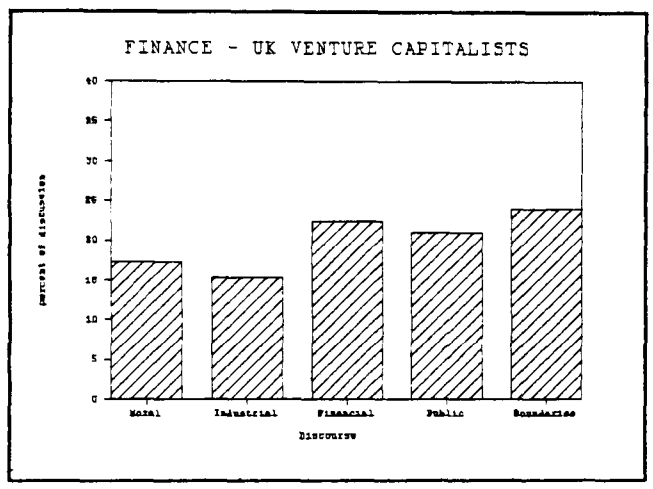

HOTOL - JOURNALISTS
percent of discussion
0
5
10
15
20
25
30
35
40
Moral
Industrial
Financial
Public
Boundaries
Discourse

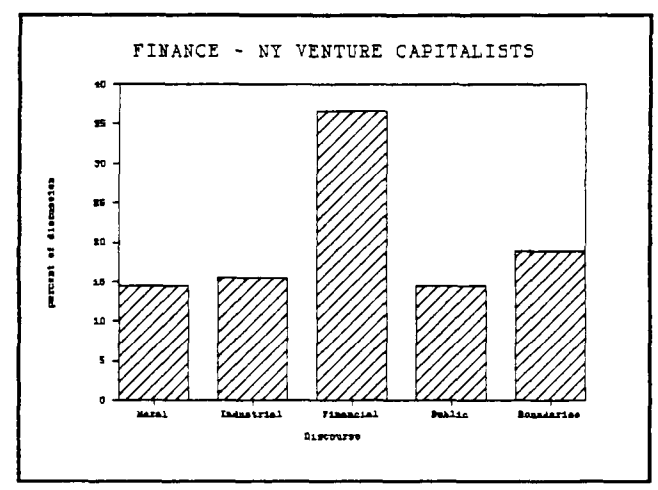

HOTOL - OTHERS
percent of discussion
0
5
10
15
20
25
30
35
40
Moral
Industrial
Financial
Public
Boundaries
Discourse

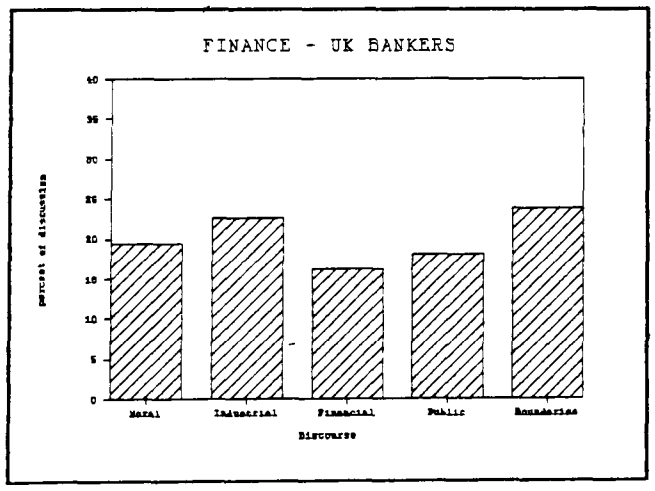
FINANCE

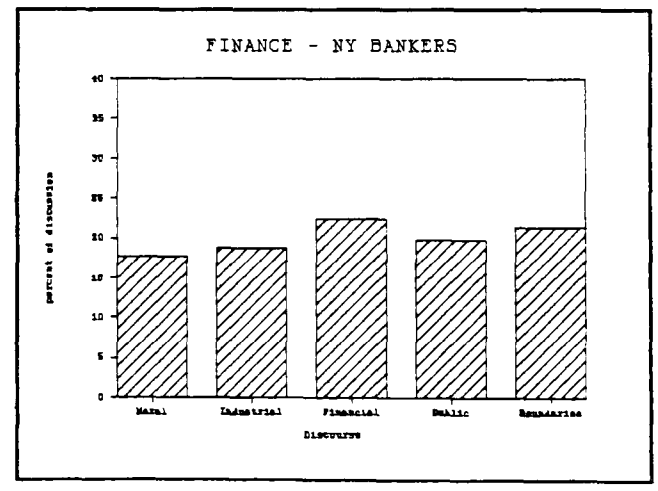

FINANCE - SEED
percent of discussion
Moral
Industrial
Financial
Public
Boundaries
Discourse


FINANCE - INTERMEDIARIES
Percent of discussion
Moral
Industrial
Financial
Public
Boundaries
Discourse

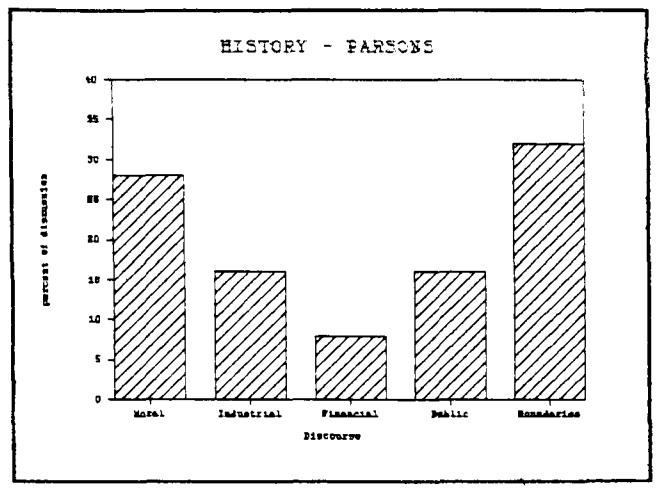

FINANCE - UK VENTURE CAPITALISTS
percent of discussion
Moral
Industrial
Financial
Public
Boundaries
Discourse

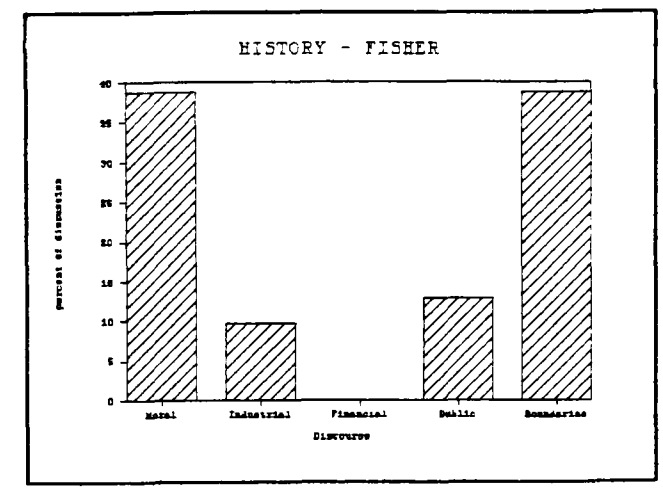

FINANCE - NY VENTURE CAPITALISTS
percent of discussion
Moral
Industrial
Financial
Public
Boundaries
Discourse


FINANCE - UK BANKERS
percent of discussion
Moral
Industrial
Financial
Public
Boundaries
Discourse

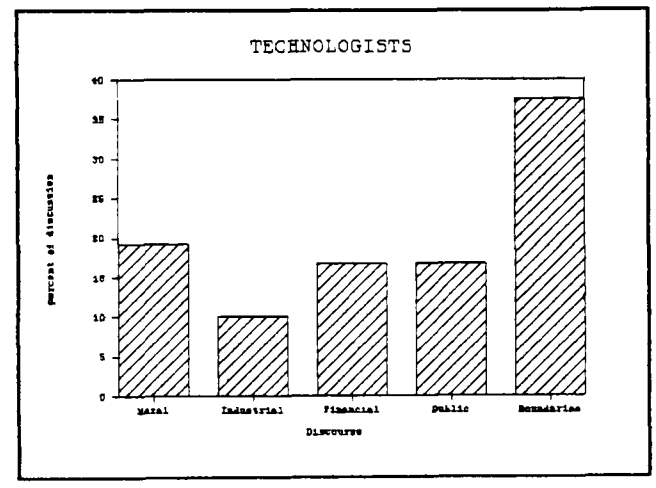

FINANCE - NY BANKERS
percent of discussion
Moral
Industrial
Financial
Public
Boundaries
Discourse

HISTORY

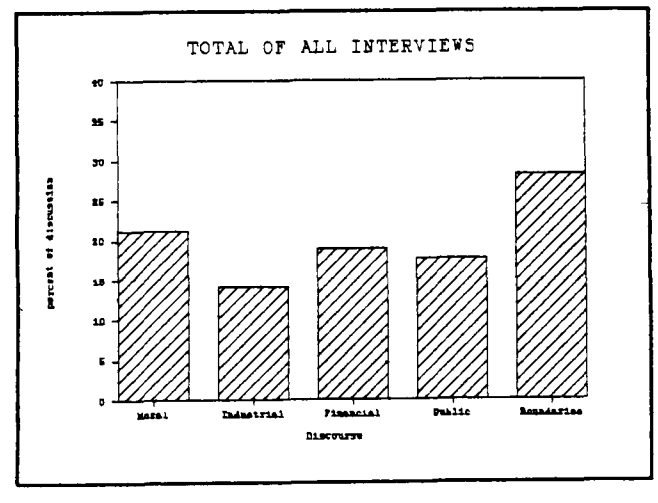




TECHNOLOGY



ALL INTERVIEWS



The final histogram presents all of the interviews (including the Parsons and Fisher letters), as a sort of template for comparison. It is reassuring that the distribution between the different discourses is relatively even in this aggregate of all the data, as might be expected.

The rest of the histograms are a little more confusing. The HOTOL data does not show any great difference between the originators and those in BAe or Rolls-Royce - or in government for that matter. Moreover, in no case is industrial discourse particularly prominent, contrary to what might have been predicted. That moral discourse and boundaries characterized the way they talked is a little more expected perhaps, but the lack of difference between originators and the rest is again perplexing. The only person who fits what might be seen as his stereotype, is the journalist, whose words were spoken in moral and public discourses, and about boundaries - but even here the emphasis on moral discourse is perhaps unexpected.

The interviews in finance were more evenly spread between the four discourses and boundaries, particularly those with the bankers and UK venture capitalists. The New York venture capitalists, and the UK providers of seedcorn spoke the most within financial discourse, perhaps surprisingly, although it is less surprising that the latter, together with the intermediaries, spoke the most about boundaries. That public discourse was also prominent amongst the intermediaries is likewise to be expected - the more so since one of them was a journalist. Any deduction that the seedcorn people ought to look like the inventors whom they might fund (represented perhaps by the HOTOL originators and Parsons) is refuted by the evidence here - they spoke in moral discourse no more than the rest of finance, and in financial discourse much more, as already noted (although it might be posited that this could reflect their greater need symbolically to mark the boundary, due to their greater proximity to it).

The clear emphasis on moral discourse and on boundaries is even more marked in the Parsons and Fisher material than in

HOTOL. On the other hand, the technologists seem to fit somewhere between HOTOL and finance, but their emphasis on boundaries is not unexpected since they were interviewed to illuminate the interface between funding and technology.

There is a temptation to read too much into this data. If a trend does exist within it, it would seem to be that the nearer the group of people is to a radical and innovative project, then the more they talk in moral discourse, and about the boundaries between the different discourses that the project consists in. Games could be played that sought to explain these, and other, differences in the data, but the deliberately impressionistic character of the analysis definitely does not warrant such extended speculation. The single strongest and most abiding conclusion that can be drawn is that everybody talks in all the different discourses all the time, and that they are well aware of the boundaries between them (whether they conceptualize them as boundaries or not). The notion of those who might be singled out as 'innovators' (the 'three-discourse people' of chapter seven) being unique in speaking in more than one discourse is quite plainly wrong. That such categories of people seem to emerge in this study of innovation requires another explanation, as does the observation that they seem to have a greater facility in multiple discourses than others. In a similar fashion, the idea of innovation being characterized by an immutable trajectory of different stages, with people at each stage only speaking in one discourse, is also exploded by this analysis of the data of the interviews.

APPENDIX IX - A SUPPLEMENTARY CONCLUSION

The appendices to the chapters of this thesis have provided supplementary material that would have been out of place within the main body of the document. They have concentrated upon the detailed analysis of the interviews. The insights and interpretations that this analysis has allowed have, however, been included in the main body of the document - indeed the analysis has been vital to the development of the discussion and to the conclusions that have been tentatively drawn. The conclusion to be drawn from the appendices, then, is that they, and the analysis that they have described, are supplementary in a truly Derridan sense - they add to, but also supply a lack within, the document they supplement.

The significance of such things as appendices and footnotes is not lost upon Derrida himself, how could it be - he is a master of infinitely detailed textual readings:

> In the essay 'Living On: Border-lines' .... a running footnote .... accompanies the text for its entire length and raises certain questions .... which cannot be regarded as in any sense 'marginal' (Norris 1987:64)

The appendices here are offered in something of the same spirit as Derrida's wicked supplementary purpose in the essay Norris describes. But such a purpose is indeed a serious and an ethical one - Derrida is saying (or rather, writing), and this thesis is illustrating, that any debate about positivistic versus interpretive methods, quantitative versus qualitative data, structural versus processual theories, is a hollow debate.

Derrida, at the end of his seminal, and misinterpreted, essay upon Lévi-Strauss, notes:

> There are more than enough indications today to suggest we might perceive that these two interpretations of interpretation - which are absolutely irreconcilable even if we live them simultaneously and reconcile them in an obscure economy - together share the field which we call, in such a problematic fashion, the social sciences. (Derrida 1978:293)

This thesis has shown something of this 'obscure economy', both in the action described in its data and in the ideas developed in its discussion - the action of those who innovate and the ideas of Jacques Derrida. A rigorous and ordered analysis of the texts of the interviews has been necessary to do this - an analysis that recalls both Lévi-Strauss's analysis of myth, and Derrida's meticulous reading of Lévi-Strauss. If the discussion of these interviews in this thesis has sometimes looked a fragmentary and disordered affair, this does not in any way mean that rigour has been abandoned, and licence taken profligately to play with the data. In the 'real world' of understanding innovation it is not a case of choosing between rigour and licence - licence is the child, and the false claim, of a limited scholarly discourse - the 'real world' cannot recognize such limits. Derrida continues:

> For my part, although these two interpretations must acknowledge and accentuate their difference and define their irreducibility, I do not believe that today there is any question of <u>choosing</u> - in the first place because we are in a region .... where the category of choice seems particularly trivial; and in the second, because we must first try to conceive of the common ground, and the 'différance' of this irreducible difference. (Derrida 1978:293)