THE DEVELOPMENT OF CHEMICAL EXPOSURE LIMITS FOR THE WORKPLACE

MARK PINEY

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Summary:

The thesis examines and explains the development of occupational exposure limits (OELs) as a means of preventing work related disease and ill health. The research focuses on the USA and UK and sets the work within a certain historical and social context. A subsidiary aim of the thesis is to identify any short comings in OELs and the methods by which they are set and suggest alternatives.

The research framework uses Thomas Kuhn's idea of science progressing by means of paradigms which he describes at one point as, "... universally recognised scientific achievements that for a time provide model problems and solutions to a community of practitioners." KUHN (1970). Once learned individuals in the community, "... are committed to the same rules and standards for scientific practice." Ibid. Kuhn's ideas are adapted by combining them with a view of industrial hygiene as an applied science-based profession having many of the qualities of non-scientific professions. The great advantage of this approach to OELs is that it keeps the analysis grounded in the behaviour and priorities of the groups which have forged, propounded, used, benefited from, and defended, them.

The development and use of OELs on a large scale is shown to be connected to the growth of a new profession in the USA; industrial hygiene, with the assistance of another new profession; industrial toxicology. The origins of these professions, particularly industrial hygiene, are traced. By examining the growth of the professions and the writings of key individuals it is possible to show how technical, economic and social factors became embedded in the OEL paradigm which industrial hygienists and toxicologists forged. The origin, mission and needs of these professions and their clients made such influences almost inevitable.

The use of the OEL paradigm in practice is examined by an analysis of the process of the American Conference of Governmental Industrial Hygienists, Threshold Limit Value (ACGIH, TLV) Committee via the Minutes from 1962-1984. A similar approach is taken with the development of OELs in the UK. Although the form and definition of TLVs has encouraged the belief that they are health-based OELs the conclusion is that they, and most other OELs, are, and always have been, reasonably practicable limits: the degree of risk posed by a substance is weighed against the feasibility and cost of controlling exposure to that substance. The confusion over the status of TLVs and other OELs is seen to be a confusion at the heart of the OEL paradigm and the historical perspective explains why this should be. The paradigm has prevented the creation of truly health-based and, conversely, truly reasonably practicable OELs.

In the final part of the thesis the analysis of the development of OELs is set in a contemporary context and a proposal for a two-stage, two-committee procedure for producing sets of OELs is put forward. This approach is set within an alternative OEL paradigm. The advantages, benefits and likely obstacles to these proposals are discussed.

KEYWORDS:
OCCUPATIONAL HEALTH; OCCUPATIONAL EXPOSURE LIMIT; THRESHOLD LIMIT VALUE; INDUSTRIAL HYGIENE

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Changes in PhD Supervision

Date	Description	Supervisors
30.11.83	Application to register for a higher degree.	Dr AR Hale, Supervisor Dr LS Levy, Associate
27.1.84	Application accepted. Start date 29.4.84	Dr AR Hale, Supervisor Dr LS Levy, Associate
Sept. 1984	Dr Levy returns from a 1 year sabbatical in Israel and takes over as main supervisor from Dr AR Hale who is due to move to Delft University in the Netherlands (December 1984).	Dr LS Levy, Supervisor Professor RT Booth, Associate
Nov. 1984	Dr Levy moves, with his research team, to Birmingham University	Dr LS Levy, Supervisor Professor RT Booth, Associate
April 1985	First year report submitted and accepted. Registered as a candidate for the degree of PhD.	Dr LS Levy, Supervisor Professor RT Booth, Associate
21.11.86	Additional Associate supervisor confirmed to oversee the social policy areas of research.	Dr LS Levy, Supervisor Professor RT Booth, Associate Dr F Steward, Associate
April 1989	Professor RT Booth becomes main supervisor	Professor RT Booth, Supervisor Dr LS Levy, Associate Dr F Steward, Associate

CONTENTS

List of Tables

Appendices

List of Figures

Appendices

GLOSSARY

AAIH	American Academy of Industrial Hygiene
ACGIH	American Conference of Governmental Industrial Hygienists (was NCGIH up to 1945).
ACTS	Advisory Committee on Toxic Substances.
AHF	Air Hygiene Foundation (from 1939 = Industrial Hygiene Foundation and from 1971 = Industrial Health Foundation)
AIHA	American Industrial Hygiene Association.
AOH	Annals of Occupational Hygiene (1958)
AOMA	American Occupational Medicine Association. (formerly the Industrial Medical Association (IMA)).
APHA	American Public Health Association (1869)
API	American Petroleum Institute
ASA	American Standards Association
BEBOH	British Examining Board in Occupational Hygiene (1967-1978).
BERBOH	British Examining and Registration Board in Occupational Hygiene (1978, formerly BEBOH).
BOHS	British Occupational Hygiene Society.
CIB	Current Intelligence Bulletin (issued by NIOSH).
DFG	(Deutsche dorchungsgenreinschaft - German Research Association).
EMAS	Employment Medical Advisory Service.
EPA	(US Environmental Protection Agency).
FCG	Field Consultancy Group (set up within HSE in 1976).
HSC	Health and Safety Commission (formed in 1974).
HSE	Health and Safety Executive (formed in 1974).
IChemE	Institute of Chemical Engineers.
ICRP	International Commission on Radiological Protection.
IH	Industrial Hygiene
IHF	Industrial Hygiene Foundation (formerly the Air Hygiene Foundation, since 1971 = Industrial Health Foundation).
IHRB	Industrial Health Research Board (set up in 1918).
IMA	Industrial Medical Association (now called the American Occupational Medicine Association (AOMA)).
IOH	Institute of Occupational Hygienists (1975).
IT	Industrial Toxicology
MAC	Maximum Allowable Concentration or Maximum Acceptable Concentration
NPL	National Physical Laboratory
NCGIH	National Conference of Governmental Industrial Hygienists (1938-1945 renamed ACGIH in 1946).
NTP	National Toxicology Programme.
NIC	Notice of Intended Change (a preliminary list of possible TLV's introduced in 1967).
NIOSH	National Institute of Occupational Safety and Health
OEL	Occupational Exposure Limit (the generic term used for chemical exposure limits in the workplace in the thesis).
OMB	Office of Management and Budget.
OSHA	Occupational Safety and Health Administration.
PEL	Permitted Exposure Level OEL produced by OSHA.
(US) PHS	United States Public Health Service.
PRO	Public Records Office (Kew Gardens, Richmond, Surrey).
REL	Recommended Exposure Limit (NIOSH suggested OELs).
STEL	Short Term Exposure Limit.
TWA	Time Weighted Average
WATCH	Working Group on the Assessment of Toxic Chemicals
WHO	World Health Organisation

PART ONE

RESEARCH RATIONALE AND METHODOLOGY

CHAPTER ONE

INTRODUCTION

In 1964 Peter Medawar wrote a paper entitled "Is the scientific paper a fraud?" MEDAWAR (1964). He argued that it was in its traditional form that is – Introduction, Method, Results, Discussion and Conclusions, and summed up what was wrong as follows:

"What is wrong with the traditional form of scientific paper is simply this: that all scientific work of an experimental or exploratory character starts with some expectations about the outcome of the inquiry ... It is in the light of this expectation that some observations are held relevant and others not; that some methods are chosen, others discarded; that some experiments are done rather than others." Ibid.

He continues later in the paper:

"... the scientific paper is a fraud in the sense that it does give a totally misleading narrative of the processes of thought that go into the making of scientific discoveries... The discussion which ... goes last should surely come at the beginning. The scientific facts and scientific acts should follow the discussion, and scientists should not be afraid to admit ... that hypotheses appear in their minds along uncharted byways of thought; that they are imaginative and inspirational in character ...." Ibid.

In this research tentative hypotheses have been generated and I have attempted to follow them to their conclusions; questions are asked and more or less satisfactory answers are given and explanations are developed and explored which I hope produce a deeper and more complete understanding of the development of chemical exposure limits than past writers. This research is unlike the experimental science for which Medawar was famous but it has developed and evolved in a manner similar to that which he describes.

1.0 Introduction

This chapter explains some of the background behind the author's research, the aims of the research and the way the aims and methodology evolved in the process of the research. Finally the structure of the thesis is described.

1.1 Reasons for the research

I have been a practising occupational hygienist for over ten years, working mainly in an academic setting. Like all hygienists occupational exposure limits (OELs) have been central to my day-to-day practice. Exposure measurements are routinely compared to the appropriate OELs. If exposure is above the OEL the automatic judgement is normally that better control should be exercised. If exposure is below the OEL, whether improvements in control are needed depends upon the potential health effects that exposure might induce, the control method currently in use and how easily control could be improved. To make this judgement I have routinely explored the background behind the OEL numbers, I have never been content to take them at face value. There are various condensed background sources of information on OELs including the Documentation of Threshold Limit Values produced by the US American Conference of Governmental Industrial Hygienists (ACGIH), the Criteria Documents produced by the US National Institute of Occupational Safety and Health (NIOSH) and, more recently, the Toxicity Reviews produced by the UK Health and Safety Executive (HSE).

In 1978 when I first started occupational hygiene work in earnest there were only the Documentation and the Criteria Documents and I, like most hygienists, would automatically go to the Documentation as my first port of call. The Documentation published since 1962 gives a summary of the evidence on which the TLV numbers are set. Study of the Documentation caused me to come to various conclusions. Firstly, the amount of information in the Documentation behind a particular TLV varied enormously in quality and quantity. Secondly, as one moved from the first edition to the fourth (there is now a fifth edition) interpretation of evidence was revised. Usually new evidence was cited but it was also clear that on occasions the committee's view of earlier evidence had also changed. Thirdly in quite a number of cases, at least to the author, it was not clear how the Committee chose a particular TLV number. For instance, with styrene monomer irritant effects were reported in the 3rd edition of the Documentation, ACGIH (1971) at 50ppm and yet a TLV (8 hours Time Weighted Average (TWA)) was set of 100ppm. Similarly, the Documentation for trichloroethylene (5th edition, ACGIH (1980)) quotes evidence to suggest that nervous systems effects can occur at TWA concentrations down to at least 10ppm yet the Committee decided upon a TLV of 50ppm. Finally, the Committee did not come to the same OEL numbers apparently using the same evidence as other organisations, in particular NIOSH. Thus the latter recommended 250ppm, 1ppm and 25ppm for acetone, carbon disulphide and trichloroethylene whereas the Committee recommended 750ppm, 10ppm and 50ppm respectively (1980 figures).

The Documentation only presents evidence on health effects, the implication being that only health effects are the concern of the Committee in its deliberation. In this context the TLV number choices made in the cases of styrene, trichloroethylene and other substances were confusing. Other factors were clearly intervening in the standard setting process. The question was, what other factors and how did they affect the process of the Committee?

Up until recently TLVs have been used officially or unofficially by most hygienists in the Western world. The obvious starting point for an exploration of the development of chemical exposure limits for the workplace was the work of the ACGIH TLV Committee. There were other groups to be considered in the UK context, particularly the BOHS and the HSE, but the TLVs clearly came first because they were the first widely applied OELs.

1.2 Development of the analytical framework

In the same way that an examination of the TLV Documentation raises a number of questions so the descriptions and statements for and against TLVs do likewise. On the one hand senior individuals in hygiene and medicine make statements such as those listed in Table 1.1. While on the other hand others, particularly in recent years, take great, and sometimes vitriolic, exception to the status and influence of TLVs, see Table 1.2.

TABLE 1.1
AFFIRMATIVE STATEMENTS ON TLVS

TLVs ensure that there is, "... no hazard to health or well being of the worker during his working lifetime." STOKINGER and WOODWARD (1958).

"The best information we have on the greatest concentrations of respirable substances which are acceptable in the working environment is the accumulated experience of the profession of industrial hygiene, expressed in the annually revised lists of threshold limits." SMYTH (1959)

"The emphasis in this field (toxic substances) is now placed heavily on prevention through scientific assessment of the risks and precise quantification of preventative standards ..." (TLVs) "Robens Committee" GREAT BRITAIN (1972B).

"The TLVs represent time-weighted average concentrations of airborne substances associated with industrial operations and manufacture, designed to protect the health and well-being of nearly all workers ... not only for their working lifetime, but after retirement." STOKINGER (1972a).

"The industrial hygienist who is able to keep his level of exposure below the TLV limit is not guaranteed freedom from any problem whatsoever, but, if a problem does occur it should be manageable." ZAPP (1977).

"I believe the scientific information supports the concept of dose-effect threshold for most chemical substances ... TLVs can never be used to guarantee absolute safety, but they can be used to control adverse health effects of all types below the point at which they can be distinguished from their background occurrence." MASTROMATTEO (1981).

"... threshold levels for all substances in the workplace do in fact exist, (and) one of the major research activities of industrial hygiene and its related occupational health disciplines is to define these levels." SMITH (1985).

"The (TLV) Committee idealistically functions as occupational health professionals by recommending TLVs to protect the health of workers **without regard** to economic or technical feasibility." LEE (1987) (Emphasis in the original).

"TLVs are health-based recommendations derived from an assessment of the available scientific information ... (they) are based on a belief in a threshold ... below which no adverse health effects would occur in workers." MASTROMATTEO (1988).

TABLE 1.2
STATEMENTS CRITICAL OF TLVS

"Prior to the date (1972), ACGIH efforts were praiseworthy as voluntary private acts by government officials, acting as individuals in alliance with management specialists, to control acute effects prior to the passage of the (OSHA) Act." SAMUELS (1981)

"A review of the existing documents which were produced to explain or justify the setting of occupational exposure standards reveals that those who performed this work in the past argued rather generously, to say the least .... A careful review of some 150 occupational chemicals in the German list of MAK - Values (based on the TLV list) revealed that less than 10% of the respective exposure limits is based on appropriate and sufficient animal tests and/or field experience." HENSCHLER (1984).

"If you poison your boss a little bit each day it's called murder; if your boss poisons you a little each day it's called a Threshold Limit Value." (J P Keogh, quoted in CASTLEMAN (1984)).

"Reverence for life requires you to end the wholesale dissemination to other nations of recommendations for standards (TLVs) less stringent ... than these prevalent .... in our own country. I beg you to end your complicity in the perpetuation of unnecessary suffering and the death of millions of workers in societies less privileged than our own.. End the TLV Committee. Force its true sponsors to find another front for their immoral mayhem." SAMUELS (1987).

"The consequences of such misplaced confidence in the TLVs are profound and global. The credibility of the ACGIH limits as scientifically, independently, and verifiably determined persists as an obstacle to a better standard of worker protection." CASTLEMAN and ZIEM (1988).

The questions raised by examining the Documentation and the irreconcilable statements in the Tables indicated that while scientific information was clearly used in arriving at TLV numbers other factors impinged. How and via what route was not immediately clear. What was clear was that research which concentrated solely on the science in OEL setting would not produce a credible explanation, the net needed to be cast wider. The problem was where to start and the simple answer was at the beginning in the history of OELs. But, like any history of ideas, one cannot write about them in a vacuum. Ideas are espoused and owned, defended and attacked by different groups in society. It seemed a legitimate intuitive assumption to pick US industrial hygienists (IHs) as the locus for any historical exploration. They were the group, specifically the sub-group of ACGIH, who had created the TLV. As to how to limit the scope of the research; from my limited reading at the start it appeared that professional groups such as IHs, industrial toxicologists (ITs) and industrial physicians (IPs) had been the main members of OEL standard

setting committees and had written almost all the literature on the subject. They were central actors on the stage but not the only ones. I decided to concentrate on the role of professions in the development of OELs but to keep an open mind on what other groups and influences might have been at play. The initial focus of the research was thus the development of the IH profession the intuition being that OELs, particularly TLVs were principally a product of this profession. The framework used to assess and plot the development of IH was based on an ecological model of professional development which I had a part in developing, HALE et al (1984). By this means the transmutation of medical IH to non-medical IH and its subsequent development was examined in some detail. The method worked in the sense that it confirmed that OELs were clearly a product of the day-to-day professional activities and needs of non-medical IH. However it was only towards the end of the historical analysis that an "inspirational hypothesis" (as Medawar might was called it) came to mind.

IH was not simply a profession like accountancy it was a science based profession and this made a difference. The evolution and subsequent development of IH had much in common with the model of how sciences develop and progress put forward by Thomas Kuhn. By combining the insights gained in the historical analysis of IH/IT as a science based profession with Kuhn's view of science as practiced by professional scientists a coherent, and I believe original, analytical framework for examining the development of chemical exposure limits for the workplace was forged. It is explained in Chapter 2.

Before the research framework is described the overall aims of the research should be stated.

1.3 Research aims and objectives

The original provisional aim of the research was very general: to examine and explore the development of chemical exposure limits and explain the confusions referred to earlier. However once the historical work had been completed and a more comprehensive framework developed the aims became more finely honed and specific objectives were formulated.

The aims of the research are:

1. To gain an understanding of, and explain, the development of OELs, including the connection between the IH profession and the paradigm it promulgated.

2. To gain understanding and explain the practice of OEL setting committees with a view to proposing alternative and improved arrangements.

The objectives of the research are:

1. To examine and explain the development of the profession of IH in the USA and later in the UK.

2. To examine and explain the connection between IH and the paradigm crystalised by this group – the OEL paradigm, and to do this within the research framework.

3. To explore the interpretation of the OEL paradigm by key individuals.

4. To examine and explain how the OEL paradigm was translated in practice. In particular to examine the work of the ACGIH TLV Committee and to a lesser extent OEL setting in the UK.

5. To examine critically the context in which OEL setting takes place and to develop an alternative practice and an alternative paradigm.

1.4 Structure of the thesis

Medawar argues that scientific papers should perhaps start with the discussion. This thesis follows this suggestion in part with an extended discussion of the analytical framework in the next chapter. Even so, it does not have what might be described as the traditional structure whereby all the discussion and conclusions are saved until the last chapters. As each of the five Parts of the thesis unfold questions arise, issues are discussed and conclusions are drawn. To do otherwise would be artificial and result in a rather stilted writing style. While Part Five contains an extended discussion and the major conclusions the interested reader who wants more detail should locate the more discursive chapters, and their associated appendices, via the contents pages and sub-headings.

The evidence to support the various themes of the thesis is drawn from a variety of sources, including published papers and books, committee minutes (ACGIH and ACTS), a content and author analysis of a journal, a pilot exercise in setting health-based OELs and interviews with a number of US and UK industrial hygienists, physicians and toxicologists (see Appendix 7). In order not to disrupt the flow of the argument a significant amount of the detailed analyses have been placed in Appendices.

CHAPTER TWO

OCCUPATIONAL HEALTH, OCCUPATIONAL EXPOSURE LIMITS AND THE GROWTH OF A NEW SCIENCE BASED PROFESSION

"Both normal science and revolutions are ... community-based activities. To discover and analyse them, one must first unravel the changing community structure of the sciences over time. A paradigm governs, in the first instance, not a subject matter but rather **a group of practitioners. Any study of paradigm-directed or of paradigm-shattering research must begin by locating the responsible group or groups**", KUHN (1970), (Author's emphasis).

"Whatever scientific progress may be, we must account for it by examining the nature of the scientific group, discovering what it values, what it tolerates, and what it disdains." KUHN (1970).

SCIENTIFIC PROFESSIONS, PARADIGMS AND PROFESSIONAL SCIENTISTS

2.1 Introduction

The person who has illuminated our understanding of the development of scientific communities, the paradigms by which they work and how they change is Thomas Kuhn and his seminal work, "The Structure of Scientific Revolutions" (SSR). This work, first published in 1962, dramatically changed previous conceptions of how sciences develop and change and why they are so productive.

Kuhn's work was and still is concerned with the development of the relatively "pure" sciences, that is the sciences which are not usually applied directly to industrial problems. He therefore only tends to address influences on the various scientific communities he has studied in terms of theoretical or experimental debates in these sciences and sometimes the effect of a paradigm shift in one science and its effect on another. He does not, because his subject matters tends not to demand it, deal with more general societal influences. According to Masterman he regards "... technology ... outside the sphere of the philosophy of science." MASTERMAN (1970). Kuhn's analysis of how scientific communities and paradigms have developed is thus, narrowly drawn. Although his work has enabled the development of a whole branch of the sociology of knowledge which, before, tended to treat science as unified, and scientific knowledge as somehow a special almost non-human kind of knowledge, Kuhn himself has stayed close to his roots; physics and the philosophy of science. Works such as those by BLUME (1974), YOUNG (1971), WHITLEY (1974) and DICKSON (1979) deal with the wider connection between the society in which sciences are practiced and the structure, practice and the paradigms of the scientific communities which exist in that society.

As this thesis is set against Thomas Kuhn's ideas of science and its evolution, the next section describes his ideas in some detail.

2.2 Paradigms, Scientific Communities and Change - Thomas Kuhn's Structure of Scientific Revolutions

In a recent anthology of papers devoted to Kuhn's work the editor describes SSR as having had: "a wider academic influence than any other single book of the last twenty years", (page v, GUTTINGS (1980)). This influence has extended through the philosophy, history and sociology of science to relatively distant areas of the academic spectrum such as economics, politics and theology.

Kuhn's book is seminal not because it contains ideas that others had not put forward before (indeed Kuhn acknowledges, within the first two pages of his book, his intellectual debt to early 20th Century writers such as Alexandre Koyne (1939), Emile Meyenson (1930), Helene Metzyer (1923) and Anneliese Maier (1949) (see SSR footnote on page vi)) but because he pulled together a collection of disparate thoughts and consistently tied his theoretical explanations to detailed histiographic research on particular developments in science. He also shows a profound insight into the psychology of human thought, perception and learning. He did not present idealised models of "science" or "scientific method".

2.2.1 Paradigms and Normal Science

"That there is normal science – and that it is exactly as Kuhn says it is – is the outstanding, the crashing by obvious fact which confronts and any philosophers of science who in a practical or technology manner, do any actual scientific research. It is because Kuhn... has noticed this central fact about all real science (basic research, applied, technological are all alike here) namely that it is normally a habit-governed, puzzle-solving activity ... that actual scientists are ... increasingly reading Kuhn instead of Popper." MASTERMAN (1970).

Early on in SSR Kuhn defines a paradigm as follows:

"These I take to be universally recognised scientific achievements that for a time provide model problems and solutions to a community of practitioners", Kuhn (1970).

He does not use the term in its normal sense, that is as a model or pattern, but gives it a much more open ended meaning:

"In a science, a paradigm is rarely an object or replication. Instead, like an accepted judicial decision in the common law, it is an object for further articulation and specification under new or more stringent conditions. To see how this can be so, we must recognize how very limited in both scope and precision a paradigm can be at the time of its first appearance. **Paradigms gain their status because they are more successful than their competitors in solving a few problems that the group of practitioners has come to recognise as acute**", KUHN (1972), Author's emphasis.

"To be accepted as a paradigm, a theory must seem better than its competitors, but it need not, and in fact never does, explain all the fact with which it was confronted". KUHN (1970).

Kuhn differentiates between pre- and post-paradigmatic sciences and observes that no one paradigm dominated the practice of a science apart from mathematics and astronomy before the seventeenth century. Up to this time various schools would put forward fundamentally different explanations for some phenomena:

"Being able to take no common body of belief for granted, each writer ... felt forced to build his field anew from its foundations ... Under these circumstances, the dialogue of the resulting books was often directed as much to the members of other schools as it was to nature", KUHN (1970).

Only after a certain schools views became the generally accepted paradigm could the community of scientists, who accepted the paradigm settle down to what Kuhn calls "normal science" and move into a period of post-paradigm productivity. A dominant paradigm has a very dramatic effect on the productivity of a science:

"... partly because the end of interschool debate ended the constant reiteration of fundamentals and partly because the confidence that they were on the right track encouraged scientists to undertake more precise, esoteric and consuming sorts of work ," KUHN (1970).

Once a paradigm is accepted by a group of practitioners, scientific activity, or "normal science" consists of puzzle solving and;

"... A devoted attempt to force nature into the conceptual boxes supplied by professional education", KUHN (1970).

Three years after SSR was first published a conference solely devoted to the criticism and analysis of Kuhn's ideas took place in London. It was addressed by most of the leading philosophers and historians of science, including Popper, Feyerabend, Lakatos, Toulmin and Kuhn himself, (see LAKATOS and MUSGRAVES, 1970). A number of speakers tended, as one might expect, to defend their earlier, pre-Kuhnian, position. Thus Popper did not believe "normal science" existed and thought it a dangerous idea and Toulmin did not believe that revolutions occurred. However a number of other speakers analysed and extended Kuhn's ideas. Masterman, in particular, added to and extended our understanding of paradigms and how they function which Kuhn himself

acknowledged in the 2nd edition of his book. She counted the number of definitions of paradigms in SSR and found there were 21 distinct variants which fell into three main groups which she called metaphysical, sociological and construct or artefact paradigms. In discussing the second grouping Masterman develops a clear description of a paradigm which adds to the description I have given based on Kuhn's work. She says that: "As a paradigm is a set of scientific habits. By following these, successful problem solving can go on: thus they may be intellectual, verbal, behavioural, mechanical, technological; any or all of these." And his metaphysical paradigm "... is something far wider than, and ideologically prior to theory i.e. as a whole Wetanschaung."

Masterman's elucidation reminds one that much intellectual and technological practice undertaken by a scientific speciality, working to a particular paradigm, is routine and shaped by habit. Thinking habits can be just as difficult to change as the routine habitual application of certain experimental techniques. She also wrestles with the unsatisfactory discussion of what governs community activity before a paradigm and identifies three different conditions:

1. No paradigm
2. Multiple paradigm
3. Dual paradigms.

Kuhn accepted this elaboration and admitted that his analysis of the transition from pre-scientific to mainstream scientific activity governed by one paradigm, was unsatisfactory.

In addition Masterman also clarifies how a paradigm works and declares that a paradigm "... is something which can function when the theory is not there ... it is an artefact which can be used as a puzzle-solving device.' and to be used this way it has got to be, in the minds-eye of the scientists" a concrete "picture" which is used analogically. It has got to be, in Kuhn's words, "a way of seeing".

It is easy to misdefine paradigm as, for instance, "basic theory" or "a general metaphysical viewpoint" but this is incorrect. The power of the concept lies, as Masterman points out, in the fact that it fits the picture of what working scientists and the communities and groups of which they are a part, actual do. And it fits what is known of the evolution of various sciences.

2.2.2 Learning a Paradigm

A paradigm is learnt, and the scientific profession reproduces itself, by the younger or aspiring members studying the standard texts and exemplary problems and solutions.

"The study of paradigms, including many that are far more specialized than those named illustratively above, is what mainly prepares the student for membership in the particular scientific community with which he will later practice. Because he there joins men who learned the bases of their field from the same concrete models, his subsequent practice will seldom evoke overt disagreement over fundamentals. Men whose research is based on shared paradigms are committed to the same rules and standards for scientific practice", KUHN (1970).

Kuhn spends some time in the post script to the second edition of his book considering how science is learnt. He expands on his use of the term "exemplar":

"By it I mean, initially, the concrete problem-solutions that students encounter from the start of their scientific education, whether in laboratories, on examinations, or at the ends of chapters in science texts. To these shared examples should, however, be added at least some of the technical problem-solutions found in the periodical literature that scientists encounter during their post-educational research careers and that also show them by example how their job is to be done", KUHN (1970).

And by doing so challenges the orthodox view of how a student becomes a scientist. Kuhn argues that:

"Philosophers of science have not ordinarily discussed the problems encountered by a student in laboratories or in science texts, for these are thought to supply only practice in the application of what the student already known. He cannot, it is said, solve problems at all unless he has first learned the theory and some rules for applying it. Scientific knowledge is embedded in theory and rules; problems are supplied to gain facility in their application". KUHN (1970).

But then goes on to argue that this view cannot explain learning in practice and that:

"In the absence of such exemplars, the laws and theories he has previously learned would have little empirical content", KUHN (1970).

Kuhn expands on this point taking as his example the equation **f = m x a** (Force = Mass x Acceleration). Learning how to use this basic equation is not simply a matter of logical and mathematical manipulation. The expression, on examination, proves to be a "law-schema". The student, as he moves from free fall, to a simple pendulum, to simple harmonic motion:

" ... discovers, ..., a way to see his problem as *like a problem he has already encountered*", KUHN (1970).

Kuhn continues his explanation and ends by summing up his view of learning the process:

"The law-sketch, say f = ma, has functioned as a tool informing the student what similarities to look for, signalling the gestalt in which the situation is to be seen. The resultant ability to see a variety of situations as like each other, as subjects for f = ma or some other symbolic generalization, is, I think, the main thing a student acquires by doing exemplary problems, whether with a pencil and paper or in a well-designed laboratory. After he has completed a certain number, which may vary

widely from one individual to the next, he views the situations that confront him as a scientist in the same gestalt as other members of his specialists' group. For him they are no longer the same situations he had encountered when his training began. **He has meanwhile assimilated a time-tested and group-licensed way of seeing"**

" ... **Scientists solve puzzles by modelling them on previous puzzle-solutions, often with only minimal recourse to symbolic generalizations...**"

"... To borrow once more Michael Polanyi's useful phrase, **what results from this process is "tacit knowledge" which is learned by doing science rather than by acquiring rules for doing it**", KUHN (1970), (Author's emphasis)).

2.2.3 Paradigm Changes - Revolutions

A community of professional scientists will work to a paradigm, conducting their normal scientific activities and will not usually examine the foundations on which their day to day theory and practice are built. As described earlier there is enormous advantage to not regularly digging up the foundations of your subject area. How then does change come about? In SSR Kuhn argues that change comes about after a crisis and a paradigm shift occurs in which the scientific community affected, not only resolve the particular crisis but actually view the part of the natural world which they study, differently; a gestalt change in vision occurs. Kuhn calls these episodes "scientific revolutions which he describes likes so":

"... scientific revolutions are here taken to be those non-cumulative developmental episodes in which an older paradigm is replaced in whole or in part by an incompatible new one" KUHN (1970).

Kuhn elaborated on his description of scientific revolutions in the postscript of the second edition of SSR:

"A revolution is for me a sort of change involving a certain sort of reconstruction of group commitments. But it need not be a large change, nor need it seem revolutionary to those outside a single community, consisting perhaps of fewer than twenty-five people. It is just because this type of change, little recognized or discussed in the literature of the philosophy of science, occurs so regularly on this smaller scale that revolutionary, as against cumulative, change so badly needs to be understood", KUHN (1970).

After the London conference in 1965 Kuhn sought to describe the learning of a paradigm and the gestalt switch in vision which occurs after a revolution, by comparison with learning a language. He pointed out that proponents of competing paradigms do not share a common language, they "talk through" each other. He compares the communication problems to translation of languages: "Translation ... always involves comparisons which alter communications ... languages cut up the world in different ways, and we have no access to neutral sub-linguisitic means of reporting." ... "Parts of learning to translate a language or a theory is learning to describe the world (in) which the language or theory functions." The focus of this thesis on IH and to a lesser extent IT, follows this

dictum. And there is also the problem that terms change their meaning but the words remain the same. Kuhn cites examples such as gravity and electricity. Continuing with the translation analogy Kuhn describes how people transfer their allegiance from one paradigm to another. He argues that: "In the absence of a neutral language, the choice of a new theory is a decision to adopt a different native language and to deploy it in a correspondingly different world." And, as with learning and speaking a language in a foreign country'... one suddenly notes that one is thinking in, not translating out of, a foreign language. At no point was one aware of having reached a decision, made a choice." He goes on to describe this change as akin to a "conversion" via a technique which could almost be described as "therapeutic".

That groups switch allegiance to incommensurable paradigms is hidden from view by the way scientific text books are written. The history of a particular science is always portrayed as if each generation built upon the work of the preceding generation and that there is in some way a seamless connection between the development of what are, in reality, different paradigms. Kuhn likens the historical perspective one gets from science text books as similar to "... an image of national culture drawn from tourist brochures" – a good analogy.

But what brings about change, what leads to revolutionary paradigm shifts?

Unlike Popper, Kuhn does not see normal scientific activity as an unremitting cycle of hypothesizing and falsification. He views it as puzzle-solving according to the current intellectual theory and technical practice of the group. If anomalous experimental results occur or a theory does not predict very accurately it is assumed that the experimental technique was at fault or an elaboration of the theory or perhaps a different slant to the theory would improve its accuracy. And this all takes place within the limits set by the paradigm.

Crises develop usually as a consequence of workings of normal science. There are always anomalous observations or bits of theory in any science but usually these are not regarded as central to the discipline. A crisis occurs when an anomaly, either a long standing or new one, becomes central to a scientific community. It is at this point that an alternative paradigm may become acceptable if it explains the anomaly and allows continued, coherent, theoretical and experimental work in what has become a crucial area for the community. However:

"... crises need not be generated by the work of the community that experiences them and that sometimes undergoes revolution as a result. New instruments like the electron microscope or new laws like Maxwell's may develop in one speciality and their assimilation create crisis in another", KUHN (1970).

Here Kuhn limits the potential external causes of crisis to the wider scientific community.

I will argue later in the thesis that external causes, although ultimately they have to translate their challenge into scientific language, may arise from outside of the wider scientific community. This is especially so in the area of the scientific professions in occupational health which have always been strongly applied sciences that interact closely with other non-scientific groups in society.

2.2.4 Scientific Communities and Individuals

Kuhn defines communities of different sizes ranging from the community of all natural scientists, to the main professional communities such as physicists, and chemists to much smaller groupings of a hundred people or less. In two places in SSR he addresses the affect that individuals may have on the paradigm to which a particular scientific community subscribes:

"Observation and experience can and must drastically restrict the range of admissible scientific belief, else there would be no science. But they cannot alone determine a particular body of such belief. An apparently arbitrary element, compounded of personal and historical accident, is always a formative ingredient of the beliefs espoused by a given scientific community at a given time," KUHN (1970).

"In short, though values are widely shared by scientists and though commitment to them is both deep and constitutive of science, the application of values is sometimes considerably affected by the features of individual personality and biography that differentiate the members of the group", KUHN (1970).

This variability in paradigm interpretation within a scientific group is essential. It stems from the nature of a paradigm and is especially important during times of crises when a variety of explanations is important. One of the variety may very well offer a solution to the anomaly causing the crises without breaking faith with the current paradigm. If paradigms were viewed, incorrectly, as "basic theory', and group understanding and applications of this "theory" was homogeneous then each member of the group governed by the paradigm would develop the same or very similar solutions to every anomaly. Given the routine generation of anomalies by the workings of normal science, the potential for regular and unresolvable paradigm crises is evident. The variability of individual paradigm interpretation is a great strength.

An important strand in this thesis is to examine the paradigm to which the professional scientific communities subscribed which had and have the major influence on the standard setting process in the USA and UK. A significant part of this analysis will be concerned, especially in Part 3.0, with what Kuhn terms the

"... apparently arbitrary element compounded of personal belief and historical accident" (p4) whereby "the application of values is sometimes considerably affected by the features of individual personality and biography" ,Ibid.

2.3 Science according to Karl Popper and Thomas Kuhn

Up to the time of the publication of the "Structure of Scientific Revolutions" philosophers and historians of science presented science as essentially unified and cumulative a gross simplification. Scientists improved upon the theories of scientists before them and science progressed towards a more and more accurate understanding of how the world worked – scientific knowledge was objective knowledge, different in quality from any other knowledge generated by human society. There was much debate about how the scientific process worked, how scientists progressed towards the truth and in the 1960's Popper caught the imagination of many practising scientists see, for instance, Medawar's "Art of the Soluble'. He presented clear arguments against the Baconian empiricist, inductive view of science: That scientists sense the world and somehow assimilate and classify their accumulated perceptions – "the bucket theory of science", as Popper called it. He went on to show that scientists observe the world in a very active way according to preformulated hypotheses, and the scientists formulate their hypotheses within an overall "horizon expectations'. Science progresses via the continual testing of hypotheses. The good scientist is always trying to falsify his/her, or other scientists, theories. In this way science moves forward to closer and closer approximations to the truth via conjecture and refutation, POPPER (1963 and 1972).

From the foregoing description of Kuhn it is clear that, while he would agree with Popper's critique of empiricism, he parts company with him on a number of fundamental issues concerning the progress and workings of science. Differences occur over various issues including.

1. **Scientific practice:** Kuhn argues that scientists practice "normal science' governed by a paradigm which shapes the way they "see the world'; that functions, "when the theory is not there' and yet is also an artefact which can be used as a puzzle-solving device. Normal science, "is normally a habit governed, puzzle-solving activity", not a fundamental or falsifying activity.

   It is because Kuhn's description of scientific practice is a "crashingly obvious fact' and that he "really looked at actual science", that Mastermans can claim "that actual scientists are now increasingly reading Kuhn and not Popper."

2. **Learning to be a scientist:** According to Kuhn people become scientists via a rigid professional training when they learn the paradigm via "law-schema" which he calls "exemplars" and assimilate "a time tested and group licensed way of seeing" and, "what results from this process is 'tacit knowledge' which is learned by doing science rather than by acquiring rules for doing it." In his view scientists are not imbued with the paradigm by learning the theory first and then applying it in practice.

3. **Scientific change and progress:** In Kuhn's eyes science progresses via paradigm shifts, via revolutions which he compares to gestalt changes in vision between incommensurate alternative paradigms and in which conversion is not brought about solely on the basis of logical one-to-one inter-theoretical comparisons because, as he points out, "...we have no access to neutral sub-linguistic means of reporting."

Unlike Popper's view that the continual falsification cycle approximates closer and closer to the truth, Kuhn injects a whiff of relativism into his explanation of scientific progress.

"They (other science philosophers and historians) wish ... to compare theories as representations of nature, as statements about "what is really out there.". Granting that neither theory of a historical pair is true, they nonetheless seek a sense in which the latter is a better approximation of the truth. I believe that nothing of the sort can be found. On the other hand, I no longer feel that anything is lost, least of all the ability to explain scientific progress, by taking this position." KUHN (1970).

2.4 Science according to scientists

Up until 1962 and the publication of SSR scientists saw scientific progress, by whatever mechanism, as essentially cumulative and unified. Scientific knowledge and concomitantly scientific statements, were accorded a special status different to other forms of human knowledge. A scientific fact was the unequivocal truth, uninfluenced by the individuals, the groups or the societies which produced it. After SSR this idealistic view was no longer tenable for many historians and philosophers of science. However, it must be said that many, probably most scientists, despite Masterman's optimism, still subscribe to Popperian notions of falsification and steady approximation towards the truth. What can be said unequivocably is that the bulk of scientists working in the decades before the 1960's subscribed to even simpler views of science as essentially empirical, progressing via induction based as raw observational data. The importance of this understanding will become evident when the workings of ACGIH are examined.

2.5 Scientific professions and professional scientists

Kuhn, like almost all philosophers and historians of science has concentrated on the mature, established sciences such as physics, chemistry and biology, or specialities within these sciences. The messy, more applied sciences have, in the main, been avoided. This is perhaps because they regard such sciences as "not quite science" but it is also probably linked to the role of factors external to such groups which clearly have strong effects on the behaviour and evolution of such sciences. Kuhn himself, while occasionally suggesting that societal concerns may influence the problems which a scientific community addresses lays great emphasis on the important function of the relative insulation of a scientific profession from the rest of the society. He emphasises that there are, "... no other professional communities in which individual creative work is so exclusively addressed to and evaluated by other members of the profession." KUHN (1970). This insulation plus the rigid education based on textbooks and exemplars is a good thing, Kuhn argues, for the proper functioning of "normal science'. As "the very rigidity provides the community with a sensitive indicator that something has gone wrong" (with the paradigm). Thus while admitting the existence of factors external to science, Kuhn's whole emphasis is an intra and inter– community dynamics.

To understand and provide anything like a thorough explanation of the development and structure of IH and IT and the importance of OELs their is clearly a need to extend Kuhn's powerful view of scientific development. This extension must take account of the practical and applied nature of IH and IT. My approach to this problem is to consider IH and IT and the other scientific professional groups in the occupational health arena as not only scientific professions but also professional scientists. It is necessary to consider an approach to analysing professional development which is appropriate.

2.5.1 Professions and professional development

Although there is a literature which examines the formation, function and workings of professions, few authors have applied the insights gained to the field of occupational health. Of those few that have perhaps the best example is the paper of Atherley and Hale which examined the obstacles in the path of the professionalisation of health and safety just after the Health and Safety at Work Act became law in the UK in 1974, ATHERLEY and HALE (1975).

Using the insights of other authors, principally JOHNSON (1972) and MILLERSON (1964) they concluded that: "... without control there is no possibility for the evolution of a clear and recognisable body which can pursue any of the aims we have identified...". And these aims were six elements common to a professional group, namely:

1. Power
2. Competence
3. Moral precepts
4. Voice
5. Advancement of vocation
6. Source to industrial practitioners.

The first three were identified as being attractive not only to the aspiring profession but, perhaps more importantly, to potential clients. The author's then went on to address how "occupational control" could be gained by comparing the development of the safety and hygiene professions with Millersons' "obstacles to professionalisation' of which he identify eight and the author's added a ninth of their own. The obstacles are:

1. Insufficient internal or external pressure to form an organisation.
2. Underdevelopment of subject matter and/or practical technique.
3. Great variation in quality of service provided; training received by practitioners; level of study; type of employment; social origins.
4 Rivalry between occupations and organisations.
5. Small number of practitioners.
6. Geographical isolation.
7. Underdeveloped governmental, industrial or commercial structure.
8. Absence of enterprising individuals.
9. Clear identification of the primary client.

The authors concluded that for health and safety practitioners to become professionals there has to be a demand for their particular mix of skills; the subject has to be coherent and not successfully claimed by old professionals; rivalry between competing groups needs to be minimised, there must be enough people to create a viable size of organisation and enough enterprising individuals; industry needs to be complex enough to warrant specialised professional services, and finally the profession must define who it serves.

The approach taken by Atherley and Hale was functionalist and descriptive. They used the nine criteria as a ruler or checklist against which to measure the success or otherwise of various professional groups in 1975. Ten years later HALE et al (1986) took the analytical technique a stage further. They condensed the nine obstacles into four summary characteristics:

"(1) A developed, complex industrial/employment structure based upon division of labour and producing specialised problems to be solved.

(2) Recognition by influential groups in society that the problems are important and that incompetent solutions to them are dangerous or undesirable.

(3) A well developed knowledge base and practical techniques for the analysis and solution of those problems.

(4) A sufficient number of practitioners with a guaranteed and suitable level of training and competence and an accepted monopoly of the knowledge base and the employment.", HALE et al (1986)

But went further in adopting an original ecological metaphor, "... to try to capture the complexity of the problem which faces the various competing professional and pseudo-professional group." HALE et al (1986). The ecology of professions is described as follows:

"Professions can usefully be compared to biological species struggling to survive in an environment filled with competing species of varying size and specialisation. The driving forces and constraints for biological species competing for ecological niches are the battles for food and reproductive success; in professions they are the battles for membership and control of the knowledge base as they compete for jobs (occupational niches) in the health and safety environment. The environment is a changing one, so there are evolutionary pressures which affect long-term survival. When a new niche is created by a change in the environment (eg. a change in technology or social concern) established species may try to claim the niche as their own or adapt into it. If more than one do so, they will battle for its ownership. Engineering and medicine in particular have done so in relation to the niche called occupational health and safety. However, professions which fit an occupational niche perfectly at one stage may, if they do not grow and adapt with it, find themselves displaced and superseded by more flexible, vigorous species prepared to fit themselves to those changing conditions.

An imperative of ecology is an appropriate degree of adaptation to fit a defined niche. For professions we would define it in terms of meeting the needs of employing organisations in the most cost-effective way. Industry defines its needs in health and safety within the constraints and pressure of statutory requirements and public and state attitudes. This introduces an essential difference between the ecology of biological species and that of professions, namely the niche that the latter are competing for has a subjective as well as an objective element. Industry may not accurately perceive the extent of its problems and their priorities. In particular, it may have its own way of dividing up the field of health and safety into several, perhaps ill-considered, niches. These may be based upon more traditional views of the field and may be strongly influenced by the views held by the existing professional groups.

Powerful and dominant professional species will always try to impose their own subjective view of the niche (adapting the niche to themselves and not themselves to the niche). Thus, the medical profession will always emphasise individual factors, medical examinations and diagnosis, engineers will emphasise technical failure and automating the man out of the system while personnel managers emphasise selection, training and discipline. For traditional professions this is where their interest lies. To adapt themselves well to the real niche would be difficult for a large, established and well-

defined body. It would involve radical changes to training programmes and might even threaten its adaptation to the more important niches at the centre of its ecological range. While it would be possible for a small group from any of these bodies to evolve into the niche, this might only be at the cost their ability to interbreed with (ie. to retain recognition by and pursue careers in) the central stock. This would be the case, for example, if occupational physicians seriously set out to learn environmental control technology, an endeavour which would involve them in learning basic engineering principles." HALE et al (1985).

On reflection the first author now feels that the metaphor ignores the issue of the way regulatory bodies may shape the niche and thus tends to over-emphasise the autonomy and power of the organisational client, HALE (1986). However, I regard the metaphor as a useful way, in addition to the lists of obstacles already mentioned, of gaining insight into how and why certain professions behaved the way they did, when they did.

2.5.1.1 The Kuhnian compared with the Professional Framework

There are considerable similarities between the two frameworks; they overlap. In particular both focus on the behaviour of groups of practitioners and inter and intra group dynamics. Thus, for instance, both frameworks speak of intra-group competition. Kuhn, in questioning Popperian notions of falsification as the basis of change argues that: "Competition between segments of a scientific community is the only historical process that ever actually results in the rejection of one previously accepted theory or in the adoption of another." p.8, KUHN (1970). And, applying the ecological analogy to professions one could view the replacement of professional practices by others, as occuring by a similar process.

A useful way of teasing out similarities and differences between the two frameworks is to consider the obstacles to professionalisation listed in Atherley and Hale's paper. Each will be considered in turn:

1. **Insufficient internal or external pressure**
   Not of direct concern to Kuhn but not contradictory. For a specialism within a science to increase in status the group must take some kind of organisational form and develop a journal, hold conferences etc.

2. **Underdevelopment of the subject matter**
   Where underdevelopment is a fact this could be seen as very similar to Kuhn "pre-paradigm" stage where there is no one dominant and widely accepted paradigm.

3. **Variation in quality and training**

Both the Kuhnanian and professional framework emphasise the importance of training and restricted entry into the group based on qualification. For Kuhn, scientists experience a rigid education based on textbooks, exemplars and practice. Success is judged by a persons ability to apply the paradigm in "normal science" puzzle-solving. Professional education and training is more widely drawn and judged. The ability to puzzle-solve within the main professional "paradigm" is important but other, additional, qualities are important including the ability to inform and deliver what the principal client(s) of the profession require. This direct societal link is not present in the Kuhnian framework.

4. Rivalry

Inter-group rivalry is present in both frameworks where different groups either aim to explain phenomena in the same area or claim competence over the same set of problems. And in both frameworks the rivalry is similar in the sense that the "areas/problems" claimed or defined by competitions are in fact are not the same. Part of the competition lies in the definition of the outline and limits of the problem area. Different groups have special interests in defining the research area or problems in a way which reflect their own special capabilities.

5. Small numbers

This is not so much a concern of Kuhn's. He does not set any particular limit on group numbers. For a profession to start, grow and survive absolute and relative numbers, compared with other professions are important, (see HALE et al (1986)).

6. Geographical isolation

A hindrance but given no prominence by Kuhn.

7. Underdeveloped structure

No explicit concern to the Kuhnian view though very important for the professional framework.

8. Enterprising individuals

A growing profession needs entrepreneurial individuals with vision. In an analogous, though different, way the Kuhnian framework requires the existence of excellent puzzle-solvers and, a different type of person, during crises; people who can produce original solutions.

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PART TWO

THE DEVELOPMENT OF A NEW SCIENCE BASED PROFESSION – INDUSTRIAL HYGIENE

CHAPTER THREE

THE EVOLUTION AND GROWTH OF INDUSTRIAL HYGIENE

The main concern of this thesis is with OELs and their development. The early historical work led to the development of the research framework described in the last chapter. It is not the main focus of this thesis and a precis of some of the relevant main points concerning the evolution of IH and the forces that shaped it are included here. For more detail the interested reader should consult Appendices 1 and 2.

3.1 Early industrial hygiene

In examining the early history of IH there is a definitional problem. Most IHs before the 20th century were doctors and the definition of IH was medically oriented towards the individuals suffering ill-health or disease. The recognition, evaluation and control approach adopted by current IHs is a modern invention.

Looking back in history there is a need to be aware that when authors refer to IH they may mean the current definition but usually they mean the old medically oriented definition.

Knowledge linking certain diseases to certain occupations or substances goes back to Greek times. The development of the factory system, coupled with the use of steam power and the capitalist mode of production resulted in a concentration of people in the same place for long periods. At the same time discoveries in the science of chemistry and their application to production and product formulation meant that factory workers were exposed in a concentrated way to old and new physical and health hazards, never before experienced on a mass scale. The Factory system concentrated people in one place and the division of labour concentrated people in one job. By this process, and the effects of the mechanisation of production, certain groups became highly exposed to poisons which before may have been used on occasions by relatively skilled workers as they manufactured a whole product. Thus links between certain occupations and diseases became clearer and clearer to certain perceptive individuals over the period of the industrial revolution.

Much of the factual history can be found well summarised in TELEKY (1948). He plots the early knowledge of occupational disease and identifies several key figures including Paracelsus, Ramazzini, Thackray and Patissier.

3.1.1 US Public Health Service (PHS) and other institutions

The PHS is one of the oldest institutions in the US state and can trace its origins back to the US Marine Hospital Service in 1798. Initially it was concerned with the health and welfare of seafarers but by the end of the 19th century had expanded its interests to include the prevention of infectious disease. Parallel with this development were various individuals state initiatives which usually revolved round the local Departments of Labour.

In 1912 New York state set up the first Department of IH and in 1913 the PHS followed suit with an office of IH and Sanitation. Another body which was to become important in fostering IH was the Bureau of Mines founded in 1912.

3.2 IH proper starts

"The concept of team work in IH has been built up to the highest degree in America and is so much a part of our method of operation that we are apt to take it for granted and not reflect too much on its importance in the continuing growth of the field." HATCH (1951).

World War I stimulated the development of the IH approach especially in the ordnance factories and ship yards. By the end of the War sanitary engineering, taking an IH approach to controlling health hazards was still small but had become firmly ensconced in the PHS and the Bureau of Mines. After the War there was movement of individuals from the Bureau to the PHS which conducted the first genuinely integrated field surveys relating exposure to health effects. Also the first postgraduate IH course started at Harvard university. 1920 can be seen as a watershed in the development of IH. Most of the founding fathers developed or became converted during the 1920s. Most worked in the PHS or local state Departments of Labour. They came from engineering disciplines in the main, principally chemical and sanitary engineering and they adopted some quite consciously, the sanitarians approach to prevention. Some such as George Whipple (Professor of Sanitary Engineering at Harvard) approached the role of sanitary engineers with an evangelical fervour. Him and a few others converted many, including Theodore Hatch to the cause.

3.2.1 IH expands

By the 1930s IH clearly existed as a discipline separate from medical-IH with a few identifiable individuals practicing it.

Various factors led to an increase in numbers of IHs in the 1930s. These factors include:

(i) Certain well publicised disasters such as the Gauley Bridge disaster where hundreds of men died from an acute form of silicosis.

(ii) A large increase in claims for compensation for diseases such as silicosis and asbestosis.

(iii) The New Deal programme and the associated Social Security Act in 1935 which released funds to increase the number of IHs and train them in local state of government departments.

3.2.2 The Journal of Industrial Hygiene

The JIH was founded in 1919. It was based at Harvard School of Public Health and was edited by Edsall, from Harvard and Collis from the UK Medical Inspectorate of Factories. It rapidly became the international journal of IH. A subject analysis of the contents of the journal from 1919 to 1939 demonstrates the change in definition of IH from medical to non-medical in nature. In 1919 medical subjects occupied 85% of the JIH, by 1939 this had fallen to 35%. The change in type of author was similar, if not so dramatic, medical doctors being 75% of the first authors in 1919 and 40% in 1939, (see Appendix 2 for details). The effect of the various non-medical personnel on the content and definition of IH can be clearly seen; IH was a far more complex and multi-disciplinary subject by 1940 than it was in 1920. The majority of people who worked in the area of IH were not medically qualified. By the mid-1930's recognition, evaluation and control had real meaning.

3.2.3 Industrial toxicology

Another new profession was also evolving in the USA in the 1930s; industrial toxicology (IT). The impetus came from concerns about food adulteration in the early 1900s leading to the first US Food and Drugs Act in 1906. In a similar way to IH, some well publicised disaster such as the Elixir sulphonamide scandal in which over 100 people were killed and the New Deal initiative led to the creation of the Food and Drug Administration (FDA) in 1935. At the same time, various large companies concerned with product liability started their own toxicological testing laboratories. The early industrial toxicologists (IT), some of whom were also practicing IHs (like Smyth and Carpenter) formed what can be seen as an alliance with the new profession of IH. By the late 1930s toxicology represented 20% of the subject matter of the JIH and in 1936 the Journal changed its name to the Journal of Industrial Hygiene and Toxicology – a sign of the times. IT offered the

possibility of recognising potentially hazardous materials before they were introduced into the workplace. More than this, IT also offered the possibility of identifying thresholds of effects and allow the setting of risk free exposure levels. This alliance and mutual aid between IH and IT and the optimistic vision that harm could be predicted and prevented continued into the 1940s and beyond and was very influential in the TLV committee.

3.2.4 The ACGIH and AIHA

By the late 1930s there were enough IHs in government and private organisations to form two professional organisations to represent the interests of the IH profession. In 1938 the National Conference of Governmental Industrial Hygienists (NCGIH) was formed "... to promote IH and sanitation in all its aspects and phases...." NCGIH (1938). A year later the American Industrial Hygiene Association (AIHA) was formed open to IHs working in private companies, consultancies and academic institutions. The formation of these two organisations were the first public signs that the profession of IH had come of age.

The fact that IHs in the public sector felt it was necessary to create a professional organisation separate from IHs in the private sector was almost certainly due to the different needs of the clients they served. There is some indication that members of the NCGIH (later called the ACGIH) were aware of the conflict between what was termed "capital" and "labour" in an early spoof IH training manual (see Appendix 1). Some members are clearly on record as believing that they thought it was important to represent the workers interest, to hold the ring as it were between "capital" and "labour". The early members of the N/ACGIH clearly felt that it would compromise the purpose of the organisation to allow IHs from private companies to be members. The AIHA has never been so exclusive. The effect of clients on the priorities of a professional group could not be more clear.

3.2.5 World War II

World War II had a dramatic and stimulating effect on the size and status of IH as a subject and a profession. The PHS Division of Industrial Hygiene (formerly the office of IH and Sanitation) expanded from a position of having less than 30 staff in 1939 to having over 200 in 1943. Also an additional 60 "industrial engineers, physicians and chemists" had been trained and sent to bolster state activities, BLOOMFIELD (1944).

By the end of the war ACGIH membership had risen to 300 and the AIHA had started a purely IH publication the "Industrial Hygiene Quarterly" which became a Journal in 1957 and, published monthly, continues to this day.

3.3 The ecology of professions and the evolution of industrial hygiene

This chapter is very much a precis of the arguments explored in some length in Appendix 1, in which the various stages in the evolution of IH are examined in some detail. In particular an attempt is made to explain why IH evolved in the USA the way it did and why, for instance, it did not develop in Germany where toxicology was well developed and one might have expected it to. While the rivalry and competition between the other professions laying claim to the field is also explored the importance of co-operation between individuals from different professions within the PHS is also emphasised. The major professions with which the developing profession of IH had to compete, and to co-operate, were industrial medicine and engineering. But for the reasons explored in Appendix 1 IH was able to develop a unique approach to controlling ill-health at work which Hatch later described as ".... a joint field of specialisation" HATCH (1951), which was not an option for the two older professional species.

The sanitary engineers and chemists who metamorphosed into IHs in the 1920s and 1930s used engineering and organisational skills to control exposure and applied chemistry to measure their success. However they went further than most physicians were aiming at the time and set themselves the ambitious goal of preventing chronic disease and linked this goal to the development and use of OELs, primarily TLVs.

In 1948 Frank Patty (one of the founding fathers of IH) published the first edition of what is now a standard reference work, "Industrial Hygiene and Toxicology" PATTY (1948). In the preface he reviews the progress of the IH profession. In the 1920s, "A few farsighted pioneers in the field of public health saw the possibilities of making the control of industrial health a vocation and of enlisting the technical aid of professions other than medicine to do the job." Ibid. He confirms that "IH has successfully withstood abortive attempts at absorption by safety engineering and medicine," IHs two main ecological rivals. He sees such rivalry as productive:

"One of the outstanding reasons that industrial hygiene has been so successful in controlling adverse environmental conditions and in preventing or controlling occupational disease is that pronounced rivalry has developed between the medical men of the sciences, as well as between the scientific professions involved, for instance, chemists and mechanical engineers. This rivalry, as has been pointed out before, has been wholesome and has resulted in advancing accomplishments in the field of industrial health that would have been improbable without it." Ibid

When he comes to review developments in other countries he clearly described his view of the relationship between medicine and IH. He takes as his primary source of information an International Labour Office (ILO) publication concerning the regulation and control of "poisonous materials in industry" in Europe.

"... The rules and regulations in these countries ... are based on the results of periodic medical examinations of workers in plants handling such substances. At certain intervals prescribed by regulation the worker must be examined by a physician. It is the task of the physician to **make an early diagnosis to recognise and evaluate the first signs of absorption of a poison** (original author's emphasis) and if necessary to remove the men from work temporarily. The ILO believes that this is a reliable method for the timely detection of endangered workers and also of the hazards in a specific plant." Ibid

Patty is not convinced and cuts straight to the nub of the problem:

"The shortcomings of such a plan of approach obviously are in evaluating the first sign of absorption – a trick physicians in the United States have not been able to turn to their complete satisfaction. The toxic materials that give rise to recognisable, specific dependable clinical signs in advance of serious injury are disappointingly few." Ibid

As the common concern of the IHs at least was with the prevention of chronic hazards, a different method of recognition was needed apart from or in addition to routine medical examinations. This optimistic project is described by Patty:

"Industrial hygiene has laid aside its swaddling clothes and entered a vigorous stage of advancement. It is no longer seen by industry on the aimless effort of intellectuals collecting bottles filled with nothing ... The safeguarding of industrial health is on a business basis of evaluation and control and is recognised as such by both labor and management. The purpose of the industrial hygienist is no longer merely to "lock the door after the horse has been stolen" but to anticipate and prevent harmful situations, or to control them before serious injury results." Ibid

Earlier in the preface he made a similar point:

"Industrial hygiene procedures have largely passed through the period of enquiry into the causes of ill health and now devote their energies to anticipating and avoiding harmful situations before they have time to cause injury." Ibid.

The "harmful situations" were to be avoided by combining the skills of industrial toxicologists and hygienists to produce OELs. A reminder of the importance of industrial toxicology to industrial hygiene is clear to see in the title of Patty's book.

3.4 IH profession and paradigm

IHs are, like all applied scientists, both professional scientists and scientific professionals with clients who have subtly differing demands. IH evolved in the USA in a specific environment. It addressed the problem of controlling ill-health in the workplace and developed original methods for doing so. The paradigm that the profession forged during its development bears the hallmarks of IHs problem solving methods and priorities and the optimistic vision of its potential for controlling chronic ill health hazards. It is considered in the remainder of this thesis and it is always important to remember where the paradigm came from and under what circumstances it was created.

PART THREE

THE OCCUPATIONAL EXPOSURE LIMIT (OEL) PARADIGM EXPLORED

CHAPTER FOUR

THE EVOLUTION AND INTERPRETATION OF THE OEL PARADIGM

"Men whose research is based on shared paradigms are committed to the same rules and standards for scientific practice", KUHN (1970).

4.1 Introduction

The last chapter was primarily concerned with the evolution of non-medical IH as a step on the way to understanding the OEL paradigm. The next two chapters are concerned with the paradigm itself. The work of six IHs will be examined to build up a picture of how the paradigm evolved and whether it has changed since its inception. Because non-medical IH developed in the USA and that is where the OEL paradigm was forged all the people considered are US hygienists.

A point which is worth emphasising is that as the OEL paradigm is the main focus of this part of the thesis the major emphasis will be on IHs and ITs as professional scientists.

In order not to disrupt the flow of the thesis, and for reasons of space, the early articulation of what was to become the OEL paradigm is dealt with in the text. The details of the analysis of the later articulation, and the response to attacks on the paradigm, are to be found in Appendix 3 which examines the writings of Stokinger, Smyth and Hatch.

According to Kuhn, paradigms are not often explicitly articulated. They are, at the start of a paradigmatic science or, during a paradigm-shift amongst a community of scientists, or when internal or external problems force a re-evaluation or a defence or the paradigm. As Kuhn reminds us paradigms are only debated by the scientific community which adhere to them at certain times. In the case of the OEL paradigm, it started proper in the 1930's and as will be shown developed in the 1940's and 50's. There was a challenge in the 1960's and 70's. The work of the IH's chosen cover these different periods. Some overlap and one covers the entire period from the early days of non-medical IH to the challenge to OEL paradigm in the 1970's.

Because it gives added insight the work of the different IH's is considered chronologically. The development of each individual's ideas can be, and is where appropriate, related to the development of IH and other events and processes. But the analysis of the OEL paradigm itself

throws light on the development of IH. This circularity is inevitable for it is essentially artificial, as Kuhn reminds us, to separate the professional scientific community that subscribes to a paradigm and the paradigm itself. In an attempt to focus on the OEL paradigm the two are separated in this thesis by considering the community and the paradigm in different chapters.

4.2 Selection Criteria

The work of individual scientists has been selected on the basis of number of criteria. Some individuals have been selected on the basis of more than one criteria. The criteria used in the section are:

(i) Whether the individual wrote a lot on the subject of standard setting.

(ii) Whether the individual was active on a standard setting committee or in a standard setting organisation.

(iii) Whether views of the individual on standard setting were prominent in their scientific profession[1] .

These criteria have been applied by reference to the historical description of the creation of the scientific professions from which the paradigm sprang, and the major journals in which a practicing scientist might publish.

There is an unavoidable subjective element in the selection process but the author has made every attempt to identify some of the important figures who have developed, influenced and interpreted the standard setting paradigm, at least as regards the English speaking world. More objective methods based on, for instance, the analysis of citation indices may be available in the future but at present still experimental (see for instance DIEKS and CHANG (1976)).

4.3 The Occupational Exposure Limit Paradigm

The paradigm to which a scientific community works is not often explicitly stated in its entirety. It is only usually visible, at least the bare bones of it, when the paradigm is forged or during periods

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1 This has been judged not simply on the basis of the number of publications a person may have written but upon their influence and status within the scientific community.

of uncertainty in the community when the paradigm has to be defended. Describing a paradigm in such terms suggests that it can be simply written down as a set of basic tenets, as the dogma, to which a particular scientific community subscribes. In practice, to completely describe a paradigm is very difficult because it is more than a simple list of theorems, it is a living, working body of knowledge. The day to day regular work of scientists ("normal science" as Kuhn calls it) consists mainly of puzzle solving, of learning to see a problem as like another usually by reference to examples. To capture the essence of a paradigm it is thus necessary not only to try and explicitly state the fundamental tenets but also to examine the day to day activities of working scientists in their community. It is only by doing this that one can see how scientists deal with contradictions and anomalies[2] and how, explicitly or implicitly they handle these problems and incorporate their solutions within the bounds of what the paradigm will allow. Understanding the nuances of how a scientific community sees and uses a particular paradigm is not simple.

In what follows the OEL paradigm will be revealed by the detailed analysis of the writings of key individuals selected according to the criteria already outlines. The writings and in some cases the recorded views of each individual will be described and analysed by comparing them with a working definition of the OEL paradigm. Once the analysis has been completed it will be possible to give a fuller definition of the paradigm; to describe changes in paradigm with time and to point to individual differences in paradigm interpretation.

4.3.1 A Working Definition

Strictly speaking a completely self contained OEL paradigm does not exist. Part of the foundations of the standard setters paradigm are bound up with (paradigms)/rules which govern the practice of IH, industrial toxicology (IT) and, to a lesser extent, industrial medicine. This is so particularly at the level of "tacit knowledge" which is only revealed by studying the intellectual and technical practice of the scientific professions involved.

It was not until IH and IT took organisational form and set up structures to create OELs that the OEL paradigm implicit in the workings of these scientific professions become more explicit. The

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2 There are always contradictions and anomalies in a scientific field for which there are no coherent explanations (many may not even be noticed until a crisis in the community highlights their existence). But the working scientist continues his or her day to day work on the assumption that the paradigm to which he or she, as a member of the community, subscribes is correct. The implicit assumption is that if the community did enough work on the problem(s) then it could be explained without altering fundamental paradigm.

need to understand the workings of professional scientific practice, in order to get to grips with the real meaning of the OEL paradigm, will show itself again and again in the following analysis.

4.3.1.1 The Paradigm – a Working Definition

A working definition of the OEL paradigm is given below in the form of a series of statements of principle.

(i) Toxic chemicals can be used safely as long as they are subject to appropriate controls.

(ii) Industrial hygiene control methods exist which allow the safe use of all known toxic chemicals.

(iii) "The dose makes the poison" - people can cope with a certain degree of exposure to toxic chemicals and if the dose (the exposure)[3] is kept below a critical level (measured over a working shift, usually defined as 8 hours) then no harm will occur to the people exposed. (NB: there are various definitions of harm including; no harm at all, some effects but not indicative of harm and some short term effects which are recognised but are not regarded as indicative of long term or permanent harm).

Possibly the most fundamental statement as regards this thesis is that:

(iv) It is possible for standard setters, IHs, ITs and industrial physicians, to identify a level of exposure at which little or no harm to health will occur, ie. not only is it believed theoretically that there is a dose level which "does not make a poison", but that practically it is possible to identify this dose level (exposure level).

(v) And finally, the primary factors which are taken into account in setting OELs are the actual and potential health effects of the materials considered. Other factors, if they enter into the process at all, are secondary.

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3 There has been a tendency to confuse these two terms and regard them as synonymous. While measurement of exposure can be a good predictor of dose this is not always so and a variety of factors may have to be introduced to predict dose from exposure measurements in epidemiology "person years at risk" in a certain job category is often used as a surrogate measurement of exposure. In a similar sense exposure is often used as a surrogate of dose. See ATHERLEY (1986) for further discussion of these issues.

4.4 The Paradigm according to Key Individuals

Armed with a working definition of the OEL paradigm the writings and views of some of the key individuals will be examined.

Before embarking on the examination of key individuals it is useful to consider what conclusions may be drawn from such analyses. I take it that the people are not simply cyphers, mouthing the idea and ideologies of their times. They have their own peculiarities, beliefs and visions yet no one can step completely outside the epoch in which they live. Therefore, in describing the intellectual and technical practices of certain key individuals I take these to be, in a loose way, representative of the views of many industrial hygienists, toxicologists and industrial physicians, at the time of publication. To a degree the authors were describing the views which were commonly held by many. In practice the process is developmental; individuals are influenced by past and current ideas and practices and in turn develop their own views and influence the generally held perceptions. There still remains though the problems of how to summarise and analyse the writings of an individuals who worked perhaps over 40 years or more. The following analyses will, I believe, show that people refine their views, that they have contradictory views, that they forget or modify and reinterpret old views and that they response to events external to their individual work and external to the organisations within which they work. Yet people, in a scientific profession, work to a commonly held paradigm which, unless the paradigm to which they work undergoes a revolution, constrains the limits of their changing views. However as KUHN (1970) points out in all paradigm interpretations "the application of values is sometimes considerably affected by the features of individual personality and biography". These "features" will be examined in each of the following analyses and will, I believe, show that although a person's ideas are modified there are usually underlying assumptions which can be identified and which become the individuals repeated themes. These themes have roots, which in some instances, can be traced to the writer's formative years in their profession or in some case to even earlier times.

The following analysis is an attempt to put flesh on the bones of the Working Definition of the OEL paradigm. The method being to examine the work of individuals who were leaders in the scientific communities which developed the paradigm to determine, as far as is possible by this method of analysis, the basis of paradigm, how it was changed with time and how it was defended.

4.5 Key Individuals in the United States

The individuals whose writings will be examined and analysed include:

CEA Winslow*
P Drinker*
TF Hatch*
W Cook*
WP Yant
JJ Bloomfield
HE Stokinger*
HF Smyth*
JH Sterner

* Only these individuals are dealt with in detail.

The pattern of examination will be similar in each case. Firstly there will be a justification of why the person has been selected usually, together with some biographic material. Then the individuals writings will be analysed and their approach to OELs may be compared with the Working Definition of the OEL described earlier or this comparison may be left until the writings of several people have been examined.

4.5.1 CEA Winslow

4.5.1.1 Reason for selection

Winslow was the professor of public health at Yale from ~1910 well into the 1920s. Together with Cecil Drinker he set up the first IH postgraduate degree course in the world. He was a very influential figure in the early development of the public health movement and its concern with the work environment and was a founder member of the National Safety Council, established in 1914. He was one of the first non-medical industrial hygienists at a time when the profession had no organisational focus and the number of non-medical people who practiced IH could be counted on the fingers of one hand. From the standpoint of this thesis he is of particular interest because he was very influential in the setting of two early OELs; WINSLOW et al (1919) and NATIONAL SAFETY COUNCIL (1926).

4.5.1.2 Published works on OELs

(a) Polishing, grinding and sandblasting

In 1919 Winslow published the work he had done with Greenburg and Angemyer who were engineers and both worked in the newly created Public Health Service (PHS) Office of Industrial Hygiene and Sanitation. They had investigated dust concentrations in workplaces using polishing, grinding and sandblasting processes. In the case of the polishing machines they related those levels to the static pressure (Ps) in the throat of the local exhaust ventilation (LEV) hoods. Winslow had no dose response data by which he could relate the dust levels to likely degree of risk and so he set his standard on a different basis. He objects to setting a Ps standard[4] because this would vary with hood shape and size and was too many steps removed from the air movement induced by the LEV and the process which it controlled. He followed the approach adopted by the UK Departmental committee concerned with lead exposure in the manufacture of earthenware and china, GREAT BRITAIN (1910). And this approach recommended a certain minimum air velocity at the process. However for Winslow:

"The only standard which can be altogether satisfactory to the sanitarian, is the one that deals directly with the actual condition of the air inhaled by the worker ... Mechanical standards are convenient and easy of application, but ... what we must ultimately rely upon in the future is a standard that rests upon the number or weight of dust particles actually contained in the air breathed by the worker", WINSLOW et al (1919).

He goes on to quote approvingly of the approach adopted by Higgins and Lanza (Lanza was then Head of the PHS, Office of IH and S) and the Massachusetts State Board of Health (MSBH).

Lanza was concerned with dust in coal mines and arrived at a standard according to the following logic:

"The most reasonable standard then appears to be are based on the quantity of dust that will remain in suspension after the best known methods have been put into use for its abatement", WINSLOW (1919).

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4 Up to 1911 specifications for control had been in general terms. Winslow gives two quotations, "injurious dusts must be removed 'as far as practicable' or 'as far as the nature of the business permits'. After 1911 various industrial commissions and boards gave specifications in terms of a certain minimum Ps in the hood exhaust duct (eg. Wisconsin and New Jersey specified 5 inches water gauge).

The MSBH adopt a similar approach but were more precise in their definition of what they considered "the best known method" to be:

".... In fixing standards of industrial hygiene it was reasonable to require that conditions should be maintained in any industry approximately equal to those already found in the best plants of that industry in actual operation", WINSLOW (1919).

In a similar vein to the Massuchusetts approach Winslow set a standard for polishing workshops of 200,000 particles per cubic foot (less than or equal to 1/4 standard unit particle)[5] on the basis that this could be attained in the better workshops:

"It appears then that the dust content of a polishing shop can be kept generally under 300,000 small $\frac{1}{4}$standard unit dust particles per cubic foot and should not average over 200,000", WINSLOW (1919).

He went on to show that a Ps of ~ 3" in the hood dust on a well maintained system would usually result in a dust concentration of 200,000 per cubic foot. The implication of this statement is that Winslow still expected inspectors to rely upon Ps measurement during routine inspections. He did not, understandably, advocate the routine use of dust measuring equipment which was cumbersome, time consuming and would require skills which were almost certainly not available or widespread at the time Winslow wrote. Non-medical IH was, after all, still in its gestation phase.

The standard Winslow advocated was a practicable standard based upon what could be attained in a well run polishing workshop and in line with the approaches adopted by Higgins and Lanza and the MSBH. Having defined the standard Winslow then went on to argue that the same standard, 200,000 particles per cubic foot, was also practicable for grinding and sandblasting workrooms, even though the average dust concentration for grinding rooms was 353,000. Winslow, in this case, argues that:

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5 $\frac{1}{4}$standard unit particles were in the size range 2-10 micrometers. From earlier post morten work on miners lungs it was known that only "extremely minute" particles remained. None were greater than 12 µm and the great majority were ≤ 1-2µm. Winslow concludes: "It is therefore very possible that particles over 12 microns in diameter are relatively unimportant, and that only those lying under the limit of 12 microns need to be taken into account". Although the exact definition of respirable dust had to wait another 40 years early investigators such as Winslow did appreciate the need to be more concerned about that fraction of a dust cloud < 12 µm in size.

"The failure to comply with the standards suggested on page 445 (200,000 ppcf) is evidently due in this instance to obvious defects in the exhaust system, and it is clear that such a system as that at present installed should be radically reconstructed", WINSLOW (1919).

This is a strange use of a practicable standard and suggests perhaps, that in Winslow's mind, 200,000 ppcf was becoming something more than a practicable standard for polishing shops. The confusion is compounded in that Winslow also suggests a polishing shop gravimetric standard (total dust) of 0.06 milligrams (mg) per cubic foot of air (mg pcf) maximum and 0.03 (mgpcf) average. He goes on then to argue that although the South Africans recommend 0.14 mg pcf and Higgins and Lanza 0.28 mg pcf in mines:

"... It is obvious that the air of a polishing shop can, and therefore should, be kept freer from dangerous dust than a metal mine", WINSLOW (1919).

So the logic appears to be that a practicable standard based on polishing shops can be applied to grinding and shot blasting shops, because it is probably attainable, but it cannot be applied to mines because it is almost certainly unattainable.

(b) Benzene

In 1926 the final report of the National Safety Council's Committee on Benzol was published. The Committee was chaired by Winslow.

The Council first became officially concerned about the use of benzene in 1922, although reports of chronic poisoning had been published since the late 19th Century, and set up a Committee to examine the problem in 1924. The first report, in 1924, was encouraging. Benzene concentrations could be kept < 200 ppm by LEV and preliminary health effect data seemed to indicate that with this magnitude of exposure "the hazard of benzol poisoning could probably be avoided", NSC (1926). However the study only covered Summer conditions, the medical evidence leaned heavily on a questionnaire survey and potential benzene substitutes had not been investigated. A further study was undertaken in 1924, again with the help of Greenburg, seconded for a year from the PHS. He surveyed 18 different work rooms using his own sampling and analytical method[6] and could relate the results to the amount of benzene used and,

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6 As was typical of the time the investigator had to develop his own apparatus. Greenburg invented a method which was simple and effective, "and the apparatus portable and the analysis must be made in a comparatively short time". He reviewed the literature and modified a charcoal adsorption method of collection and analysis. He gives few details in the NSC report but does report a sample volume of ~ 20 litres and elsewhere a preferred sampling rate of ~ 1 liter per minute which implies ~ 20 minute collection period.

more significantly, to the effectiveness of the LEV control applied to the process. The exposures ranged from 70 ppm - 1,800 ppm and the results did tend to be higher in the Winter. Greenburg came to the conclusion that:

"With ideal LEV ... even large quantities of benzol can be used without heavy atmospheric contamination (78A lining, 150A coating and 75B mixing)", NSC (1926).

The results for these workrooms were:

	SUMMER	WINTER
78A 150A 75B	70 ppm 90 ppm 100 ppm	90 ppm - -

When exposures span the range recorded by Greenburg then levels of 100 ppm or less must have seemed low by comparison. The medical survey was also more thorough. In 26 of the 81 workers examined the "blood picture was characteristic of benzol poisoning" and they could relate the number of cases to the degree of exposure. What investigation revealed was however "distinctly disconcerting"

"... Local exhaust ventilation reduced the proportion of workers affected from nearly one-half to less than one-fifth; but even one-fifth remains a pretty high figure" ... We are forced to the conclusion that the control of the benzol hazard ... is exceedingly difficult; that in practice, systems of exhaust ventilation capable of keeping the benzol concentration below 100 ppm are extremely rare; and that, even when this is accomplished, there remains a decreased, but substantial, hazard of benzol poisoning", NSC (1926).

They divided the use of benzol into two conditions; where it was used in enclosed systems, in its manufacture and use in the chemical industry, and, where it was used as a solvent in a range of manufacturing industries. Under the first set of conditions the danger was accidental acute poisoning and in the second "very great danger of chronic poisoning".

The discussion of what policy to adopt towards the manufacture and use of benzol is revealing. The Committee decided that with enclosed systems fatal accidents:

"will no doubt continue to occur ... just as such accidents will continue to occur, from the use of steam boilers", NSC (1926).

And continuing use of this analogy they went on to justify the continued manufacture of benzene:

From the wording of this passage it would appear that "abandonment" had been proposed to the Committee or at least discussed as a possibility by the Committee.

It was not an option they felt able to recommend although it would have solved the chronic poisoning which the NSC survey showed was almost inevitable when benzene was used as a solvent. To deal with this problem the Committee recommended:

"... that all workers ... should be given a thorough medical examination before employment and re-examined, with systematic blood counts, once a month thereafter".

"... that exposure be diminished ... by the most effective LEV", NSC, (1926)

And they went on to recommend substitution of benzene by one of its higher homologues (eg. Xylene or Toluene) as:

"Under ordinary conditions of use ... these substances appear to be relatively harmless", NSC, (1926).

The implications of their conclusions are that they accepted as inevitable a certain number of deaths from acute poisoning during manufacture and that use of benzene would itself cause a substantial number of chronic poisoning and deaths.

The Committee chaired by Winslow, did not recommend an OEL but it did emphasise that exposure could be brought down to ~ 100ppm by applying the best LEV and working practices at the time. The 100ppm practicable limit became the de facto OEL after the publication of the NSC report in 1926. Thus when Cook reviewed the literature on a range of toxic industrial materials in 1945 including benzene, COOK (1945), although he noted that "there is evidence that poisoning has occurred at less than 100ppm", p938, COOK (1945), and that Bowditch and Elkins had suggested a MAC (Maximum Allowable Concentration) of 75ppm, BOWDITCH and ELKINS (1939) he recommends a MAC of 100ppm referring to it as "the American standard". Because of the doubts about this value he recommended efforts should be made to keep exposures below 50ppm. Cook's work was very influential and his review, together with that of the American Standards Association (ASA), was the basis of the first list of MACs published by the American

Conference of Governmental Industrial Hygienists (ACGIH). The ACGIH's first MAC for benzene, 20 years after the NSC report, was 100ppm, ACGIH (1946).

4.5.1.3 Winslow's Approach to OELs

Winslow's approach to benzene was very similar to his approach to dust in polishing shops. The process was investigated, the controls were examined and air sampling was performed. By these measures, which were innovatory in 1919 and were by no means in regular use in 1926, Winslow was able to identify the most effective types of control and state, with some certainty what exposure levels would occur if these controls were applied. His approach was still tied to process specific controls but was supplemented by air sampling from which others, in the case of benzene, went on to derive a general performance standard or OEL. He worked during a watershed between an emphasis on process specific standards and the use of more general performance standards (OELs). This change in approach was prefigured in Winslow's work but was not clear cut. In the case of the dust standard in particular he appears to have been loathe to let go of the connection between the process and the standard. And confusion arises when he then extrapolates a practicable standard for one process (polishing) to two other, completely different, processes.

With the dust standard Winslow had no dose-response data and implicitly set a practicable standard. In the case of benzene there was dose response data, however, interestingly this did not seem to have a large impact on the decision of Winslow and the Committee. They called for the best controls to be applied, which implied an OEL of 100ppm (which implication was picked up by many) and supplemented this with the need for medical supervision as the Committee's own work showed that ~ 1 in 5 people would be chronically poisoned at ~ 100ppm. Because of the proven toxicity of benzene the Committee called for the use of substitutes wherever possible. While the National Safety Council's report is very thorough there is a feeling of passivity about it. Having demonstrated to their own satisfaction that there was a problem even in well controlled workplaces Winslow, and the Committee, did not know what to do with the evidence or if they did they shied away from the implications. There was no call for a ban on the production or use of benzene or restriction of its use to certain relatively well controlled processes. Nor were there recommendations for the design of existing control measured to be improved. Instead Winslow, in effect, fell back on his earlier practicable approach.

There is another important observation to make about the benzene standard which, it will be shown, is a repeating theme in the OEL story. Once a numerical limit is set there is a strong tendency for it not to change and for it to become fossilized, almost reified. In the case of

benzene the practicable limit in 1926 was 100ppm. At about the time the NSC report was published Dallavalle was enroled as a doctoral student at Harvard, supervised by Theodore Hatch to work on LEV design. This work laid the basis of the quantitative design of LEV captor hoods. And together with the efforts of a relatively small group of IH researchers and practitioners improved the IHs understanding of LEV design and process control. By the mid-to-late-1930s the definition of what could practicably be done to control benzene exposure on the same processes Winslow had investigated, had almost certainly changed. The actual OEL, "the American Limit" did not change although various authors such as Cook and Elkins voiced their doubts about it. The 100ppm OEL effectively set in 1926 remained the dominant numerical limit until 1947 when ACGIH recommended a MAC of 50ppm[7] . This inertial effect has occurred with many standards and will be examined in more depth in Chapter 7. It remains to compare Winslow's work to the Working Definition of the OEL paradigm, and this will be postponed until the work of the next two authors has been analysed.

4.5.2 P Drinker and TF Hatch

4.5.2.1 Reasons for selection

Drinker and Hatch started at the Harvard School of Public Health at about the same time in the mid 1920s. They worked together and collaborated on a number of projects until the mid 1940s. Their work and writings up until this time are considered together. From the 1960s only Hatch continued to write, Drinker had retired by this time.

Philip Drinker's elder brother, Cecil Drinker, was the professor of physiology at Harvard and, together with CEA Winslow in the Department of Engineering at MIT set up the first industrial

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7 By the mid 1950's the definition of what could be practically done to control benzene exposure had changed yet again. But this time it was the ASA that lagged behind developments in control methods.

The pattern of inertia is repeated in the American Standards Association (ASA) dealings with benzene. On the 15th January 1941 ASA published a permissible concentration (or Maximum Acceptable Concentration) of 100ppm, the same as the first ACGIH limit. But unlike the ACGIH limit the ASA's MAC took a long time to revise. In a wide ranging article on threshold limits Miriam Sachs had the following observation to make:

"No revisions of this ASA figure have appeared although it is commonly agreed that the 1954 ACGIH value of 35ppm is a far safer working **figure and one that is practical**", p 78, SACHS (1956).

hygiene postgraduate course. Philip followed in his father's footsteps and worked in the newly formed department from the early 1920s for over three decades. He did not work with Hatch initially (Hatch lectured at the Harvard School of Public Health from ~ 1925) but did collaborate with him in the mid-1930s to produce the book "Industrial Dusts" which became the reference book used by hygienists and others when assessing dust exposure in the workplace (a 2nd edition was produced in 1954).

For a resume of Hatch's career refer to the material in Appendix 3.

4.5.2.2 Early Work on Standard Setting

(a) Zinc Oxide

Drinker, as a physiologist/hygienist was involved in developing standards. In the 1920's he performed a range of tests on volunteer subjects, including himself, exposing them to various concentrations of zinc oxide fume for various lengths of time. This was original work for up until this time no-one had developed methods of producing and measuring stable aerosols of zinc oxide or had attempted human experimentation with this substance. Drinker was interested in the dose response relationship between zinc fume exposure and "fume fever" which was known to occur amongst, for instance, brass foundry workers. He summarised his work in a paper published in 1927 entitled: "Metal Fume Fever: IV - Threshold doses of zinc oxide, preventive measures, and the chronic effects of repeated exposures", DRINKER et al (1927). The paper reveals a lot about his attitude to exposure limits.

He starts by reviewing the current literature. Although work on brass founders ague had been done in Germany and was reported by a Russian author in the mid-1920s on concentrations which appear to produce fever, "there are apparently no figures from which one can estimate concentrations that may safety be permitted in industry". Drinker and his collaborators set out to examine the quantitative relationship between fume exposure, exposure duration and symptoms especially temperature rise, taken to be an indicator of fever reaction. The three variables, concentration, time and breathing rate were combined and plotted against temperature rise. Drinker and his collaborators found that they plotted their results: "the points fall along a fairly straight line or zone which crosses the horizontal axis at a finite point". They went on to argue that "the data from all other subjects are grouped, with three notable exceptions, fairly well about the line determined by subject A. Points above the line indicate hypersusceptibility and those below the line hyposusceptibility" (p 339, DRINKER et al, 1927)). The three subjects whose

results do not sit in the "zone" of exposure response are discussed in detail. Two are explained as artifacts due to "over-breathing" (ie. not keeping to the correct experimental regime) but subject "C" is thought to be "unusually susceptible".

Based on this work Drinker states that exposure to 45mg m^{-3} of zinc fume for 20 minutes should not cause "metal fume fever" (see original Figure 4.0 reproduced from the paper below as Figure 4.1).

FIGURE 4.1 (TAKEN FROM DRINKER ET AL (1927))

Taking the data for subject A the average breathing rate and exposure time can be calculated and if there are inserted in the equation given by Drinker they imply a threshold dose (over a 17 minute exposure period) of ~ 85mg/m^3 at a C x E x 1/B value of 170. If the value of C x E x 1/B is taken to be 50 which is roughly where the top edge of the exposure response zone cuts the horizontal axis the threshold concentration becomes ~ 25mg m^{-3}. Quite where Drinker's value of 45mg m^{-3} comes from is difficult to say.

After the experimental work is discussed a recommendation for a limit is made.

"We use a concentration of 15 mg per cubic metre as the threshold limit, because we have found that men exposed for eight hours (8) to concentrations of that order do not ordinarily acquire fever". DRINKER et al, (1927).

The limit is not based on the experimental work on fume fever but on average conditions in the various zinc plants that Thompson, an associate of Drinker's, visited and sampled. It was found

that: "... the most representative zinc oxide concentration was about 14mg m^{-3}" and workers exposed to these conditions "show no acute or chronic illnesses ascribable to zinc".

The authors recognised that although preventing short term exposure to concentrations above 45mg m^{-3} could be difficult for some processes: "In most instances, the fume concentration can be reduced to the threshold level by applying suitable methods of ventilation".

While admitting that fume fever was dose-rate dependent effect Drinker uses the fact that a certain dose of zinc, acquired over a day, does not produce the fever reaction to argue that:

"If workmen do not acquire the fume fever as the result of their exposure, there is no reliable evidence that daily inhalations of about this order produce chronic illness or permanent systemic damage". DRINKER et al, (1927).

Although he recognised that repeated fever attacks may be debilitating quoting a survey of the PHS to justify this observation. The general conclusion is that if the acute effects of zinc can be prevented and average exposure is kept below the "threshold" little or no illness either acute or chronic will occur. This first paper of Drinkers on his practice of standard setting is revealing in a number of ways:

(i) Firstly it is clear from the experimental results that there is a graded response related to time and concentration. However, it is also clear that Drinker has in mind ideas of a fairly sharp threshold of response and sees the underlying dose response relationship as a well defined curve. He focusses on the curve and not on the spread of response around this average line. Inconvenient data are regarded as special and atypical for one reason or another. Above the "line" people are hypersusceptible and below the "line" they are hyposusceptible. Imposing a preconceived model on experimental data is the essence of "normal science" and is how Drinker behaves. An observation of particular interest is the way his belief in dose response curves and the resultant single figure threshold causes him to downplay the spread of the dose response zone. Rather than encompassing the variety of human response he then sees out-riders at a significant distance from the response zone as special rather than part of the spectrum of response.

(ii) The experimental results are dominated by the data from tests on one person, Subject A (this was probably Drinker himself). The results for other subjects tend to be discussed with reference to subject A's response. Reliance on and reference to the results of tests

on one individual disguise the spread of response that might be expected in a population of people exposed to zinc oxide fume.

The experimental method was original and generating the challenge aerosol was a tricky business. These practical constraints may have limited the number of tests which were possible in the experimental series. However, another interpretation of Drinker's approach is that he believed strongly in "normal people" and that this normality existed as a narrow band of response to zinc oxide fume and was a property located in individuals. People tended to be either normal or abnormal in some way, whether they hyperventilated and therefore got a larger dose of fume than normal or were hyper or hyposusceptible to the fume.

This is a view which I impute from Drinker's experimental approach and his interpretation of the test results. Even if my interpretation is too fanciful, and practical constraints were the main factor behind the test methodology the fact is that by performing the tests on a limited number of people and concentrating on the results of one individual Drinker tended to maintain, if not sustain, a normal/abnormal view of the world.

(iii) There is a confusion in the paper, and perhaps this reflects a confusion in the author's minds, over the use of the term threshold. Sometimes it means the short term exposure limit of 45mg m^{-3} (20 minutes exposure time) experimentally determined; at other times it means the 8 hour time weighted average of 15mg m^{-3} which, in effect is the reasonably practical standard which can be attained by industry by applying the then current control techniques. However both are referred to interchangeably as dividing lines between safe and unsafe conditions though they are derived in different ways.

(iv) Finally, there is a belief, stated explicitly, that by protecting against the short term acute effects of zinc oxide fume and keeping to the reasonably practicable standard of 15mg m^{-3} over the workday, that this will protect against any long-term chronic effects that zinc oxide might have.

In many ways this early paper by Drinker can be seen as prefiguring the ideas which became incorporated in the OEL paradigm of later standard setters in the 1940s and 50s.

The next paper considered was a product of joint work between Drinker and Hatch and tries to reconcile the practicalities of control with what was known of the dose response relationship for silica dust exposure.

(b) Granite Dust

In 1930 Hatch, Drinker (Philip) and Sarah Choate published a paper summarising the work they had done on the control of dust from the pneumatic grinding of granite from the mid 1920s onwards (HATCH et al (1930)). During this period J Dallavalle joined the School as a doctoral student and worked under Hatch on exhaust ventilation hood design. Some of this work was incorporated in the dust control project. The need to control this source of dust was stimulated by the findings of a PHS survey of granite workers. The evaluation of dust exposures and assessment of the effectiveness of dust control being performed by JJ Bloomfield, a person who was to dominate the shape of industrial hygiene in the PHS in the years to come.

The PHS team had studied the health of workers in fourteen plants in Barre, Vermont and tried to relate this to their dust exposures. Hatch quotes liberally from the report. He states:

"Investigations of a number of dusty trades have shown that the protective mechanism of the lungs is capable of taking care of a certain amount of inhaled dust. This limit appears to be fairly well defined and varies with the nature of the dust; it is therefore referred to as the threshold dose or standard of permissible dustiness. Evidence of a threshold dose for the granite cutting industry was obtained in the Barre investigation". HATCH et al, (1930).

The study identified a group exposed to 6 million particles per cubic foot of air (mppcf) in which: "there was no indication whatsoever of any unfavourable effects on health"; this group were referred to as Group D. Another group, Group C showed some effects at 20m ppcf. Hatch et al summarised:

"Thus it would appear that a safe limit lies somewhere between the amount of dustiness found in Group C and Group D or between 9-20 mppcf". RUSSEL et al (1929) quoted in HATCH et al (1930).

Hatch comments:

"This proposed standard is of great importance in the present study for two reasons: First, it helps to differentiate the hazardous from the safe processes; and second, it will serve as a criterion of operating efficiency in the development of the dust control system", HATCH et al (1930).

Again drawing upon the previous PHS survey Hatch could show that certain classes of process were already working below the "tentative standard". In this work he concentrated on pneumatic hand tools and surfacing machines which could be enclosed. However he did not address control of dust from lathes or the relatively high average dust count in the "general plant atmosphere".

CLASSIFICATION OF GRANITE CUTTING PROCESSES BY DUSTINESS

Process	Average dust count (million particles/cu ft)
All pneumatic hand tools	59.2
Surfacing machine, inside	44.0
Surfacing machine, outside	44.9
General plant atmosphere	20.2
Lathe	17.9
Polishing mill	9.0
Sandblasting	6.2
Saw	4.6
Office employees	1.9

HATCH et al (1930)

Perhaps he believed that control of the various sources would bring the general plant dust level down.

The experimental procedure was carefully designed so that hood shape and airflow rate could be measured and related to likely dust concentration in the breathing zone of the operator.

The experimental procedure was designed according to Hatch to elicit:

"The minimum air velocity requirement was taken to be the velocity necessary to reduce the dust exposure to ten million particles per cubic foot of air (the lower limit of the tentative standard of the Barre investigation". HATCH et al, (1930).

FIGURE 4.2 (TAKEN FROM HATCH ET AL (1930))

Thus for instance it was found that 200 feet per minute (FPM) was needed for hand tools at the point of dust production to keep the dust concentration in the breathing zone below 10 mppcf. Different hood configurations worked better than others as Figure 5 reproduced demonstrates (see Figure 4.2). Hood No 1F required only $\frac{2}{3}$ the amount of air as Hood No 2 to reach the 10 mppcf "tentative standard". The Figure is revealing though in another way. The dust control efficiency of each of the hoods increases, at different rates for each hood, as the volumetric airflow rate is increased but reaches a plateau in each case. Interestingly this plateau corresponds approximately to a dust level of 10 mppcf. This correspondence between the "tentative standard" and the capability of the control technology may simply be fortuitous; it seems more likely though that the capability of the exhaust ventilation dictated the region in which the "tentative standard" could practicably be set. After all 10 mppcf was at the lower end of the dust exposure spectrum between 20 and 5 mppcf (the average exposures of Groups "C" and "D" in the Barre study) and close to the 9 mppcf which was the upper limit of exposure of Group D. This observation only applies to the pneumatic hand tools. Work on surfacing machines indicates that lower levels, below 10 mppcf, could be reached.

What observations can be made on this paper concerning Hatch's and Drinker;'s views on standards?

The main observation is that the standard is defined in at least 5 different but overlapping ways. It is variously defined as:

(i) Representing a "threshold dose".
(ii) Representing a "standard of permissible dustiness".
(iii) Representing a "tentative standard".

When it comes to control:

(iv) It (the standard) helps "to differentiate hazardous from safe processes".
(v) It (the standard) "serves as a criterion of operating efficiency".

As with Drinker's work on zinc oxide fume there is a tension between these various definitions. Almost as if Hatch and Drinker believe intuitively that a safe dust exposure level exists but also recognise the limitations of the only study from which a dose-response relationship can be

defined. Sometimes they are uncertain about 10 mppcf but at other times 10 mppcf becomes a threshold dose.

This tentativeness becomes less so when the question of control is discussed. The 10 mppcf is a benchwork for the engineer to use in design work and processes are classified as either "safe" or "hazardous". Also there are practical limitations in the capabilities of the then current exhaust ventilation designs and 10 mppcf was really the practicable limit for pneumatic hand tools. The figure of 10 mppcf also had the merit of sitting at the bottom end of the range which Russell et al had recommended and which they recognised could "be obtained by the use of economically practicable ventilation devices applied to the source of the dust", RUSSELL et al (1929), (quoted in DRINKER and HATCH, 1936).

However as in Drinker's earlier paper, ideas about threshold doses, safe and hazardous processes and practicable limits are all mixed up together in an ambiguous soup.

As in Winslow's committee report on benzene there was evidence, from Russell et al's work, that silicosis could develop with prolonged exposure at < 10 mppcf. Drinker himself in a review of the pneumoconioses in 1936 mentions that even at 9 mppcf "occasional non-disabling silicosis" developed. The evidence was sparse and both investigators knew that controlling to the limits, which they had set as their benchmarks, would have a dramatic effect on the more acute forms of the disease caused by benzene and silica dust[8] .

(c) Pneumoconiosis

Drinker reviewed the understanding of how dusts caused lung disease in a paper entitled: "The causation of pneumoconiosis" six years later[9] . In his review of dust sampling methods and their

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8 In 1920 a Dr D C Jarvis, a Barre doctor, persuaded the Trudeau Sanatorium to set up a committee to investigate tuberculosis (TB) and phthisis amongst stonemasons. Of 427 granite cutters all but 28 had definite or probable silicosis and tuberculosis. The TB incidence amongst the cutters was 1095 per 100,000 compared to 96 per 100,000 for the US adult population, (quoted in HOSEY et al, 1957).

9 The paper illustrates how far industrial hygiene had progressed by this time. The difficulty of getting a good measure of the cumulative time weighted average exposure was understood and attempts were made by Bloomfield and others to circumvent this problem by combining short term measurements (which were all that were available at the time) with time and motion studies. Also it was understood that the active (biologically active) fraction of dust in the case of pneumoconiotic diseases was the fine dust fraction (below 3 μm physical diameter). Interestingly Drinker does appreciate that a simple cumulative measure of exposure may not be the least measure of likely harm (ie. silicosis) and that intermittent high dust exposures may be the important parameter. He quotes a South African investigator who suggests that

pros and cons Drinker summarises the importance which was starting to be placed on air sampling results:

"On the other hand, it is well to have records which indicate that the environment is as clean as is desired. In our own experience the ignoring of such samples has raised difficulties in proving in court that adequate dust control had been enforced. **There is no doubt whatever that, in the United States at least, dust samples now have a very definite place in depicting working conditions to compensation boards or to a court.** Under the circumstances, it is very unwise in making surveys to ignore the clean places". DRINKER (1936) (author's emphasis).

This contrasts markedly with the UK where air sampling results were not used in courts until the 1960s and even then only sporadically.

But the most interesting section is the one which deals with "Standards of Dustiness".

Drinker starts by giving his common sense reasoning on the need for standards which is worth quoting at length:

"Under one name or another the plant or mine manager always wants an objective for his dust control program. It serves no useful purpose to evade the issue on the ground that precise figures are not available. Probably they never will be. **The practical man argues, very properly, that if dust is the cause of silicosis there must be some degree of dustiness or of air cleanliness which is safe.** Having been told that silicosis is a disease caused by breathing silica, the practical operating man naturally expects of the physician or hygienist some objective of air cleanliness, call it by whatever name one likes". DRINKER (1936), (Author's emphasis).

And he goes on in the next paragraph to recommend the US PHS survey of the Vermont granite sheds as a basis for a standard:

"In this country the US Public Health Service studies have furnished the only published data we have from which we may suggest dust standards. In the case of Barre granite it was pointed out that a dustiness of 10-20 million particles per cubic foot was reasonably certain not to cause disability". DRINKER (1938).

He also addresses the question of dusts which are of "no proven pulmonary significance" as follows:

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such "dust floods" may be the "deciding factor" but then argues that of necessity "only figures of average dustiness" can be collected. This problem of whether Concentration x Time = Degree of Injury (Risk of) is valid was recently reviewd by ATHERLEY (1986) and it is interesting to see that early investigator like Drinker had some realisation of the limitations of their data.

"What standard should the manager of such a plant take as his objective or need to take any precautions at all? We cannot give him any standards but we can only suggest that he investigate one of the many plants which has reduced dustiness without waiting for any physiological justification. Generally the manager and workmen of a clean plant will uphold eloquently the advantages of dust control". DRINKER (1936)

Finally he quotes a success story of how controlling dustiness can reduce the incidence of silicosis. The example is taken from the South African gold mines and Drinker ends by appealing to the reader:

"It would be hard to devise a more eloquent or complete proof of the advantage of dust sampling and of dust standards". DRINKER (1936)

What does this paper reveal?

It indicates that air sampling and therefore air hygiene standards were become a more mainstream part of occupational health activity and had become accepted in some courts and by some insurance companies as evidence of adequate or inadequate dust control in the USA. Airborne dust standards were not now simply the province of industrial hygienists, doctors and engineers in the PHS and a few universities, they had moved into the public domain[10].

By the time Drinker wrote his paper in 1936 neither he nor Hatch, nor other hygienists/doctors could afford to be so relaxed or vague about the status of a specific number standard. There was less space than in the 1920s, there was more at stake, courts and industry and the hypothetical "mine manager" wanted to know where the "threshold dose" lay.

Using the example of a "mine manager" Drinker appeals to commonsense notions of dose response ... "if dust is the cause of silicosis there must be some degree of dustiness or of air cleanliness which is safe". The Barre study in Vermont and the 10-20mppcf bracket within which a "tentative threshold" possibly lay now becomes:

"reasonably certain not to cause disability".

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10 This paper fits in nicely with the background described in my early material on the evolution of IH. Both IH and air sampling had come of age by the mid 1930s, also silicotic victims were suing their employers in increasing numbers and local state schemes were having to pay out compensation. Remember, the Air Hygiene Foundation (set up and financed by industry) was formed primarily to help industry to handle compensation claims for silicosis. Note also that both Drinker and Hatch were involved with the AHF (later changed to IHF) from its very earliest days as scientific/technical advisers.

Drinker avoids specifying a particular dust standard unlike Hatch had done in 1930 when he worked to 10mppcf. At least he (and Hatch) know this was an attainable limit in the granite industry, it was not possible to be so sure for other industries. And very probably the "practical operating man" did not just want some objective standard of air cleanliness he wanted a feasible standard, a level that could be attained in his industry.

The courts and the insurance companies wanted clear cut threshold doses to work to, to simplify decision making but the "practical operating men" also wanted standards they could work to as well. Perhaps this explains Drinker's reluctance, in this paper, to nail his figure to the mast, he knew the two demands were irreconcilable. But he also knew that dust was dangerous and that controlling exposure worked; by doing so the more virulent forms of silicosis could be reduced, the evidence from South Africa demonstrated this[11]. He knew first hand what uncontrolled silica dust exposure could do and must have personally seen and met many victims. He therefore calls for a general, what would nowadays be called a "precautionary policy", to reduce exposure to any dust even those of "no proven pulmonary significance".

This paper, as with the 1930 one with Hatch again demonstrates the tension between wanting, and believing, a safe or threshold dose must exist and the problems of reconciling this idea or goal with current and potential conditions in industry, ie. all industries where silica dust is released or generated.

It would appear then that using the phrase "eloquent ... proof" which Drinker cites in his 1936 paper he means that there is some certainty that third-stage silicosis can be eliminated and he lays this vision before his audience. His reservations that second and first stage silicosis may still

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11 Drinker cited the South African experience again in the same year he published his paper on pneumoconiosis, DRINKER and HATCH (1938). He quotes the chairman of the Miner's Phthisis Medical Bureau, but this time also offers his own view: "No 'New Rand miner' who has entered the industry since August 1923, ie. $10\frac{1}{2}$ years ago, has as yet contracted silicosis". Drinker comments: "Of course freedom from silicosis during a working period of $10\frac{1}{2}$ years is not proof that the hazard has been eliminated. But there is no doubt at all that the severe or rapid silicosis which gave the Rand mines their bad name has been wiped out. The engineers job in controlling dustiness continues ..." (DRINKER and HATCH, 1936). Drinker earlier on in the book reviews the various categories of silicosis dividing them into three stages, the third stage being the worst. He again comments that "third-stage silicosis described by Lanza (et al) ... seem to be disappearing ... and in their stead are being featured in the literature the cases more difficult to diagnose ... There is no doubt at all that the rapidly fatal third-stage silicosis has virtually been eliminated in the South African gold-mining district", DRINKER and HATCH (1936).

remain a problem are not included in the paper but are mentioned in the book he co-authored with Hatch. The lack of reservations in the paper give it an optimistic feel. It almost sells "permissible limits" ending on a rather over optimistic high note: "In defence the use of standards of dustiness the experience in South Africa where such standards have been applied successfully for many years is cited", DRINKER (1936).

(d) "Limitations in Standards of Permissible Dustiness" (DRINKER and HATCH, 1936)

Drinker and Hatch specifically address the limitations of OELs in the first edition of their book, "Industrial Dusts" which was to become the reference book for IH's and others in much of the English speaking world. Earlier in the book they quote approvingly the UK view, (and to a large extent therefore the British Empire view), on permissible dust concentrations:

"... Such standards may be of value in estimating the relative efficiency of the methods used for suppressing dust in a single industry, but it would be wrong to adopt any of these standards as the measure of safety under different conditions and in other industries", DRINKER and HATCH (1936).

In what seems peculiar in retrospect Drinker and Hatch appear to have substantially agreed with this position. They identify three reasons why people should be circumspect about permissible dustiness:

1 Mineralogical Composition

"It is not possible ... to compare two dusts with respect to their silicosis-producing capacities on the basis of their silica content without considering also the variation in the amount of free silica with respect to size", DRINKER and HATCH (1936).

2 Dust Floods

They argue that the dust concentration must be above a certain minimum for harm to occur and therefore a sudden "dust flood" may be very significant. They quote Clark and Drinker "... a sub-threshold stimulus (dust inhalation) for a long time produces no reaction whereas a relatively brief super-threshold stimulus may cause a reaction", CLARK and DRINKER (1935). From a contemporary perspective the existence of sub- and super-thresholds would be regarded as debatable or at least substance dependent. It does throw additional light on Drinker's approach to toxic substances and is additional confirmatory evidence of the conceptual view he brought to bear on the evidence.

3 Industrial Selection

They quote Brundage who pointed out that an "industrial population ... is composed of individuals who are ordinarily able to engage in wage earning occupations, whereas the general population includes "invalids and persons with physical impairments so serious as to preclude attempts at factory employment", [12] .

They continue to amplify this point:

"The way in which industrial selection limits the value of the standards of permissible dustiness is obvious. The studies must be made upon the men at work with little or no consideration of the condition of those who quit. Hence, the resulting standards are based only on the reaction of the men after selection of those best fitted for exposure to the dusts under investigation", Ibid.

They illustrate the effect by quoting two studies, one on cement workers and the other on lead workers. For the cement workers, "the respiratory disease was 68% higher among those who quit during the progress of the study than among those who stayed at work". And for the lead workers there was a "high rate of turnover, especially in the departments with the greatest lead exposure", DRINKER and HATCH (1936).

The first of the three reasons for being circumspect argues for care being taken in extrapolating one silica dust standard based on a certain industry to another industry and echos the UK Home Office's view. The second would appear to be arguing against the time-weighted-average (TWA) as a measure of dust exposure and risk without additional information on sudden high exposures. And the third argues that selection will distort the true prevalence of a disease, to a greater or lesser extent, and will lead to an under estimation of the risk at the particular permissible dust level derived from such studies.

These three reasons are in addition to their more fundamental doubts about standards of permissible dustiness which they outline as follows (all quotations are from DRINKER and HATCH, 1936).

"The idea of adopting standards of permissible dustiness for each harmful dust has a medico-legal appeal that is not all justified by the data available today".

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12 Although they did not use the term they had identified and were referring to what would nowadays be referred to as the "healthy worker effect" well known to epidemiologists.

It may not be justified but it is too tempting for many to resist and was explicit in Drinker's paper on pneumoconiosis of the same year and his earlier work on zinc oxide fume. He and Hatch knew the limitations of the data but by this date the New Deal era was building up to a head of steam, there was pressure for some legal controls on working conditions. This pressure was in addition to that generated from the compensation claims which were coming through the pipeline to manufacturers and their insurance companies. Drinker and Hatch acknowledge the latter's influence and are still unhappy with the result:

"Having had no compensation laws covering silicosis until recently, certain states are now drafting regulations defining permissible dustiness in precise figures which will not stand the most elementary analysis ... In none of the original studies was there a single suggestion that the threshold figures were usable as legal standards", Ibid.

Although this may be (and was) the case they did understand why people did quote limits but they did not approve:

"In spite of this (the paucity of the data), there is a strong temptation to plot a curve with permissible concentrations and content of free silica as co-ordinates and then to interpolate or extrapolate freely from this curve to obtain the standard of dustiness in any industry. We cannot admit the soundness of this practice from a medico-legal standpoint ...", Ibid

However there were some legitimate uses for the "threshold figures".

"... On the other hand, there is much to recommend the use of figures thus obtained as basic criteria in the design and operation of dust-control equipment and as general guides in the appraisal of working conditions".

Hatch and Drinker had used the figures supplied by the PHS field surveys as a basis for their criteria in the design of LEV hoods to control granite dusts, and they could see the obvious appeal of this approach to "the practical man". "Threshold figures" could be used as benchmarks in the design and development of "methods used for suppressing dust". Drinker and Hatch and the Factory Inspectorate in the UK would appear to be as one on this issue. They would, at first sight, also appear to agree that such figures should not be used in any other way, they should not be used to specify permissible dustiness levels especially if there were then made legal standards. However there are two contradictions in the last quotation, one minor and one major, which indicate that Drinker and Hatch and the UK approach had in reality parted company.

The minor contradiction is that figures used as benchmarks in design which have some connection with risk of ill health are not simply engineering standards. They are bound to take some note of the health implications of the surveys from which they are drawn. But the major contradiction is

contained within the phrase "general guides in the appraisal of working conditions". If the data are so unsound how can they be used to provide even "general guides"? And if in fact general guidance levels can be derived is it not inevitable that others, not so close to the data, will come to regard the "guides" as permissible limits, and even enshrine them in legislation! This was obviously Drinker and Hatch's concern but it goes against the grain of their own arguments presented in this section and sits strongly with Drinker's own views stated quite boldly in his paper on pneumoconiosis published the same year as the book:

"... If dust is the cause of silicosis there must be some degree of dustiness or of air cleanliness which is safe", Ibid. DRINKER (1936).

While this is Drinker's version of a commonsense view, in the context of his paper it is fairly certain that this was Drinker's view also. The view that permissible limits should not be set also appears to be contradictory on Hatch's part. In his work on granite dust control, with Drinker, although the term limit has a number of definitions (as was discussed earlier) he quite obviously believes in threshold limits and that such limits are fairly well defined. Such limits helped "to differentiate hazardous from safe processes" and served as criterion of operating efficiency ... (for) dust control systems".

The various overlapping yet contradictory views on permissible limits, that Drinker and Hatch expressed in the 1920s and 1930s, relate to their own development and application of various concepts linked to exposure and harm on the one hand and also to methods of control on the other. They were scientific professionals wrestling with the very real problems presented by relatively uncontrolled exposures to toxic substances in the workplace, and having to justify their actions. But ultimately the confused usage, evident in the papers that have been dissected, results I would argue because Winslow, Drinker and Hatch all operated during the period before the scientific profession, which accepted the full blown OEL paradigm, existed. All three individuals pioneered the concepts and practices (intellectual and practical) which would be incorporated in the paradigm but were actually working in what could be termed a pre-paradigm period[13].

As the views of these pioneers strongly influenced the practice of the new scientific profession of IH and the OEL paradigm it is worth considering what they brought with them as founders. It is also instructive, in the case of Drinker and Hatch, to consider how their views changed by the

13 As was outlined in Chapter 3 the 1940s was a transition period as the scientific profession of IH (and toxicology) established itself and consolidated its organisational form and grew in professional influence. The ACGIH rose to its ascendant position over this time and is considered in some detail in the next chapter).

time they came to publish the 2nd edition of their book in 1954. And finally, it is an appropriate point at which to compare the work of these three people and compare it with the Working Definition of the OEL paradigm outlined earlier.

Chapter 2 reviews the development of industrial hygiene and touches on the parallel development of toxicology where this is relevant. Our understanding of the approaches of Winslow, Drinker and Hatch can be given added insight by considering the context of their work, what their work actually consisted of and what concepts they brought to that work[14] .

Winslow and Hatch were engineers and Drinker was a physiologist with a strong experimental bent. Both Winslow and Hatch brought with them, from public health, and sanitary engineering in particular, a vision of controlling health hazards in the workplace by engineering means. Drinker was interested in this approach but was also concerned to understand the aetiology of occupational diseases. The sciences they were trained in were strongly quantification oriented and it was probably second nature to Winslow to apply quantitative principles to occupational health. He was one of the first people to advocate the use of air sampling as a method of assessment and he did this because his goal was to reduce disease by controlling the condition of the air inhaled by the workmen. Air sampling with the major focus being on the quality of the workplace air were two important innovations introduced by sanitary engineers such as Winslow.

All three were practical men who wanted to a greater or lesser degree to bring about improvements in workplace conditions. Their interest was in providing practical solutions and practical approaches to industry and all worked closely with either industries or individual companies to promote change. Their approach to occupational health gave them insight into the conditions which caused disease and the methods which could be used to control exposure. They all started at the control end of industrial hygiene. In addition Drinker and Hatch appear to have believed implicitly in threshold doses and they brought this concept and the ideas surrounding it to their work. It is of interest to speculate on where Drinker and Hatch got their ideas from. Ideas concerning threshold are based on concepts such as homeostasis, compensation and repair and these in turn can be seen as derived from medical pathology. Such ideas were almost certainly current within the institutions in which Drinker and Hatch worked and in the case of Drinker were almost certainly part of his early undergraduate training.

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14 The importance of the "tacit knowledge" which is embedded in the day to day intellectual and technical practice of professional scientists is emphasised by Kuhn in the postscript to the 2nd edition of his book "Structure of Scientific Revolutions" and is summarised in in Chapter 2.

Of the three, Winslow was the most mainline engineer. The standards which he and his co-workers derived were essentially practicable limits. For Drinker and Hatch sometimes the concentrations were permissible limits set to reduce or prevent harm and sometimes the same concentration was a benchmark for the designer or a measure of good control practice. This ambiguous use of the same concentration figures is evident in all their writings. They too felt uneasy about the derivation and use of "permissible dust limits" and tried to demonstrate the problem in the first edition of their book. However, as was shown earlier, the ambiguities remain even in this discussion. These contradictions can be interpreted as follows. As practicing non-medical IH's all three aimed to discover what practicable dust controls meant in terms of exposure and, in the case of granite dust, two of them did a lot of the fundamental work into the controls. They used field survey results from the PHS to indicate which ball park they should be working in but the actual benchmark level was in all cases the practicable level which their work identified. Acute occupational disease was still rife in the USA when these three men started their careers. The practicable limits which they specified, or in the case of Winslow and benzene, implied, would control the more acute effects of exposure, and they must have been fairly confident of this fact. In this sense then Drinker and Hatch in particular could believe that they were working with "threshold values". They appear though to have carried the definition much further than simply controlling the more acute forms of disease. Their practicable levels sometimes became "threshold values" or "permissible levels of dustiness" which, from the preconceptions they brought to the subject, they knew must exist. The wish to increase the status and extend the definition of the limits with which they were dealing and make them of more universal application is evident. The confusion and ambiguity present in their writings derives from them attempting to reconcile the practicable limits which they worked with to the threshold values which they knew must exist.

In 1936 there was still the intellectual space in which Drinker and Hatch could ruminate on the wisdom of permissible limits. This space, for Drinker and Hatch and others, was soon to contract due to pressures internal and external to the developing IH profession. External pressures derived from the New Deal initiative and the way this raised occupational health higher up the Federal state agenda, the increasing financial pressure of compensation claims and the activities of the new local state IH units which began to blossom at around this time.

By 1954 when Drinker and Hatch published the 2nd edition of "Industrial Dust", their attitude to "permissible dust levels" had changed. They were less critical but still had reservations.

Their concern at the extrapolation of standards to all processes using a certain material, especially where silica dust was concerned, and the use of time weighted average measures of exposure has gone[15]. Indeed they are full of praise for the work of the two organisations which have contributed most to the standard setting process:

"We have great respect for the data on threshold concentrations of various dusts derived from the series of field studies by the US Public Health Service over the last 30 years ..."

The most useful lists of permissible concentrations of various substances are those published annually by the ACGIH", DRINKER and HATCH (1954).

But they acknowledge that standard setting is far from an exact process:

"Generally the figures used are the results of the experience and opinions of qualified persons"

and go on to quote Schrenk:

"The concept is that of the art, rather than the science, of industrial hygiene, DRINKER AND HATCH , (1954)

By 1954 IH had become a recognised scientific profession and, OELs formed a central part of the professions intellectual and technical practice. By this time medicolegal permissible dust limits were no longer the anathema they had been to Drinker and Hatch in 1936. Indeed their conversion to the concept probably took place almost as soon as the first edition of their book was published. Drinker's paper of 1936 is indicative and by 1946 he was prepared to put his name to an industry wide silica dust standard of 20 mppcf (for dust with a free silica content of 5-50%) in the ACGIH's first MAC list. He was instrumental in compiling the list, being one of the five people to comprise the first ACGIH sub-committee on threshold limits, ACGIH (1946).

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15 They withdraw their objection to time weighted averages (TWA) on purely pragmatic grounds. It is still acknowledged that; "A concentration of 40 mppcf in a granite shed, for example, is probably more than twice as dangerous in one of 20 million. In the quarry ... the dust flood produced by blowing off drilling holes is undoubtedly more serious than is indicated by its proportionate number of particle hours" (DRINKER and HATCH, 1954). But the cumulative day long and working life time approach, built up on the basis of time and motion work study and average dust concentration per process, is seen to have great advantages over "dust concentration data alone". For silica dust the cumulative life long exposure is seen as the best predictor of risk and the process concentrations tell the IH where to concentrate dust control. It is not clear how this rationalisation deals with their "dust flood" objection to TWAs but the practical problems of measurement of exposure required to deal with a dose rate dependent effect have always been too daunting for the sanitary engineer and the IH and Drinker and Hatch more or less admit this in Chapter 7 of their book entitled "Appraisal of Dustiness".

The reality of this standard in terms of the difficulties encountered by some industries in meeting it and the protection from silicosis it offered were well described a decade and a half later at an annual ACGIH meeting:

In 1962 the ACGIH adopted the silica dust formula. In the discussion which took place before the Conference voted Mr Asle (Vermont Department of Health and Chairman of the Conferences Committee on Dust Evaluation) made the following comment on the protection of the 20mppcf standard:

"... and I would first hate to see you go back to a range of values from five to fifty percent with a 20mppcf limit, because this is not sufficient to prevent your men from getting silicosis. I have an x-ray file full of x-rays of men who are dead that will prove twenty million particles of granite per cubic foot of air is too much for them to live and work." ACGIH (1962).

He also had experience of the control and health problems in the slate industry:

"... that industry accepted that (the 20mppcf TLV) and they fought like hell to reduce it even to twenty. And we have a sanatorium that has many men who have died of silica tuberculosis because of a MAC that was too high." ACGIH (1962).

The attitude of engineers to standard setting is explicitly dealt with and is quite revealing:

"Lacking epidemiological data, engineers may have to set their own figures, remembering that the ultimate criteria of the healthiness and safety of the workplace are the health and safety of the men", DRINKER and HATCH (1954).

In the same way that Winslow set practicable standards 35 years earlier, if there was no dose-response data on health effects, and in the case of benzene, even if there was, so Drinker and Hatch, as engineers, recommended this fall back position. But they also touch on the age old dilemma that they wrestled with but did not answer in 1936. What do you do if the data indicate some health effects at or below the exposure levels which you know to be practicable for industry? Drinker and Hatch more or less give their answer in their discussion of the development of the silica dust standard. While they are making a candid comment on the motivation of one of the first PHS survey reports the comment could equally well be applied to their approach to threshold values in the 1940s and 50s.

The PHS team recommended a limit of 1mg per 100 litres of air. Drinker and Hatch comment:

"This means that these men, experts in their field, thought in 1917 that a limit of 10mg/cum of dust, high in quartz, was adequate. **But the real reason they suggested 10mg was probably because they could attain it rather than because it would ensure freedom from silicosis**", DRINKER and HATCH (1954) (Author's emphasis).

4.6 Winslow, Drinker and Hatch and the Paradigm

Winslow, Drinker and Hatch were some of the earliest pioneer figures of the IH profession and amongst the earliest standard setters. The approaches they brought to the new profession were derived from their training in the engineering and biological sciences, the work of other pioneers, most of whom worked in the PHS, a few small university departments, and their own special personal slants. The methods they developed and the ambiguities they wrestled with in trying to reconcile controlling exposure to, and deriving exposure standards for toxic substances, were imported into the profession when it was founded and set up its own formal structures. And more importantly from the viewpoint of this thesis, these ambiguities were incorporated in the OEL paradigm.

All three authors started work before the profession which promulgated OELs was created and before the paradigm which underpinned the setting of OELs was consolidated. They worked in a pre-paradigm period and it can be seen in their writings. None of them spoke in general terms about permissible dust limits, indeed Drinker and Hatch specifically warned against doing so. All this was to change with the creation of the ACGIH and the AIHA. Be that as it may, in the writings which have been examined the authors did not subscribe to all the statements in the working definition of the OEL paradigm. In summary these were:

1 Toxic substances can be used safely.
2 IH control methods are adequate for all toxic substances.
3 Threshold doses exist for all toxic substances.
4 Standard setters can identify threshold exposures (doses).
5 The major consideration in the setting of OELs is the health effects of the toxic substances.

Of these five statements the three authors would have probably subscribed to 1-3, for certain substances to 4 and would have given an equivocal answer to 5. It would appear that Winslow, Drinker and Hatch, in the 1920s and 1930s did not subscribe to the full blow paradigm. They worked to a set of less universal, more pragmatic beliefs. In doing so they provided the exemplars used in the teaching and training of the next generation of IHs and help lay the basis of

what was to become the OEL paradigm. The foregoing analysis has teased apart and examined the content of those exemplars and in doing so has revealed what the profession 'bought' when it took these exemplars to its heart; in one word, it bought confusion.

CHAPTER FIVE

THE OEL PARADIGM – A DISCUSSION

5.1 The Development of the OEL Paradigm in the USA

In the early days of the 20th Century the scientific professions of toxicology and non-medical IH did not exist. Physiologists and biological-chemists worked on what would now be called toxicological problems and German scientists in particular had investigated the acute effects of some vapours and gases on animals and humans, mainly experimenting on themselves or their support staff. Industrial hygiene as a speciality of medicine was only just getting off the ground though by the time the first decade of the century had passed various organisations had been created which fostered medical IH and offered courses in the subject, usually as part of a wider public health course. From the turn of the century up until the 1920's there was no group with the appropriate orientation to develop and sustain the OEL paradigm. The medical IHs tended, with notable exceptions, to focus on the individual and they were concerned, not only to develop methods of differential diagnosis so that occupational diseases could be unequivocally be identified, but also with increasing their recognition within the medical profession as a whole. The control of the work environment was left to non-specialist engineers and the technology of control was crude, haphazard and under-researched. Through the 1920's and 1930's all this changed as two new, small scientific communities evolved, non-medical IH and industrial toxicology. The former group developed out of an amalgamation of medical IH and sanitary engineering and was concerned with controlling workplace exposure to within certain limits. Non-medical IH was a numerate profession organisationally based in the US PHS and State Boards of Health or Departments of Labour. It developed methods of control and used air measurements to validate at least some of the new designs. The evidence showed, by the mid to late 1930's that if exposure to siliceous and lead dusts in particular, could be brought under control, within certain broad limits, occupational disease could be prevented. The new non-medical IH profession worked in small federal and state organisations which had no right of entry to workplaces and from the start adopted an approach of working with the industries it studied; usually trying to persuade them to adopt the new control measures or more usually to cajole the majority of an industry to adopt the control practices of the better run companies. The few limits that were promulgated at this time were billed as both adequate to control occupational disease and reasonably practicable. For some authors there was no problem in reconciling these claims whereas others had more difficulty and this difficulty showed itself in the confusion of claims in

their writings. Thus Sayers and Dallavalle in a review of the work of the PHS including its "field surveys" could claim:

"The Public Health Service in all its field investigations has attempted ... to establish what may be considered the **safe conditions** under which workers may be exposed **indefinitely** without injury to health" SAYERS and DALLAVALLE (1935) (Author's emphasis)

Whereas Drinker and Hatch a year later in their seminal book were far more careful in their claims for the standards recommended by various states.

Underlying the activities of many of the medical and non-medical IH's was the belief, often unstated that there should be a level of exposure which was essentially "safe" and it is evident that individuals came under pressure from "practical men" as Drinker put it to specify what this level was, (see DRINKER (1936).

In the 1920's and 30's the authors reviewed tended to oscillate in the claims they made for the limits they developed or used. This circumspection and careful qualification changed in the late 1930's. A rash of limits and limit lists were produced including BOWDITCH (1937), ELKINS (1939), DRINKER (Cecil) (1939), BOWDITCH et al (1940), ASA (1941 onwards), STERNER (1943), US PHS (1943), COOK (1945) and ACGIH (1946). Some of the lists were qualified others were not but they all showed one characteristic in common. All the limits quoted or recommended were single figures and usually referred to an average concentration over a working day. This was not simply a change in style of presentation. Something had happened between the early and late 1930's. In the 1920's and early to mid-1930's most lists of limits were in the form of columns which related exposure duration to the maximum concentration which could be tolerated. Only the last column dealt with prolonged exposure. Thus Sayers and Dallavalle have five columns in their table labelled: Kills in very short time, Dangerous for 1/2-1 hours exposure, Maximum concentration for exposure $\frac{1}{2}$-1 hour, Amount causing slight symptoms after several hours exposure and, lastly, Maximum allowable concentration for prolonged exposure. And the change in style was not simply because the limits in the late 1930's/early 1940's were concerned with materials which only had chronic effects. There is much commonality between the lists, its simply that the later lists omit the first four, in Sayers and Dallavalle's case, columns. The most obvious and likely explanation for this outward change lies in the development of non-medical IH and industrial toxicology. The former took organisational form in the late 1930's/early 1940's and supported, and in turn was supported by industrial toxicology. The toxicologists in the shape of people like the Smyth and Carpenter not only believed that by means of animal studies they could understand the toxicology of a material but that they could also, by means of these same

studies, identify safe levels of human exposure. And in the case of Smyth (Junior) and carbon tetrachloride the hygienist in him was convinced that the chosen level was also practicable. Patty's quotation from the first edition of his book sums up the enthusiastic vision of some of the early IH's in the new profession.

"Industrial hygiene procedures have largely passed through the period of enquiry into the causes of ill health and now devote their energies to anticipating and avoiding harmful situations before they have time to cause injury", PATTY (1948)

And Elkins in the same year explicitly makes the claim that MAC's are set at the bottom end of the dose response curve (see Figure 5.1, ELKINS (1948) reproduced below)

FIGURE 5.1 ELKINS VIEW OF MACS

Somewhere between the mid 1930's and the early 1940's the OEL paradigm crystalised in the minds of non-medical IH's and industrial toxicologists. Kuhn makes the important point that in order to understand a science it is important to locate the "responsible group or groups" because: "A paradigm governs, in the first instance, not a subject matter but rather a group of practitioners", KUHN (1970). It is my contention that before the late 1930's there was no group to develop and promulgate the OEL paradigm and therefore there was, in reality, no paradigm. There were individuals like Drinker, Hatch and Smyth who used the concepts but they and their compatriots, who came together to give IH organisational form in the late 1930's, were isolated and unsure until their professional scientific community coalesced. This is not to say that the OEL paradigm simply snapped into place in the minds of non-medical IH's when the ACGIH and AIHA were created. The foundations of the intellectual and technical practice of the non-medical IH's goes back much further into the history of the US PHS and other organisations as explored in Chapter 2.0. A more accurate way of understanding what happened would be to see the 1920's and 1930's as a preparatory period during which time non-medical IH's developed and applied their techniques, created their intellectual and technical practice together with their exemplars with which to reproduce their profession, and increased in number and power. This preparatory period came to fruition in the late 1930's and once this occurred the OEL paradigm was applied on a wide scale by the IH's. However it would be a mistake to see OEL's and the paradigm which underlay them as simply the creation of a single new professional scientific group. Other groups and forces had an interest.

Insurance companies were generally in favour of OEL's because they saw them as means of limiting claims and as a method of policing their clients* . Warren Cook subscribes to this view and it is no accident that he, working for the Metropolitan Insurance Company, was the first person to bring together and distil the OELs used across the USA and the evidence on which they were based.

Employers were in favour of OELs, particularly in the larger companies, because the majority, if not all, OELs were practical, achievable levels and were often already being attained in the better

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* Bowditch subscribed to this view in his discussion of the lowering of the carbon tetrachloride limit from 110ppm to 40ppm in Massachusetts, he describes the insurance companies strong objections and surmises:

"While I may be mistaken, it seems to me that it is quite obvious why they should object to such a lowering ... if the standard were lowered from 100ppm to we will say 40, it would enable industrial workers ... to make claims and secure compensation which they would otherwise not be able to secure", BOWDITCH (1944)

organised and controlled workplaces. They were an additional aid in the battle to limit compensation claims and keep insurance premiums within bounds. And, if the employer could show compliance they meant that there was little to fear from state regulations and inspections.

Nationwide standards were favoured by large business. In a discussion of the carbon tetrachloride standard Dr Frank Low for the industry makes the point eloquently:

"We ship a car load of carbon tetrachloride in drums to our Chicago warehouse. We don't know where those drums are going. They will be divided up between five or six states. If those five or six states have different standards, it puts us in an impossible position", BOWDITCH (1944)

OELs were an aid to business planning.

Government authorities were in favour of OELs firstly in local state organisations and later in federal authorities. OELs were the least interfering way of regulating the exposure to toxic substances in industry. They were a simple performance standard and how the individual employer met this standard was left up to it, although some states did offer guidance in specific cases.

Warren Cook took strong objectives to this view arguing that insurance companies want low limits to reduce, "the number of cases of occupational disease", and therefore "the amount of money to be paid out in compensation claims", Ibid.

Greenberg felt that "... compensation isn't decided on the basis of how much carbon tetrachloride there is in the atmosphere", Ibid, But on whether a person has symptoms of poisoning and has, or has not, been exposed to carbon tetrachloride.

Quite who was right in this debate is hard to say. The fact that the insurance companies objected so strongly to the Massachusetts decision goes against Cook's argument that they wanted low limits. And the fact is that many people with symptoms of poisoning were bound, as nowadays, to go unnoticed. The fact that a 40ppm standard had been exceeded would not, as Greenburg points out, by itself be enough evidence to claim compensation. But, it could alert enforcement agencies and others to the possibility and bring about a more careful examination of the workforce than might otherwise have taken place. Exceeding the 40ppm limit would not by itself allow a claim but it might point the finger and would be useful supplementary evidence in court.

Insurance companies like their clients would want practicable limits and based on Smyth et al's work, in the case of carbon tetrachloride, that is what they got. Summarising the impact of Smyth's research a Dr Frank Low (speaking for the manufacturers) said: "It is largely as a result of that work that the present standard in so many states has been accepted as 100ppm", Ibid.

This convergence of interest, albeit for different reasons, meant that the various groups mentioned were willing in the early 1940's to sit together on the ASA Z37 committee and develop consensus OEL standards. Although the process was slow, a fact Smyth criticised in the mid 1950's it shows that the OEL had come of age. And it was not the ASA but the IHs in particular the ACGIH, who seized the time.

A convergence of interest would not guarantee that OELs were produced and gained acceptance. Similar interests existed in other countries but the USA was the only country to pioneer OELs on a large scale. OELs and the paradigm that underpined them developed when they did, in the USA because the time was ripe and institutions and organisations existed which were at the right stage of development. Infectious diseases were on the wane, public perception of chemical hazards was increasing and various pioneer IHs could see the possibilities for their developing profession. The chemical industry in the USA was at the start of a phase of explosive growth. Gerarde in a review of the development of industrial toxicology two decades later recognised the connection: "The truly phenomenal growth of industrial toxicology is tied to the development of new products", GERARDE (1966).

FIGURE 5.2 INCREASE IN CHEMICAL PRODUCTION (TAKEN FROM GERARDE (1966))

And he illustrated his point by means of two figures (see figure 5.2).

The new scientific speciality of industrial toxicology was growing slowly in size and had started to forge an identity separate from pharmacology. It was intimately linked to IH and for the first few important years from 1936 to the late 1940's shared the same journal with the hygienists. The existence of the ACGIH, the AIHA and the Journal of IH and toxicology meant that there was a community of like minded scientists with a common interest in the OEL paradigm. They subscribed to it, the OELs enhanced the joint status of their professions and their professional supporters, in the shape of industry, insurance companies and government had reached the point where OELs were acknowledged to be the most acceptable way of dealing with the growing problem of chemical hazards. OELs also had an enabling effect on production processes which Sachs refers to in her review of th development of TLVs. She strikes an analogy between the TLV and the MPE (Maximum Permissable Exposure) value used in radiological protection and

points out how useful MPE's were in facilitating innovation in processes using or producing ionizing radiation:

"In radiological health and radiation safety the MPE value, blessed by national and international committees, had made possible much of the rapid adoption of various medical techniques, uses of radioactive isotopes, and forays into the field of nuclear energy", SACHS (1956).

Sachs identifies an important effect of OELs. Once a "safe" limit for a material has been set and "blessed", process and product innovation could forge ahead as long as exposure was kept within the bounds of the limit. From this perspective, OELs far from being a brake on industry had, in fact, exactly the opposite effect. Indeed, Gerarde in his review of the development of industrial toxicology makes a similar revealing comment on the purpose and effect of what he regarded as one of first OELs based on large scale animal testing:

"The first large-scale use of dust and fume chambers and large-scale mass-testing experiments on animals for developing standards for industrial practice was initiated by the Manhattan Engineering District during World War II. It was imperative that safe operating levels be established for personnel working in plants producing a large number of atomic-bomb materials. Although the safety of the worker was a factor, it was not the most important one; the prime element was security. There must be no losses of telltale radioactive materials that might give away the program, nor - most important of all - must there be any mysterious illness or deaths from the manufacturing process that might cause comment outside the plant or create a psychological barrier against employees seeking work in such a hazardous plant", GERARDE (1966).

Although the circumstances and production pressures surrounding the Manhattan project were special it is not too far fetched, given the analyses in Chapters 4 and Appendix 3 and the comments of Sachs, to regard OELs set for industrial processes as fulfilling similar roles to the MPEs derived for radiation work. This point is considered in greater depth in Part Five of the thesis.

The major point to be made here is that once the IHs and the ITs had found their professional feet and convinced themselves and the groups mentioned earlier then the way was open for rapid acceptance of OELs. Another sign of the times was the name the ACGIH gave its OELs. In the first two lists, circulated in 1946 and 1947 OELs were referred to as Maximum Allowable Concentrations (MACs) in line with similar names used by local state organisations and the exemplary list prepared by Cook. But in 1948 the name was changed to "Threshold Limit Value" (TLV) and has remained the same every since. No explanation for the name change is given in the preface to the 1948 list but the implication is that the Committee can do what Patty described in his optimistic vision of 1948 and what was depicted in Elkins figure the same year. That is the Committee on Threshold Limits could identify safe levels of exposure and by using these levels

the hygienist could ensure that "... harmful situations (were avoided) before they had time to cause injury".

The chairman of the Committee that year made two particularly pertinent statements of belief which were to be echoed repeatedly in the 1960's and 70's when the OEL paradigm, advanced by the ACGIH and IH's in general, was felt to be under attack. In his first statement he said:

"In view of the annual revision of threshold limit values, it has been the purpose of the Conference to seek values which, on the one hand protect the individual workman, and on the other would impose no impossible burden on the manufacturer".

And in his second:

"There is no industrial poison so potent, so virulent, that it cannot be manufactured safely under carefully controlled conditions", ACGIH, (1948).

By 1948 the OEL paradigm (see Working Definition) was the basis by which the ACGIH Committee worked.

The 1950's were a period of adjustment and consolidation for the ACGIH TLV. The number of TLVs increased steadily and more and more people other than the relatively small number of professional IHs referred to them. During this decade the official use of the ACGIH list spread throughout a large part of the Western world. There were challenges: the ASA continued to produce OELs but the process was slow, the standards were identified as being US standards and they were consensus, compromise standards. Smyth referred to them as "... alternative opinions, which are not widely referred to ...", SMYTH (1962a). They did not have the underpining of the scientific paradigm which ACGIH and IHs in general projected onto TLVs.

There was cogent criticism of the methods of the TLV Committee right from the start from within and without the IH profession. Smyth and his colleagues made several telling points in 1949: among other things they did not like a single limit with no information on its basis, or the preface to the list which was seen as too absolute and they did not like the name TLVs because it implied that the limit was a dividing line between safe and dangerous concentrations when in fact, as Smyth put it "... no such description can be truthfully attached to them". Smyth and his colleagues preferred them term "hygienic guides" to TLV and the AIHA eventually started producing OELs of this name in the late 1950's. As with the ASAs OELs they have never rivalled the TLV in status or influence. Elkins refers to, "a small minority which has been

sceptical of, and even hostile to, the use of MACs in preventing occupational disease", ELKINS (1948). He never identifies the individuals or their group affiliations but it would appear that some were physicians who like Teleky posed medical surveillance as an alternative to MACs. Elkins gives the idea short shrift: "To abandon MACs and rely solely on medical control measures would be to take a long step towards the dark ages of empiricism in occupational disease control", Ibid.

Right from the start of the TLV Committee and all the way through the 1950's various industrial physicians sniped at the committee for not having enough doctors in the membership or for producing meaningless standards. For instance Castleman quotes a Dr Frank Princi as saying before the Industrial Medical Association:

"Most of the (TLVs) are picked out of a hat, 95% are on the basis of animal experiments only, incorporated into state codes, and we are faced with ridiculous standards. Is there a doctor among the group that puts out these standards?" Quoted in CASTLEMAN, (1984).

This type of criticism did not deflect the IH and IT professions from their chosen paths and the ACGIH Committee did not falter.

It would appear that whenever TLVs were used or referred, implicitly the OEL paradigm was accepted. Certainly Stokinger, in the late 1950's and early 1960's projected the TLV as the US standard based on the process of scientific standard setting. There were changes in the TLV Committee's composition in the 1960's, documentation was published together with Short Term Exposure Limits (STELs), skin notation and carcinogens appendix were introduced. Some of the criticisms of the IH community eventually had an effect but the biggest factor which accelerated these developments was the incorporation of TLVs in the Walsh-Healey Act of 1960. All of these developments took place within the framework of the OEL paradigm and caused no fundamental upset. The challenge to the paradigm which is evident in the writings of Stokinger, Hatch and Smyth did not really threaten until the late 1950's. All three authors were at their most prolific in the 1960's and early 1970's. Hatch only wrote on the subject during this period, at the end of his career. Both Smyth and Hatch in particular were concerned with the fundamental basis of OEL setting. Stokinger was also but unlike Hatch and Smyth he concentrated on a defense of the ACGIH approach and methods. He was chairman of the TLV Committee all through this period and so his method of defence is perhaps not so surprising. In 1970 they all attended and addressed a joint symposium held by the American Academies of IH and occupational medicine entitled "Thresholds: Do they exist?" The hygiene profession was examining the basis of the OEL paradigm, but why, what was it responding to and why is answering these questions

important in this thesis? I will address the last question first. The fundamentals of the paradigm to which a scientific community subscribe are not debated during periods of "normal science". They are visible when a new paradigm is accepted by the community or when a crisis arises. For the OEL paradigm the 1950's was a period of "normal science" and the sixties were a period of crisis. That is why the exploration of the paradigm is so fruitful in the literature of this period. The basis by which the IH community worked was being re-examined and defended. Also the way the IHs defended their career shows up the paradigmatic nature of their project, ie. they were working to a paradigm and the fundamentals of their scientific profession were, they felt, being attacked and undermined. An examination of where this challenge came from gives a unique insight into the connections between the IH and associated professions, the OEL paradigm and the society of which the IHs were a part and in which they functioned. The challenge came from a number of directions simultaneously and the scientific social and political elements cannot be easily disentangled, which again reflects back on the position and function of the IH profession and OELs.

5.2 A threat to the OEL paradigm

The threat to the paradigm to which the IH's worked came from a number of overlapping and inter-connected quarters.

In the late 1950's the US IH's and IPs (industrial physicians) had their first chance to make formal contact with their Russian counterparts and the PHS translated various Russian works. Smyth refers to the first effects of preliminary contact; "... serious uncertainty was evoked ... when it was rumoured that values enforced in Russia were in some instances one-tenth or less than the ACGIH values", SMYTH (1962). By 1963 a delegation of US IH's and IP's had been organised and they embarked on a four week tour of scientific institutes and workplaces; Stokinger and Smyth were amongst the delegation of six people which was led by Magnuson. It was called the US Industrial Toxicology Delegation and spent four weeks in the USSR, MAGNUSSON et al (1964). The translations of Russian works, the tour and the more regular presence of Russian delegates at international conferences meant that the fundamental differences in philosophical* approach to OELs became more widely known. They had to be answered and various authors including the ones examined earlier did so.

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* Philosophical is perhaps the wrong word. The Russian scientists were actually working to a different scientific paradigm derived from different basic assumptions and a different history of development of the sciences involved particular medical sciences. Dinman argues that these differences ultimately stem from the dominance of Pavloviam views in Sovient medicine and the relationship Pavlov's ideas to views of human existence and social change. DINMAN (1976).

Though meeting the alternative Russian OEL paradigm was unsettling and was not really resolved until Hatch proposed his all embracing model in 1973 this was not the only scientific challenge the defenders of the OEL paradigm had to deal with.

In the late 1950s Watson, Crick and Wilkins worked out the structure of DNA, revolutionised biology and gave enormous impetus to molecular biology. The problem had been central to the field for a number of years. The way was now open to molecular biologists to investigate how the genetic information was coded in the DNA and how this information was translated by the cell into the manufacture of cellular structural, control and catalytic proteins. The science of molecular biology expanded apace throughout the 1960's and had an enormous impact other biological sciences, including toxicology. Two separate but connected areas of particular relevance in this context were radiation biology and carcinogenesis. Stokinger expresses doubts that a threshold of effect exists for ionizing radiation in his earliest paper on OEL's in 1955. And Sachs expressed similar reservations. She quotes another hygienist, LS Taylor: "Even the smallest amount of radiation has some effect on the living human body. Even if the harm is normally undetectable by the individual, genetic damage can result from single minute changes". SACHS (1956). An understanding of the central role and structure of DNA reinforced this view and also help spread the idea of no or very low thresholds of effect in the related field of chemical carcinogenesis. The models of Druckrey in Germany that Stokinger found so unpalatable were published in early 1960's.

It would appear that apart from the Russian alternative paradigm fundamental changes in sciences which impinged on IH and IT (Industrial Toxicology) undermined the general belief that thresholds of toxic effect existed for all toxic substances no matter what their mode of action. In the case of ionizing radiation and chemical carcinogenesis a strong case had been developed by the early 1960's that questioned this view.

On the social and political front two movements developed in the 1960's which were to put IH's further on the defensive. The first was the environmental movement which hit the headlines and started to grow in numbers and as a political force with the publication of books such as Rachel Carson's "Silent Spring" published in 1962. By the late 1960's the movement had begun to build up a head of steam and grew even more powerful in the 1970's. At the same time scientists from

other scientific communities started to question the presumptions that hygienists and toxicologists had made which struck at the basis of their paradigm. These scientists were not in any direct sense a product of the environmental movement but individuals working in scientific communities not traditionally concerned with industrial or community health. Perhaps a way of understanding why such scientific challenges developed during this period would be to see the environmental movement as creating the social and scientific space to question old assumptions. The toxic effects of lead were systematically questioned by such outsiders from the mid 1960's. Patterson published the paper which caused the most stir in 1966 and started a long and fierce battle between her and scientists with similar beliefs and the toxicologists, physicians, hygienists and others who Robbins and Johnstone refer to collectively as "occupational hygienists" and other medical scientists, ROBBINS and JOHNSTONE (1976). Patterson was a geological chemist and her main target was the threshold concept by which it was argued that below a certain "lead balance" maintained by the body's excretory mechanisms no harm would result. Robbins and Johnstone summarise this position:

"This concept had been successful in reducing poisoning amongst workers in high lead environments and had been elevated to paradigmatic status by the research of RA Kehoe and his associates ... This cognitive structure was derived from the methods and expertise particular to medical scientists, depending as it does on the identification of clinical symptoms of lead poisoning and correlation with lead concentrations in blood and urine, the latter data obtained by fairly elementary methods of analysis. If followed also from this paradigm that lead levels in the tissues of the general population ... were to be considered as 'natural' - being safely maintained by the body's defence processes", Ibid.

Patterson questioned this view;

"... he attempted to redefine the concepts of 'natural levels', emphasising that the term as used by the threshold paradigm meant only "currently typical levels", Ibid.

She concluded:

"... the 0.25ppm level of lead in blood, which has been and is still regarded with ill-founded complacency actually seems to lie between the average natural concentrations of 0.002ppm and an acute toxic threshold of 0.5-0.8ppm. This suggests clearly that the average resident of the US is being subjected to chronic lead insult. The threshold of damage concept, as applied to lead, is an ill-defined opinion unsupported by any evidence", (PATTERSON, (1966)), quoted in Ibid.

The battlelines were drawn and the fierce debate has continued until the present day. Robbins and Johnstone describe it as follows:

"The controversy bears all the marks of a conflict between self-contained systems and belief; lacking concepts and terminologies in common, the protagonists tend to 'talk through' each other", p358, Ibid.

Later they quote the conclusions of another authors commentary:

"The crux of the problem is that the classical concept of acute lead toxicity lacks terms to answer Pattersons arguments. There is no recognition of low level damage, lead poisoning is defined by clinical signs and symptoms ..." (Editorial from Science and the Citizen, April 1968), Ibid.

The language used by both groups has been vitriolic and sometimes very personal with both sides at times claiming that the other is not scientific. Both sides were from different scientific backgrounds and owed allegiance to different professional groupings. They worked day to day by subtly different paradigms and saw the problem of lead and its effects on health differently. When each accused the other of being unscientific what they were in fact saying was, you do not subscribe and work to our paradigm and there was no logical way of resolving the difference. At the same time this rejection was coupled with the need to maintain as tight a professional hegemony as possible over an area of human knowledge. The "occupational hygienist and medical scientists" were not only saying to Patterson et al, you are unscientific, but also, you have no right or qualifications to speak on these matters, this is our territory.

Elliot summarised the position of any profession facing a challenge:

"The patterns of thought and activity which develop in a profession are supported internally and externally by its own structure and the relationship it has established with other organisations and associations. Not only careers and economic interest are at stake, but also established patterns of thought and ways of approaching the world", (ELLIOT, 1972) quoted in Ibid.

This quote reminds us, when examining scientific controversies, to look further than the logic of the arguments put forward by the opposing groups to the relationship the particular profession has forged with external benefactors and supporters.

Initially Patterson et al had no constituency of "organisations and associations" apart from the tacit support of "environmentalists" but the IH's and the others had definite constituencies: industry and the governmental organisations for which they worked. Both Hatch and Stokinger called for a vigorous defence of the principles by which industry used toxic materials. Hatch was particularly forthright in 1970:

"Any challenge to this operating principle must (therefore) be met with the utmost vigour by those responsible for industrial health maintenance; and, in this effort, they must be given full support by all others concerned with the survival and progress of industry", HATCH (1972)

Stokinger made similar points in a number of his papers in the 1960's and 70's and in his case what really seems to have rattled him was the combination of a more suspicious attitude by the public towards the use of chemicals together with groups of scientists, like Patterson, who were prepared to articulate similar concerns on scientific grounds in scientific fora. Stokinger described such interventions as, "... seemingly conflicting evidence from non-toxicological quarters", STOKINGER (1972a).

By the late 1960's the scientific and social forces described were pressing hard and yet no adequate response was forthcoming from the IH's and other "health scientists". And there was another social and political force which gained momentum and support in the 1960's and that was the campaign by the labour movement and a section of the Democratic Party for federal legislation which culminated in the OSHA Act of 1972. Part of the campaign for these changes was the argument that there was still a huge amount of disease and disability being caused by work and that newly perceived hazards such as carcinogens were not being dealt with. The campaign reflected badly, at least tacitly, on the past work of the IH's and others. As Hatch put it:

"There is practically no recognition of the very great accomplishment in occupational disease control and accident prevention over the past half century or more", HATCH (1974).

5.2.1 The hygienists and others respond

The hygienists, industrial physicians and toxicologists responded to the challenge in a number of ways. They regularly repeated what they believed to be the fundamental axioms by which their professions worked. They held joint symposia reiterating their beliefs and occasionally pouring scorn on alternative views such as Patterson's. And some individuals, such as Hatch, explored ways of extending the paradigm by which the professions worked without breaking faith with the axioms of the past. The first two responses had two functions which are well described by Robbins and Johnstone:

"(The) external function is to persuade and reassure public and clients in order to maintain the professional authority of the group ... (The) second and internal function is to underline cognitive orthodoxy and so discourage deviance amongst toxicologists (and hygienists et al)".

That the "hygienists" (industrial hygienists, toxicologists and physicians) rejected the "non-toxicologists" position is not surprising. They came from different scientific groupings and institutions with different loyalties and traditions, and the views they shared, as was explored in Chapter 3.0 were, in the words of Hufbauer; "... more akin to an ideology than a Kuhnian

paradigm", HUFBAUER (1982). It is perhaps not surprising that the response of the "hygienist" was not simply a questioning of theoretical or technical debate.

The details of the response are evident in the work of Smyth, Stokinger and Hatch covered earlier. What is particularly interesting is the form and language of the response. The challenge was pitched at a scientific, social and political levels. The response was, only in scientific journals or meetings and was couched in scientific language. Social and political issues to do with the way chemicals and processes are used and the degree of harm which should be allowed are subsumed into an apparently neutral scientific formal language. The issues are described in their social context on occasions but they are never explored and the solution is always to return to bedrock scientific beliefs. Most of the writings explored read as if social issues are not being discussed, they are hidden in the formal scientific language of the hygienist. That is one reason why it is so difficult to see the OEL paradigm for what it is - a social construct.

Apart from reiteration, the OEL paradigm was expanded to incorporate and cope, as far as the industrial hygienists, physicians and toxicologists were concerned, with the challenge at the scientific levels of the USSR and the environmentalists. Hatch, in 1973, presented a formulation of the paradigm which at the same time as incorporating the USSR's approach to homeostasis dealt with and attempted to explain away the challenge of Patterson and her like. Within the professional scientific communities in occupational health in the USA, and in large measure internationally, he was successful. His model was explicitly adopted by the WHO and, as was shown earlier, his model is repeatedly referred to in the papers of key individuals and standard texts.

The substance of the OEL paradigm is the subject of the next section. What can be said at this point is that it survived a severe challenge and by means of a model articulated by Hatch the professions involved explained the challenge to their own satisfaction and that of their clients. The morale and composure of the hygienists and others was maintained. The challenge of the environmentalists, the alternative scientific formations of scientists from other communities and the destabilizing effect of no-threshold theories of carcinogenesis continued and still harry the hygienists and other professions today. Kuhn specifically rejects the idea that science progresses simply by falsification or confirmation procedures. For him "competition between segments of the scientific community is the only historical process that ever actually results in the rejection of one previously accepted theory or in the adoption of another", KUHN (1972). The competition between the "hygienists" and the "non-toxicologists" continued with no resolution. To understand this phenomena the process described by Khun needs to be extended to include the

influence of the organisations and associations who support the "hygienists" and the "non-toxicologists" many of whom, in the case of the "hygienists" are their clients. Also the manner in which the debate is conducted should be considered.

The "hygienists" still maintain their hegemony over the field of occupational health. They are collectively the oldest and the biggest of the two competing groups in the scientific community, and they have the most economically powerful supporters (industry and large parts, but not all of the state). This continued dominance is, I would suggest, due not only to the extension of the paradigm by Hatch but is also due to power of the scientific community defending the paradigm (ie. industrial hygienists, toxicologists and physicians) and the power of this scientific professional communities' clients. The competition, in this case, cannot be resolved simply by "competition between segments of the scientific community ... " because resolution is not only about competing scientific theories and groups. Although the "hygienists" appear to be conducting a scientific debate, the language and content of their work subsumes social and political positions. Positions which accommodate, or at least do not clash with, their professional supporters and clients. The resolution of what at first sight appears to be a scientific conflict therefore cannot take place without a resolution of the social and political conflicts embedded in the "science".

The development of the paradigm which underpins the OEL's and the use of OEL's have gone through phases, from tentative beginnings in the 1930's through a consolidation phase in the 1940's followed by a period of sustained growth in the 1950's and 60's. The OEL paradigm was challenged in the 1960's but its hegemony in the field of occupational health was maintained by the IHs and ITs. The production and use of OEL's continue though not without challenge. This challenge takes a number of forms and ranges from theoretical disturbances stemming from developments in the related sciences of molecular biology and carcinogenesis to direct challenges to the OEL paradigm from non-toxicological quarters. The OEL paradigm is not trouble free or untarnished but it has survived.

5.3 The OEL Paradigm - Variations on a Theme

"Whatever scientific progress may be, we must account for it by examining the nature of the scientific group, discovering what it values, what it tolerates, and what it disdains", KUHN (1970).

It is all too easy to regard a paradigm as a set of rules, which, in theory, could be written down, and perhaps the creation of a Working Definition of the OEL paradigm encourages this view. It is a mistaken view because it is in reality very difficult to encapsulate a paradigm and if one considers how sciences progress it is clear that a paradigm could not be a set of rules, an algorithm. If it were then all scientists in a certain community would arrive at the same answer to the same problem and the same dead-end when faced with the same anomaly. As Kuhn repeatedly emphasises, sciences are the products of like minded, but not same minded, groups of individuals and each individual can interpret the paradigm differently. Rather than creating anarchy as some of his critics suggest this explains how anomalies are overcome and the paradigm is defended by the scientific group. In an international colloquium to discuss Kuhn's work he faced the criticism that his view of the scientific group is too psychological. One of his critics, (LAKATOS) objects to the emphasis that Kuhn places on the importance of the individual in the group. Kuhn answers as follows:

"Given a group all the members of which are committed to choosing between alternative theories and also to considering such values as accuracy, simplicity, scope and so on while making their choice, the concrete decisions of individual members in individual cases will nevertheless vary. Group behaviour will be affected decisively by the shared commitments, but individual choice will be a function also of personality, education, and the prior pattern of professional research. (These variables *are* the province of individual psychology). To many of my critics this variability seems a weakness of my position. When considering the problems of crisis and of theory-choice I shall want, however, to argue that it is instead a strength. If a decision must be made under circumstances in which even the most deliberate and considered judgment may be wrong, it may be vitally important that different individuals decide in different ways. How else could the group as a whole hedge its bets?", KUHN (1970).

It is clear from Chapter 4 and Appendix 3 that Kuhn is correct. The leeway given to the individual to interpret the OEL paradigm allowed a variety of responses to the challenge posed in the late 1960's and 70's. At the theoretical level at least the original solution of Hatch won out. However this variability poses an analytical problem. As paradigms themselves are impossible to encapsulate the fact of individual interpretation makes the task doubly impossible!

The point of writing out a Working Definition of the OEL paradigm was a device, an analytical tool which I have tried to use in the description and analysis of the development and use of OELs. It has, I believe, served a useful purpose and it can be used, in this part of the thesis, for one more task: illumination of the variety of views held by the hygienists examined. The Working Definition of the OEL paradigm can be summarised as follows:

1 Toxic substances can be used safely.

2 IH control methods are adequate for all toxic substances.

3 Threshold doses exist for all toxic substances.

4 Standard setters can identify threshold exposures (doses).

5 The major consideration in the setting of OELs is the health effects of toxic substances.

What answers would the individuals examined have given to the five statements and how would they differ? Each person will be considered in turn.

5.3.1 Winslow

Judging by his approach to setting standards he would probably have agreed with statements 1 to 3 but added that continued medical surveillance would be required in the case of some substances. In theory he would have agreed with statement 4 and would probably have given an equivocal answer to statement 5. Certainly in the case of the benzene he, and his committee, appear not to have known what to do in a situation where health effects were still evident at what were judged to be practicable exposure levels. But then Winslow worked on these problems at a time when, I would submit, that the OEL paradigm was in the making and had not been coherently forged and more importantly the non-medical IH profession did not exist. Winslow was venturing into uncharted territory by himself.

5.3.2 Drinker

Was a physiologist and became a hygienist. From his early work on zinc oxide fume it is clear that he believed in thresholds of toxic effect and that, for him, in the case of zinc oxide, these thresholds were fairly sharp. His statements on silica and silicosis ranged from equivocal, to warning against setting permissible limits to unequivocal statements in support of the existence of such limits. In the early to mid 1930's he would probably have agreed with statements 1 and 3. As the IH profession gained strength and more and more OEL's were recommended by the late 1930's he would have agreed with all the statements with some reservations on 5. The tension between the need for a practicable limit and the belief in the existence of a threshold dose is regularly present in Drinker's writings. By the late 1940's he was persuaded that OELs should be set and sat as an independent assessor on the ASA Z37 Committee and was a member of the ACGIHs TLV Committee for the first two years of its existence.

Like Winslow, Drinker was a practical experimental scientist concerned with practical solutions to problems. He rarely theorised on the basis underlying the setting of OELs unlike the next three individuals.

5.3.3 Smyth

From the extensive exploration of Smyth's work it is clear that his views on the working definition of the OEL paradigm varied over the years. His answers could have gone as follows:

1 Yes
2 Yes
3 Yes - in theory
4 Yes/No perhaps
5 Yes certainly - Yes equivocal

There were nearly always areas of contention for Smyth. He never liked the single figure limit or the acronyms MAC or TLV. For statement 4 he would have probably answered; it depends on what substance and what health effect you are considering. Although he was a cogent critic he supported the setting of OELs and his toxicological work supplied the data from which many standard setters started. He never varied in his allegiance to the basic premise that OELs should be set and could be set. His basic approach to OELs and the effect of toxic substances on humans appears to have been based on a belief in the homeostatic mechanism coupled with his view that low toxic doses provided sufficient but not overwhelming challenge. Most of his writings are concerned with the nitty-gritty of toxicology or hygiene, he did not venture into the realms of the philosophy underlying his practice even when industrial hygiene and toxicology were under attack in the 1960's. For Smyth the basis of his science was self evident and he felt no strong need to defend it. The last two individuals considered are the people who defended the OEL paradigm, tried to innovate within it and take account of developments in the field of occupational health, the sciences which impinged on the field and other developments in the wider society.

5.3.4 Stokinger

Would have agreed more or less strongly with all five statements at different periods in his career. In the 1950's he would, perhaps, have had doubts over statements 1 and 3 as regards carcinogens but these were dispelled by the 1960's. Similarly in the 1930's he appeared to believe that OELs could be set to cover all members of the working population. By the 1960's he had altered his views on coverage: all could be protected apart from the "hypersusceptibles". Stokinger, of all the people considered in this part of the thesis, was the most unswerving advocate and publicist for the list of statements I have called the OEL paradigm.

5.3.5 Hatch

Definitely changed his views over the period of his career. In the 1930's the "young Hatch" would probably agree with statements 1, 2 and 3 but not 4 and would have given an equivocal answer to 5. The "older Hatch" in the 1960's and 70's would have varied. In the early 1960's and from around 1972 onwards his response to the statements would have been prefaced by a statement such as; "It depends how you define ... ". For him, at this time, questions as to the existence and identification of thresholds had no simple, cut and dried answers. And it was this flexibility of vision that allowed Hatch to explain, at least to the satisfaction of the industrial hygiene, toxicology and medical communities how to reconcile the alternative approach of the USSR and the "environmentalists". From the mid 1960's to around 1972 Hatch was far more defensive and while he might have quibbled with the definitions of some of the words used he would probably have agreed with all the statements.

The limitations of the Working Definition of the OEL paradigm as an analytical tool are clear. Many of the subtle but important differences between the individuals considered are not captured by applying a "standard paradigm" to their work.

Thus for instance, while all the authors explicitly believed in dose response relationships, their interpretations differed significantly.

Smyth believed that small amounts of toxic material could stimulate the healthy person to meet the challenge. He based his belief on a mixture of homeostatic theory and homeopathic medicine.

Stokinger on the other hand while he described the standard asymptotic dose response curve, and defended the TLV on the basis of homeostatic mechanisms, came to believe, as his career progressed in an especially sensitive sub-group, the "hypersusceptibles". Eventually, it would appear from his writings, that he regarded anyone who responded at or below the TLV as hypersusceptible. Neither Smyth or Hatch ever went this far. Smyth would list factors which could make a person more sensitive and the list would contain "genetics" but he never gave it pride of place or used the term "hypersusceptible" and neither did Hatch although he did believe in a fixed constitution.

In the field of dose response relationships Hatch was far more innovative than Smyth or Stokinger. In his papers through the 1960's and into the early 1970's one can see him

developing ever more subtle and complex theoretical models of the relationship between exposure and effect. (The models were constructed partly as a defence of the IH and IT professions and the OEL paradigm). They are very convincing and have become virtually paradigmatic within the Western occupational health arena and possibly large chunks of the Eastern arena as well. However sophisticated though the 1972 model is, Hatch arrives at a similar place to Smyth and Stokinger, he put forward a Tolerance Limit Value rather than a Threshold Limit Value and he does so on the basis of the homeostatic mechanism. Interestingly, though it never entered his models directly, Hatch like Stokinger was a great believer in a fixed property of certain people which he termed "vital endowment". He never, as Stokinger did, suggested that such people could be identified and weeded out, but he does appear to have become convinced that people who succumbed to exposure to toxic substances are in someway constitutional different from normal. That their "vital endowment" is impaired and in 1965 he referred to such people as "physiological cripples".

What then can one say about these three men and the mental models of the dose response relationship which they developed and used. Firstly, that the professional education which they imbibed allowed considerable leeway for individual interpretation which did not require a break of faith with what would be regarded as the bedrock belief of the profession. Secondly: Stokinger and Hatch had related ideas on sensitivity. It was seen as a fixed constitutional property of individuals. Smyth never subscribed publicly to such static views on sensitivity. And Elkins, apart from allergic responses, appears to have viewed sensitivity as a normally distributed property found in any populations, ie. he did not single out certain groups or individuals as somehow special.

Finally, this variety of view within the same profession was one of the, "effects of a shared ideology" as Kuhn put it and is a source of strength for the scientific group, "When considering the problems of crisis and of theory-choice ...", KUHN (1970b).

What else informed their day to day practice and the assumptions by which they worked? Part of the answer is evident in the writings of the individuals consider as the example of the dose response relationship shows. Another part of the answer is to consider how professional hygienists and toxicologists would approach a problem and what exemplars they would have been trained in and would work to. Looked at from this point of view and considering the question, has the intellectual and technical practice, the paradigm of the industrial hygienist and the toxicologist, changed since these professions were formed, raises an interesting paradox.

Because the basic practice of the two professions has not changed fundamentally and yet the techniques, the sciences which impinge on the professions and the society in which they operate, all certainly have changed. Hygienists nowadays are still concerned to assess a work population's exposure, they examine the control methods applied or consider what else could be applied, they adopt a process based approach to the problem of occupational health and their major fixation, as in the early days of IH is with airborne contaminants. And basic toxicological practice also has not fundamentally changed. Animal or other models are still used as surrogates of human exposure. Small numbers of inbred animals are given relatively large doses in an attempt to increase the sensitivity of the test. The toxicologist, with much qualification and hedging of bets, then attempts to extrapolate his or her results to human beings. So in answer to the questions has there been any change the answer must be no and yes. No, not in the fundamental approach of the professions but yes in the details of that practice. And by details I do not simply mean the techniques applied by the two professional groups, though these are important and have caused mini-revolutions in the subject area. Details also include the standard mental assumptions and ways of thinking that a profession brings to bear on problems. This area may look static because the same words have been used for decades but in reality their meaning has changed. Words like, exposure, experience, judgement, observation and health effect all have different meanings nowadays as compared with 1940. What's more it is evident that these words had subtly different meanings for the different individuals examined. The meanings of the words has changed because professional practice has changed, thus exposure is a more exactly defined theoretical and technical concept nowadays than in 1940. However, phrases such as health effect have also changed and this is more to do with the perceptions of the professions which are in turn strongly influenced by the society of which they are a part. Thus "transient" non-specific disruption of mental function was not generally regarded as important in the 1940's and 50's whereas nowadays there is far more public and professional concern with for instance, mild, but perhaps prolonged CNS effects brought on by organic solvent exposure. And the importance of irritation as a health effect has changed. In the 1940's and 50's it was dismissed by many professionals in occupational health, but not all, as not a health effect but more a phenomena concerned with comfort. Whereas by the 1960's and 70's this attitude changed though, it must be said, there is still a wide range of opinions/perceptions within the hygiene toxicological and medical professions.

There is no doubt that the scientific professions of hygiene, toxicology and medicine have progressed since their formation. A cursory comparison of the models of dose response of Drinker in 1927 and Hatch in 1972 clearly makes the point. The latter is far more detailed and sophisticated than the former. But at root, I would argue, despite differences in the

interpretations of the professions' paradigm and differences in the theories of standard setting erected to justify and defend the profession, when pushed Drinker, Smyth, Stokinger and Hatch would all agree with the five statements put forward as the "OEL paradigm". They would do so because they had a shared past, the era of "hard facts" (as Hatch put it) and frank disease, and shared exemplars such as the control of frank lead and benzene poisoning and virulent silicosis. Because the benchmark used by the profession of IH is still the single figure OEL which is still set, in the case of the ACGIH, by the same professional organisation formed over 50 years ago.

But perhaps most importantly, because they had a shared commitment to the ideology of the profession of IH.

If paradigm, in the Kuhnian sense, is the correct way of describing the set of theories and practices articulated by those three authors and others then it perhaps explains a phenomena evident in their writings; the way in which apparently non-paradigm relevant issues of feasibility, overdesign and cost are introduced. What is striking is that these issues are rarely discussed and when they are it is usually obliquely. This fits with what one would expect if Hatch and others believed their descriptions and beliefs to be scientific and indeed paradigmatic. They, in their writings, are concerned with the fundamental tenets that underlie their profession, and from this viewpoint questions of feasibility etc are deemed to be irrelevant, they are therefore not discussed. However, IHs and toxicologists are practical, applied scientists and issues of great importance to the practice of IH occasionally leak into descriptions of the IH paradigm. What is surprising is not so much that practical issues leak into the pure paradigm occasionally but that, given the practical applied scientific project of IH and IT that such issues are not to be regularly found in the writings of Stokinger, Hatch and Smyth. The overall message of these men is an idealised model unrepresentative of the real, messier, world of practical OEL setting.

5.4 Some comments on scientific professionals

This part of the thesis has explored the OEL paradigm; how it developed, when it could be said to be fully articulated and how it was promulgated and defended. The analysis has attempted to understand the OEL paradigms as the product of a community of professional scientists. It has only considered the other aspect of the analytical framework when and where what could be termed external issues have intruded.

The analysis showed how the OEL paradigm did indeed crystalise in the late 1930s. The groups responsible being IH and the still developing nascent profession of IT. What was also clear was

that the paradigm appeared to be constrained by issues which can be broadly described as concerned with practicability.

Here the needs of the IH profession as scientific professionals with clients interests were seen to be paramount. It is clear that IHs can behave as if they were working to a Kuhnian scientific paradigm and at the same time, like any other profession to a professional paradigm concerned with clients needs.

It is this conflation of two sets of demands which has produced a lot of the confusion that surrounds OELs. How this confusion has become embedded in the OEL paradigm has been explored in some detail. It is clear that the needs of clients coupled with the practical project of IH as an applied science-based professions are part of the OEL paradigm. Perhaps this is exactly what one would have expected of an applied scientific profession but it is rarely explored or articulated.

Before proceeding to examine the actual practice of standard setting it is worth considering in general terms the idea of clients needs.

5.4.1 Clients needs

It is possible to identify some of the general needs of the different client groups mentioned in Chapter Three and this Part of the thesis.

Universities

- Teaching requires courses which attract students and need to provide a qualification which attracts employers.
- Research needs to attract sponsorship.
- Applied research requires the co-operation of industry for applied fieldwork.
- Scholarship requires publication of research and analysis in respected, reviewed journals.

US PHS (pre OSHA)	-	Practicable standards which while offering health protection could be enforced by state field units and with which industry could comply.
Industry	-	Practicable, least cost solutions to health problems.
Insurance companies	–	Standards which limited and made predictable the liability of client companies.

The immediacy and intensity of the pressure of clients needs would, and will, vary with the client for whom the IH or IT works. In general one could say that the pressure was least in the PHS and universities and most in industry and insurance companies. How individuals responded would be, and is, very much a question of his or her circumstances and personality.

The OEL paradigm in practice as opposed to theory is the subject of the next part of the thesis.

PART FOUR

THE PARADIGM IN ACTION – STANDARD SETTING IN PRACTICE

CHAPTER SIX

THE ACGIH TLV COMMITTEE

"The philosophy of the TLV as established by ACGIH in those early days of our profession has remained substantially unchanged and despite carping, criticism, misunderstandings and abuse, today its values, for better or worse, are accepted on an international basis as the best available guides for providing healthful occupational environments for the workers of the world.

TLV's have made is possible for the industrial hygiene method to prevent chronic degenerative health failure ..." p43, FREDERICK (1968)

6.1 Introduction

Frederick was the first chairman of the NCGIHs (later to be renamed ACGIH) Threshold Limits Committee (1942 - 1946). He makes enormous claims for TLVs but, although he is viewing the development of industrial hygiene (IH) and TLVs with the wisdom of 26 years hindsight his enthusiasm is in many ways akin to that of the early members and early days of IH and the ACGIH. IHs in the ACGIH may not have realised how influential their TLVs would be but they could see how industry, government, insurance companies and the occupational health field were receiving and supporting their profession and its OELs. IH, as has been described and considered in Part 1, was in the late 1930's/early 1940's a new profession, with a newly articulated paradigm and a new project; the prevention of "chronic degenerative disease" by means of IH principles, recognition, evaluation and control, and the use of single figure OELs.

In Part 2 of this thesis the aim was to explore the "OEL paradigm" based on the writings of key individuals in IH and industrial toxicology (IT). The next four chapters explore the workings of the most influential IH organisation the American Conference of Governmental Industrial Hygienists (ACGIH)[1] . This chapter is fairly descriptive and outlines how the "TLV Committee" was set up and lists a chronology of events in the history of the Committee. Chapter 7 considers the rate of TLV production and the claims made for TLV's as described in the early ACGIH transactions and the prefaces to the TLV list. It also outlines the international influence of the ACGIH and TLV's. Chapter 8 considers the actual intellectual and technical practice of the "TLV Committee" as revealed in the Minutes of this committee. The earlier material on the development of the Committee will help put debates, described in the Minutes, in context. Also, this chapter will draw upon the earlier analysis, in Part 2, of the "OEL paradigm".

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[1] From a professional viewpoint the American Industrial Hygiene Association (AIHA) was probably the most influential organisation in the USA but, as will be shown, the ACGIH has developed a far greater international influence.

So far, in this thesis, the practice of OEL standard setting has been analysed at one remove. A great deal can and has been inferred, particularly about the theoretical concepts which underpin the process. This analysis was essential to gain a deeper understanding of the standard setting process and the factors which influence standard setters. The next four chapters will add a further dimension to this analysis. Chapter 17 will draw some conclusions on the workings of the TLV Committee, how its practice conforms to paradigm expounded by various IH's, what influences are evident in its deliberations and the kind of influence the ACGIH has had on standard setters in other countries.

6.2 The TLV committee

By 1940 the National Conference of Governmental Industrial Hygienists (NCGIH) had, at its third Annual meeting set up 11 Committees. Many, if not most, of these became defunct in the next few years but one spawned what was to become the most long lasting and productive committee in the history of the NCGIH. The "Committee on Technical Standards", chaired by Theodore Hatch, was set up in 1938 at the first meeting of the conference, NCGIH (1938). By 1940 it had not accomplished much but during the discussion of the Committee's activity, such as it was, a Dr Frederick (chief chemist at the Bureau of Industrial Hygiene, Detroit) asked a question:

"May I enquire what Committee, if any, in this organisation is responsible for the establishment of safe limits or threshold limits and concentrations"? p191, NCGIH (1940).

This question caused some discussion on whether the Conference should be involved in setting threshold limits given that the American Standards Association (ASA) had started to do so in 1938. It was decided that the Conference should be involved but there was then the matter of which committee should be responsible; was it a technical matter or did it involve other factors? The choice was the Codes Committee or the Technical Standards Committee; the latter won out. Part of the discussion is worth quoting in full:

"Mr Setterlind — I would consider that safe threshold limits belong to a committee on technical standards, although threshold limits are not of a strictly technical nature.

Dr Gray — Rather than codes?

Dr Setterlind. — That would be placing more hard work on that committee. I don't think it is strictly a technical matter.

Dr Gray. — If Mr Setterlind had sat on those committees of the American Standards Association as I have for two years ... with engineers, industrialists, insurance fraternities, physicians, and others ... I think he would probably consider that it was a technical matter. I would be inclined to put it with the Technical Committee and utilize the Committee and Codes to develop codes based on the material worked out by the technical committee", Ibid.

Right at the start of the TLV Committee the question of whether standard setting was or was not, "of a strictly technical nature" was raised. At this point the Conference had a choice: to go down the path of the ASA and bring in all parties with an interest which would have automatically brought in non-technical factors into the standard setting process, or, to treat the setting of OELs as simply as technical matter. The Conference plumped for the latter option. The decision has caused a lot of confusion, as it is quite evident from the discussion of OELs in Part 2 that the setting of TLVs has never been simply a "technical matter". It is interesting to speculate on why the Conference chose to go down the strictly technical path. Partly it was the predisposition of the IH to treat the setting of OELs as simply a technical matter in the tradition of their scientific profession. Social questions concerning for instance the acceptability of risk were not debated and were in fact side stepped in the sense that the early belief would appear to be, at least for some people, some of the time, that threshold limits was safe levels. The question of the degree and acceptability of risk thus never arose. Also it would appear from Dr Gray's statement that he regarded consensus committees, such as ASA's Z-37, as inefficient and slow. A point made by Smyth over a decade later and still made, in private, by hygienists about the workings of the tripartite ACTS in the UK. Also it must be said, people such as Bowditch who had a protracted argument with the ASA's Z-37 committee over the carbon tetrachloride standard probably felt that the NCGIH would set safer standards. Finally, the technical path played to the strengths of the new profession of IH. It was a numerate profession and by virtue of its self defined remit and day to day work, was in a position to set OELs. In doing so it facilitated its own work, and, as has already been discussed enhanced the professions status and influence. It is quite probable, even if the ASA's Z-37 consensus committee had not existed that the conference would have gone off down the technical route. The meeting decided to add one or two physicians to the Committee, one of whom had to be from the US Public Health Service (PHS). In the event the Executive Committee of the Conference decided to split the Technical Standards Committee into two sub-committees which are listed in the Transactions of the next annual meeting, NCGIH (1941). However Leonard Greenberg, who was by now Chair of the Technical Standards Committee reported in 1941 that the new sub-committee on Threshold Limits had not begun to function but promised that a chairman would be appointed "within the next four weeks". By the next meeting Frederick had been appointed Chairman and the sub-committee had prepared a list of "maximum allowable concentrations of atmospheric contaminants",based on other lists prepared by "State units", p163, NCGIH (1942).

After 1942 there is a lull in reports from the sub-committee on Threshold Limits. In 1943 the wisdom of the proposed ASA 100ppm standard for carbon tetrachloride is discussed. And a year later the discussion continues with a paper by Bowditch entitled "In Setting Threshold Limits", BOWDITCH (1944) which has already been referred to in the discussion of the role of insurance companies in promulgating OELs. There was no meeting or Transactions in 1945 almost certainly because of the pressure of wartime work. The war had not prevented the two Technical

Committee's sub-committees from functioning and in 1946 both reported to the eighth annual meeting of a renamed NCGIH; from 1946 the Conference was called the American Conference. The sub-committee on Standard Methods presented a long discussion of a comparison of analytical methods for lead. Frederick presented the first list of ACGIH "Maximum Allowable Concentrations of Air Contaminants". The list was compiled from three sources; Cook's 1945 paper in "Industrial Medicine", the list of State standards presented to the NCGIH in 1942 and the ASA's list[2]. It comprised 112 MACs for gases and vapours, 19 MACs for

Toxic dusts, fumes and mists and 13 MAC's for Mineral Dusts, 144 MACs in all plus two radiation standards. The list was put to the meeting and passed without comment or discussion.

The list was up-dated each year and apart from being printed in the ACGIH transactions was published each year a few months after the Annual meeting, without comment, in the Industrial Hygiene Newsletter of the US PHSs Division of Industrial Hygiene up until 1949. In 1950 the list was published in the newly created journal of Industrial Medicine the product of an amalgamation of "Occupational Medicine" and the "Archives of Industrial Hygiene and Occupational Medicine", which itself was a renamed version of the original "Journal of Industrial Hygiene and Toxicology". The first list with a preface was published in 1953. For a chronology of developments in the history of the ACGIH TLV see Table 7.1 in the next chapter.

6.3 Eligibility and Attitude

"Industrial Hygiene is a non-exact science which attempts unsuccessfully to show an exact relationship between the Utopian cohabition of Capital and Labour, from which springeth Brotherly Love", p1 (taken from a joke "Handbook of Industrial Hygiene", prepared by candidates on an early PHS training course for State IH's), 1936.

The NCGIH was set up in 1938. Membership was restricted to "2 persons (per state) employed by Federal, State, local and territorial governments who are engaged in industrial hygiene activities", p(ii), NCGIH (1938). Only members were allowed to vote or chair committees although Associate and Affiliates, people teaching IH or IHs in other countries, could be members of committees. IHs working for private companies, including insurance companies, could not join the NCGIH. In 1939, the American Industrial Hygiene Association (AIHA) was created to cater for IH's working in or consultants to private companies. Although the (N)ACGIH slowly opened up its membership rules with time, for instance by 1946 membership was opened to <u>all</u> IH's working for "Federal, State, local and territorial governments of the

[2] There was a certain amount of vying for power between committees and at the same meeting the Codes Committee presented its own list of MAC's. After discussion it was agreed that the MAC's of the sub-committee on Threshold Limits be substituted, p56, ACGIH (1946). It would appear that people within the ACGIH could see the importance of being involved with setting and defining MAC's. There was status to be gained within the profession in doing so.

United States, Canada and Latin American countries", p68, ACGIH (1946). IHs and others working for private companies have never been eligible for membership. This does not mean that ACGIH members have never worked for private companies or that it has had no contact with equivalent professionals in such companies. For instance when the AIHA was set up on 25 September 1939 four of its nine directors were members of the ACGIH. They were; J Bloomfield, F Patty, P Drinker (who also chaired the Industrial Hygiene Foundations Engineering Control Committee) and W Frederick (AIHA, 1939). But despite the obvious overlap in membership it is evident from the quotation from the joke IH "Handbook" that the conflict between "capital" and "labour" was recognised by governmental IHs and they saw the need to form an exclusive organisation, separate from the conflict. By means of this exclusivity the ACGIH has always claimed that it is non-partisan and neutral in its deliberations[3] . In fact Elkins, a founder ACGIH member and former TLV Committee chairman went further. At the 28th meeting of the ACGIH he considered the significance of TLVs. In 1948 he made it plain that he saw the MAC as being below the concentration level which would affect any worker. In this later paper he is more candid and describes the attitude of the workers (W), the employers (E) and the suppliers (S) when faced with the uncertainties of identifying the threshold level, (see Figure 1.0 from his 1966 paper reproduced below).

[3] It has also used the claim, in common with all hygienists, that its products, such as TLV's are scientific, simply technical matters.

Elkins view is worth quoting at length:

"The uncertainties due to the paucity of reliable technical information, are augmented by marked differences in philosophy, among those responsible for sponsoring threshold limits. Unfortunately, these philosophies seem to depend to some extent on the economic interest of the interested parties. Figure 1 is a cumulative probability curve representing the proportion of workers who will be affected to a given degree by a given concentration of a fume or dust. The exact point at which the TLV should be set depends in part on how severe the effect is with which we are dealing. In general, however, it is to the advantage of the worker (W) to have the limit set at a concentration where no one at all will be affected.

On the other hand, the employer (E) would prefer a limit which will protect most workers, but he would rather not undergo the extra expense of providing for the most susceptible. After all, if he has only a few men exposed, the chances are none is highly sensitive.

Finally, the supplier (S) would like to have the TLV set to protect the average worker, rather than those that are even mildly susceptible. He feels, rightly or wrongly, that a low TLV adversely affects the value of his product in the market place.

Many employers, and proportionately even more suppliers, have industrial hygienists and toxicologists who skilfully and forcefully present their points of view. The workers themselves are not so represented. **It has fallen to the lot of the governmental industrial hygienist to protect the workers' interests when TLV's are promulgated.** Many representatives of industry probably feel that government agencies side with the worker altogether too strongly". p117/118, ACGIH (1966). (Author's emphasis).

Clearly then Elkin's at least, and probably many other ACGIH members[4] regarded themselves as holding the ring between competing interests who would want to dilute the effectiveness of the TLV. For Elkins, and almost certainly others, the ACGIH was there to "protect the worker's interests".

[4] For instance, in a discussion of the health effects of benzene and whether or not to reduce the limit to 35ppm a Dr AV Nasitir made the following statement towards the end of the discussion:

"It seems to me the primary purpose of this group or of us as industrial hygienists is to protect the worker and there is a danger that while the experts are consulting and discussing the ethics of the matter, the patient may die. I think it is far safer, if there is any doubt, to reduce the maximum allowable concentration. You can always revise it upward, but if there is any doubt, it should be decided in favour of the worker", p17, ACGIH (1948).

CHAPTER SEVEN
THE ACGIH TLV

This chapter charts the development of the TLV Committee and TLV; it examines the numerical increase in TLV number and its international influence. It looks in some depth at the descriptions and definitions of the TLV which have been put forward by Committee members and the various descriptions given in the prefaces to the TLV list since 1953.

7.1 Chronological development of the TLV Committee and TLV

7.1.1 Introduction

The ACGIH was founded in 1938 and the sub-committee on Threshold Limits was set up in 1942. Table 7.1 lists the chairmen of the sub-committee, the dates over which they held office and the total duration of their periods of office. Table 7.2 lists significant developments in the history of the TLV Committee and the TLV. The chronology of events in this latter table gives a good feel for the development of the practice of the committee. Together with the next two sections (7.2 and 7.3) one can see the increasing national and international influence of the TLV and how the Committee responded to external events and influences. It is also clear that the process of the Committee has been a learning process and practice and definitions have changed, in one instance, several times in the last five decades.

7.1.2 Commentary on the TLV Committee and the TLV

The following section leans heavily on Tables 7.1 and 7.2 and anticipates some of the points which will be amplified in Sections 7.2 and 7.3.

Table 7.1 TLV Committee Chairmen

Date	Name	Period of Office (years)
1942	Dr WG Frederick	5
1947	Dr LT Fairhall	4
1951	Dr WG Frederick	1
1952	Dr AL Coleman	10
1962	Dr HE Stokinger	15
1977	Dr H Elkins	4
1981	Dr V Carter	5
1990	Dr E Mastromatteo	6

Total number of years = 51

Table 7.2 - Chronological Development of the ACGIH TLV Committee and TLVs (together with notes taken from the annual transactions)

1938	**National Conference of Governmental Industrial Hygienists founded. Committee on Technical Standards set up, chaired by Theodore Hatch.**
1940	**Dr W Frederick's question (see text) stimulates a debate on the need for a committee to set "safe limits or threshold limits".**
1941	**Committee on Technical Standards split into two sub-committees one of which was called the sub-committee on Threshold Limits.**
1942	**Dr W Frederick appointed chairman of the sub-committee.** List of limits used by "State Units" prepared.
1945	**No NCGIH meeting or transactions.**
1946	**NCGIH renamed the ACGIH. Frederick presents the first list of 144 "Maximum Allowable Concentrations of Air Contaminant".** The list was published in the transactions and, until 1950, in the Industrial Hygiene Newsletter of the US PHS.
1947	**Dr LT Fairhall (US PHS) becomes chairman of the sub-committee.**
1948	**Name of the limits changed from MAC's to Threshold Limit Values (TLVs).** No reasons are given in the transactions but Stokinger who succeeded Fairhall said later that his predecessor had not liked the term MAC as he felt it was "inexactly descriptive", STOKINGER (1972a).
1949	Committee on Hygiene Standards of Exposure at the 9th Congress on Industrial Health criticises ACGIH TLV's on a number of counts: (i) the name MAC or TLV is misleading, they preferred "hygienic standards" (ii) they wanted "the full text of the report and not just the table". (iii) they did not like the publication of the TLV's in the US PHS's IH Newsletter because it gave the standards "quasi legal" status. SMYTH et al (1950) (ACGIH never changed the name and Documentation was not published until 1962, they did take action on one of the criticisms).
1950	**TLV list published in Industrial Hygiene and Occupational Medicine.** This was a newly formed journal and gave TLV's a much wider audience. First documentation of 25 TLV circulated internally.
1951	**Development of a TLV for noise starts. Chairman starts specialist sub-committees.**
1952	**A. Coleman becomes chairman of the committee.** Survey of limits used by 57 IH units reports back; out of 48 replies, 38 use TLV's and the rest are strongly "guided" by the values.
1953	**First TLV preface published.** Second batch of documentation for 25 substances circulated internally. They were combined "and made available to members of the conference". Twenty two substances requiring TLV's are put forward by members of the conference. Over 250 requests for one or more reprints received during the year.

1954	**"Air pollution" statement added to preface and tentative list started.** Stokinger presents his first paper on OEL's as a member of the TLV sub-committee.
1955	Sub-committee emphasises that TLV's are 8 hour time Weighted Averages. 150 organisations and individuals request > 1,000 reprints.
1956	A survey of the manner in which 56 governmental IH units use TLV's reveals 28 used them as "guides" and 13 to formulate regulations. A lack of uniformity between states "... led to some confusion, especially in cases where companies operate plants in several states ..." Over 3,000 reprints distributed. Committee membership increased from 5 to 8.
1957	
1958	**Preface to TLV list rewritten completely.** Work on documentation proceeds. 50 IH units canvassed on the need to reduce the trichloroethylene TLV from 200 ppm.
1959	**"Permission was granted in response to request from the British Embassy to publish the list in a table to be included in a new publication of the British Department of Safety, Health and Welfare".** List also now published in American Medical Association Archives, IH Digest and the ASHRAE Guide. Work on documentation continues.
1960	Documentation ready.
1961	**Skin (S) notation introduced.** First publication of TLV's in booklet form.
1962	**H Stokinger becomes chairman of the Committee whose membership was increased from 8 to 14. 3 carcinogens listed separately for the first time with no TLV numbers** (Appendix A). **Siliceous dust formula introduced after much discussion. 1st edition of TLV Documentation published.** Gases, vapours and dusts combined as a single list.
1963	Joint meeting held with ASA's Z-37 committee to iron out definitional difficulties. ASA agreed to clearly list TWA and ceiling values and ACGIH agreed to add "ceiling limits where appropriate". Appendices on "TLVs for Mixtures" and "Bases for Assigning Limiting "C" Value" added.. "c" Notation added to 27 substances.
1964	**AIHA formally approach ACGIH to set up a joint TLV committee. Offer is rejected. Explanation for assignment of "c" values given.**
1965	**Appendix D and E listing inert particulate and gases added.** The final draft of "Principles and Procedures for Developing Experimental Animal Data for Threshold Limit Values in Air" is finished. Pennsylvania has set various short-term limits. ACGIH resolves to do so also to prevent, "... a chaotic situation, which could arise if each state adopts different limits for the same substance, a condition that existed for the TLVs two decades ago".
1966	**Second edition of the Documentation published.** IHF and the IMA (Industrial Medical Association) volunteer to "... serve as the anonymous repository of information (from industry) that would be of help in developing TLV's". Members of TLV Committee attend IHF and IMA meetings. " "Excursion Factors" Extended to entire TLU List.
1967	**Notice of Intended Changes (NIC) introduced.** This replaced Tentative Values and a substance stayed on NIC for 2 years before moving onto the main list. Proposal for NIC came from IMA a year earlier. Concern expressed "that the newly proposed criteria and standards for air pollutants are so low ... that ultimately they will undermine the TLV's for industry".
1968	**The vast majority of OSHA standards which became the Federal exposure limits under Section 6(a) of the OSHA Act, 1970, were based on the TLV list of 1968 (minus Appendix A on carcinogens).**

	NIC on 16 substances sent to MCA, AIHA, IHF and ACGIH stimulated, "A flood of communications ..." Stokinger and others attend a 1 1/2 day meeting on TLV organised by the IHF. Dr S Roach from the BOHS attends. Comments and suggestions were incorporated in the agenda of the next committee meeting. The IHF repository has yielded very little data: "The response has been disappointing". Numerical values of old TLVs remain on the main Recommended List.
1970	Asphyxiant gases given the "E" designation.
1971	Airborne contaminants and physical agents TLV's combined in one booklet. Ad hoc committee on STEL's set up. TLV's adopted by OSHA published in the Federal Register.
1976	**First tentative Short Term Exposure Limits (STEL's) listed for 450 substances.** STEL, "was in fact a ceiling value (not a TWA) that could not be exceeded for any time period up to 15 minutes..."
1977	**H Elkins becomes Chairman of the TLV Committee.** STEL's deleted for 141 substances.
1980	**Appendix D on Excursion Values removed.** The need for a universally applied rule was untenable and had been superseded by specific STELs.
1982	**Definition of STEL changes to become: "A 15 minute time weighted average exposure ..."**
1983/4	**The idea of excursion limits is reintroduced but with a different definition:** "Short-term exposures should exceed 3 times the TLV-TWA for no more than a total of 30 minutes during a working day and under no circumstances should they exceed 5 times the TLV, provided that the TLV-TWA is not exceeded". **The first Biological Exposure Indices (BEI's) are published.**
1985/6	A further 182 STEL values withdrawn.
1986/7	**E Mastromatteo becomes Chairman of the Committee.**

How the limits were to be set and defined and how the Committee should operate was another matter. The early discussions and MAC/TLV definitions are revealing and are examined in more detail in Section 7.4. What is clear, from the chronology of events in Table 7.2, the changes in Committee Chairman in the early years and the production rate of TLVs (see Section 7.2) is that it took some years for the Committee to get into its stride.

The Committee was strongly influenced by the standards developed and used by individual states and the standards recommended by earlier investigations such as P Drinker and H Smyth (Jr). The first pronouncement on MACs (as they were at first called) consisted of a list of standards used by different US states. And the first list, presented at the conference at its eighth meeting and published in 1946 was a composite of current state standards and the comprehensive list published by Cook in 1945. In its early days the Committee was not so much setting standards as selecting standards from existing lists. It was strongly influenced by the past work of other hygienists, and, as will be shown later by the theoretical and practical approach of these same people. As one would expect the ACGIH Committee adhered to the OEL paradigm which crystalised in the USA in the 1940's. The Committee

has interpreted the paradigm in different ways at different times an area which is explored further in Section 8.4.

The dissemination of the early MAC/TLVs was restricted to conference members. They were published in the ACGIH Transactions and the US PHS IH Newsletter. Even so, they were picked up quickly and used by IHs working in other countries.

In 1948 the name of the limits was changed from Maximum Allowable Concentration (MAC) to Threshold Limit Value (TLV). The significance of this change has already been discussed in Chapter 10. The Committee adhered to the OEL paradigm from the start and was named the "Sub-committee on Threshold Limits". The use of the term MAC shows the origins of the limits first put forward by the Committee, ie. State MAC lists, and an older pragmatic absolute approach to standards derived from the work of enforcement agencies and engineers looking for "bench marks". By changing the name in 1948 the Committee transmuted a motley list of limits based on health effects and judgements of practicability and reasonable practicability into something else. The transformation took place as the OEL paradigm became consolidated in the minds of the IH profession in general and the ACGIH Threshold Limit Committee in particular. The Chairman, Fairhall, may also have been concerned that the limits his committee recommended were not confused with and seen as the same as ASAs MACs.

The list of MACs/TLVs was published for seven years with no explanation of their status. In 1953 the first preface was published. Documentation for some of the substances on the list was available and was circulated internally within the Committee and to other individuals in the conference. However, full documentation for all TLVs was not published until 1962. Almost from the start the Committee was criticised by IHs for only producing tables of figures with no explanation of the status of the limits or guidance on how they should be used; Smyth being one of the first, SMYTH (1949). Similar criticisms have more recently been levelled by hygienists in the UK concerning the Control and Recommended limits produced by ACTS.

The Committee has refined the TLV over time. In the early 1950's it was a single figure which referred to a TWA measurement and people were warned not to extrapolate the limit for use in judging ambient air quality. Later, starting in the 1960's, skin notation, ceiling values and STELs (Short Term Exposure Limits) were introduced.

By the mid-to-late 1950's the international use and status of TLV's had been secured and is covered in Section 7.3. The ACGIH TLV Committee's initial audience was the membership of the Conference and other hygienists. The TLV thus gained professional backing and there were no effective rivals in the USA or indeed in the Western world until the 1970's. Hygienists in North and South America naturally followed the USA (most of them had been trained there), and enforcing agencies such as the

factory inspectorate in the UK and in certain states in Australia looked to the USA and therefore the TLV, when they came to recommended exposure limits.

In 1960 many but not all TLVs were incorporated in an amendment to the Walsh-Healey Act of 1936. Companies engaged in interstate business involving contracts greater in size than $10,000 had to abide by the standards ..." unless they (the work) was undertaken in strict conformity with prior written approval of a qualified industrial hygienist ..." WALSH-HEALEY (1960).

The incorporation suddenly increased the status of the TLV which changed, for the first time at the federal level, simple recommendations to legally enforceable exposure limits. The Act also increased the status of "qualified industrial hygienists". Stokinger, the Chairman of the Committee remarked that for the first time industrialists started to take notice of workings of the ACGIH TLV Committee. It was criticised, amongst many other things, for being too small and in 1962 its size was increased from 8 to 14. During the 1960's various other innovations were introduced to facilitate the Committee's relationship with industry and increase the acceptance of TLV's by industry. Stokinger wrote a guide on the use of animal data in setting standards and described the "Modus Operandi" of the Committee. An attempt was made to collect anonymous data from industry via the IHF and, in 1967, the Notice of Intended Change (NIC) was introduced which allowed industry and other interested parties 2 years to discuss a proposed limit with the Committee before it was moved to the main list. Industry made use of the NIC and Stokinger describes, in some detail, how particular industries lobbied the Committee (see Appendix 3).

The AIHA had, from 1958 onwards, at the instigation of Henry Smyth started producing "Hygienic Guides" which included some documentation. They may have been used by the Association's membership but they never gained state-wide or international recognition. When the Walsh-Healey Act incorporated the TLVs it must have been clear to the AIHA that their standards were never going to be able to compete with ACGIHs. In 1964 the AIHA formerly approached the ACGIH to set up a joint TLV Committee run by both organisations. The ACGIH rejected the offer and encouraged the Association to submit evidence to its TLV Committee if it wished to be involved.

Following discussions with the ASA Z-37 Committee ceiling values were added to some TLVs in 1963 and in 1965 the Committee resolved to set Short Term Exposure Limits (STELs) similar to those that Pennsylvania had just started using. The Committee decided to do this to prevent "... a chaotic situation, which could arise if each state adopts different limits for the same substance, a condition that existed for TLVs two decades ago". ACGIH (1965).

The importance of uniform State-wide standards, is clear to see. As with MACs, the ACGIH follows on from local State initiatives with the perceived need to maintain an inter-state uniformity of standard. It also incorporated the idea of a ceiling value from the ASAs standard setting committee, Z-37. In the event it took the TLV Committee another 11 years to introduce STELs. In 1976 450 substances were given tentative STEL values. One year later 141 were withdrawn followed by a further 134 in 1974 and 182 in 1985. When the STELs were first introduced, in 1976, they were ceiling values, not to be exceeded within a 15 minute period. This definition was unworkable and after criticism from hygienists the second definition was adopted. Six years later, in 1982 they became TWA values measured over a 15 minute period. The development of the STEL will be considered again a little later. The next development which deserves highlighting is probably the most significant development in the history of the ACGIH/TLV.

In 1970 the OSHA (Occupational Safety and Health Administration) Act was passed which allowed, within a 2 year period, that:

"... after such data, by rule, promulgate as an occupational safety and health standard any national consensus standard ... In the event of conflict ... the secretary shall promulgate the standard which assumes the greatest protection ..." OSHA (1970).

OSHA incorporated most of the 1968 TLV list, minus the carcinogens in Appendix A, as Federal exposure limits in 1971 (Federal Register, Vol 36, No 105, 29 May). The OSHA Act also allowed the Secretary of Labor to set new standards via a rule making procedure. As this procedure is so lengthy (only 11 have been set) it means that the vast majority of OSHA exposure limits are still, officially the 1968 TLVs. While this would imply that later TLVs were downgraded, in practice, for the reasons discussed in Chapter 6, this is probably not the case.

As Figure 7.1 shows the overall rate of production of TLVs did not fall off in the 1970's although production did falter from 1969 to 1972 and was more erratic than in the 1960's. There was discussion within the Committee of the role of TLVs and some individuals felt that with the advent of NIOSH there was no role for the ACGIH. Yet the TLV Committee has continued and indeed flourished. In 1977 it even contemplated making a take-over bid for the NIOSH and OSHA standards. In a paper entitled "The TLV Committee in the Era of OSHA, NIOSH and TOSCA" an anonymous author proposed that "ACGIH take the initiative" and put forward the idea of a "National Commission on recommended guidelines for exposure to chemical substances at the workplace "and the present TLV Committee with minor changes could form the basis of the proposed commission" ACGIH (1977). Needless to say this proposal, if it was ever formally made, was not accepted. Perhaps it was simply an exercise in kite flying but its existence indicates the confidence that at least one member of the Committee had 7 years after the OSHA Act when it became clear that the rate of production of OSHA standards would be low and ACGIH still had an important standard setting role.

7.1.3 Interim Conclusions

The ACGIH has not been original in the type of standards it has set. Most of the initial 146 MACs were a collation. STELs were first set by Pennsylvania State IH department. The ASA Z-37 Committee was the first to use ceiling values and a joint meeting the two organisations held convinced ACGIH to follow suit. The ACGIH insistence that the TLV referred to an average concentration over an 8 hour day or 40 hour work week is not original, but it has been a consistent refrain and is probably the part of the definition of the TLV which is most associated with the ACGIH.

The Committee has been noticeably slow in responding to suggestions and criticisms from within the IH profession and also slow to introduce innovations. Smyth penned cogent criticisms in 1950 and 1956 taking the committee to task for the name TLV, for publishing a bald table of figures with no explanation and for making extravagant claims. Documentation took 7-8 years to prepare and was not published until 1962. The name has never been changed although, as the TLV definition did mellow and move away from the early more absolute claims.

Other examples of slowness to act include the introduction of STELs which were first suggested in 1965 but not introduced until 1976. And biological TLVs which Stokinger was keen on promoting were first discussed in 1968 and draft descriptions and definitions were circulating in 1970-71 (STOKINGER, 1971b), yet Biological Exposure Indices (BEIs) were not published until 1984.

It is also clear that Committee learnt as it went along and the introduction and use of ceiling values and STELs are good examples. ASA's Z-37 Committee convinced ACGIHs TLV Committee that certain rapidly acting substances needed absolute limits above which no exposure should be allowed. The TLV Committee introduced ceiling values for some substances in 1963. But although the idea is sound the definition used by the ASA and the ACGIH has always been faulty. It refers to "instantaneous concentrations" of a substance and as any measuring system has a response time the term is in effect meaningless and the ceiling value, as defined by ACGIH, is unusable. STELs were introduced in 1976, 450 all in all and 141 were withdrawn in 1975, a year later. When first introduced the STEL definition was a confused mixture of a ceiling value and a TWA and, as with the official ceiling value definition, was unusable. It took six years for the STEL to be redefined as a 15 minute TWA which is a realistic measurement period that hygienists and other investigators could actually work to.

The unoriginality, the slowness and evident learning process are signs of which the causes lie in the size, structure and status of the ACGIH. The Conference has always been a small, largely voluntary organisation spread thinly across a geographically large country. The TLV Committee rarely meets

face to face and the various sub-groups and sub-group chairmen have a lot of autonomy. Decisions are taken at intense one or two day meetings before the Conference's annual meeting. The Committee has few resources to call upon and has relied heavily upon local state support, the co-operation of industry and more recently the support of NIOSH and OSHA where many of the key conference personnel are or have been (NIOSH was the Division of Industrial Hygiene pre-1970) employed. Looking back over the history of the TLV Committee there have never been the personnel, time or resources to respond rapidly or discuss or research policy options in depth. Important changes have relied upon the commitment, knowledge and judgement of individuals. The ACGIH TLV Committee has thus, in a sense, developed its policy and definitions on the hoof. A larger organisation with greater resources might have acted more quickly and would have presented a more polished product and image. The slowness of the TLV Committees process and the changes described may have been, and may still be, confusing for the individual new to the organisation but at least they have the merit of being fairly open and relatively public.

Whatever the organisational limitations of the TLV Committee there is no denying that for a small, voluntary body it has been dogged and productive. The Committee and the TLVs it has produced have had a major global impact and carried a version of the OEL paradigm throughout most of the Western world.

The next two sections examine the numerical increase in TLVs and the international impact of the TLV Committee and the TLV.

7.2 Number of TLVs

Table 7.3 lists the number of TLVs published each year and the number revised up to 1970. After this year revisions and additions to the list were lumped together and it is difficult to extract figures for those limits which were revised.

Figure 7.1 shows the change in TLV number since 1946 and when each chairman was appointed. Figure 7.2 based on Table 7.3 shows the percentage of TLVs which were revised each year, up to 1970.

Table 7.3 Number of TLVs 1946 - 1987

(Chemical substances only)*

Date	+/-	Nº revised or added to NIC	Nº of TLVs (total)	Notes
1946	-	-	144	Siliceous dusts = 3 values Chairman - Frederick
1947	+12	0	155	Chairman - Fairhall
1948	0	13	155	
1949	+1	0	156	
1950	0	1	156	
1951	0	1	156	Chairman - Frederick
1952	+5	0	161	Chairman - Coleman
1953	+6	5	167	
1954	-1	4	166	Tentative values for new limits
1955	0	0	166	
1956	+51	1	216	
1957	+24	7	240	
1958	0	2	240	
1959	+11/-1	3	250	
1960	+18/-2	1	266	
1961	+3/-1	10	268	Skin notation added
1962	+14	10	282	Chairman - Stokinger Carcinogen Appendix added. 4 substances - NO TLVs given. Silica formula first introduced.
1963	+14	42 (24 = C 18 true revision	296	"C" notation added (24 substances values). + App C = Excursion factors)
1964	+7	9 (7 "C" values)	303	
1965	+12(42)	14**	345	(+ 30 inert gases/dusts)
1966	31*	6**(19)		(Book = 31, booklet = 33)
1067	20	8**(12)	404	**"Classified as Revisions" in NIC
1968	32	13**(5)	434	
1969	13	7(11)	447	
1970	-2(12)	11(4)	446(453)	Revision + addition lumped
1971	-7(13)	11(22)	438(466)	
1972	12(13)	18	430(479)	
1973	29(28)	14	479(507)	Std's being revised and moved to NIC original figure to brackets. In 1974 no figure given but substance listed! - Very confusing
1974	-9	41	468	
1975	+47	27	515	
1976	+74	27	596	
1977	17	20	613	Chairman - Elkins
1978	11		624 estimate	
1979	11	61	635	
1980	23	44	658	
1981	-44	53 (9 actual additions)	614	Chairman - Carter
1982	14	17	628	
1983	4	6	632	
1984	4	27	636	
1985	1	7	637	
1986	0	14	637	Chairman - Mastromatteo
1987	0	11	637	

* Includes inert dusts, asphyxiants and Appendix A substances which were assigned TLVs.

Figure 7.1. Change of TLV number with time. .

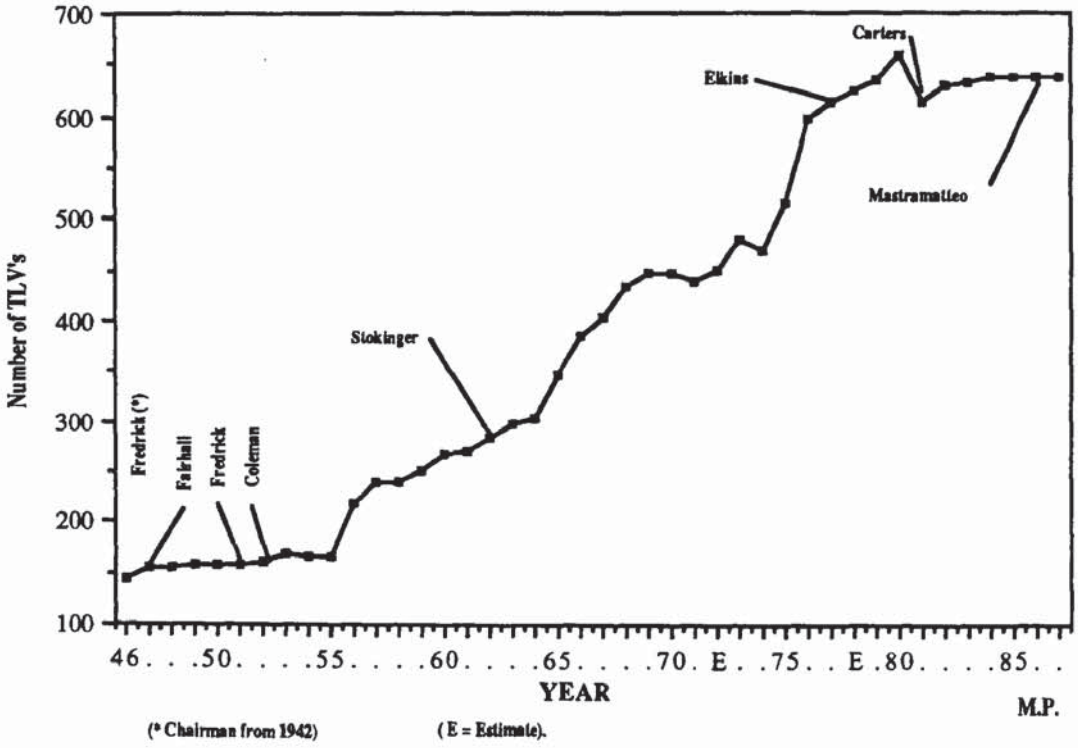

Figure 7.2. Percentage of TLV's revised 1946 - 69.

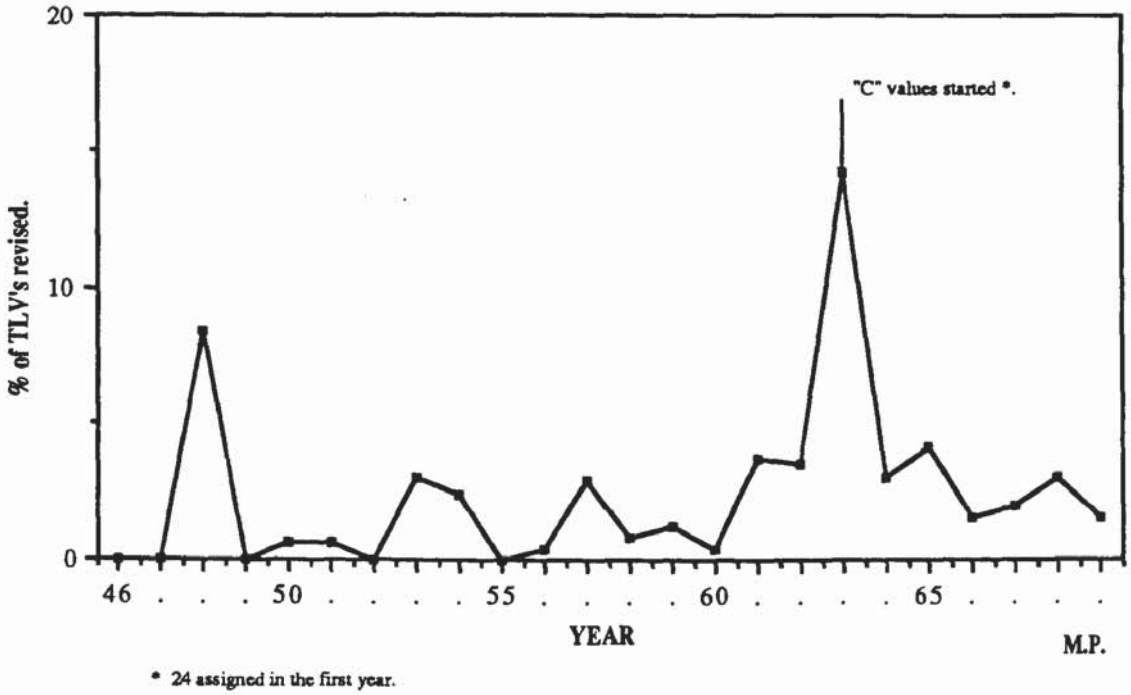

Figure 7.1 is divided broadly into 4 phases:

1946 - 1955
1955 - 1964
1964 - 1976
1976 -

Before examining these phases, the overall productivity of the Committee will be examined. Up until 1955 productivity was relatively low (~ 2.4 TLVs/year). After 1955 there was a jump in productivity which was sustained for the next 25 years, though it has dropped off and plateaued out more recently. This pattern suggests that it took a number of years for the Committee to develop its confidence, method and individual commitment. As was described in Section 7.1 the initial standards published were not so much set as collated. And it is clear, from the relatively rapid turn-over in chairmen that finding individuals who were suitable, willing and available to serve on the Committee was, at first, difficult. The period up to 1955 was a time when the Committee had not fully sorted out the methods by which it would set TLVs.

At first sight changes in productivity would not appear to be related to the appointment of individual chairmen. But, in the case of Coleman and Stokinger there would appear to be a lag of two or three years and then productivity takes off. These two individuals oversaw the two periods of greatest Committee productivity. Stokinger's period of terms was the most productive overall at 22.1 TLVs/year and Coleman's was the second most productive at 12.1 TLVs/year. The jumps in productivity are not simply related to individual chairmen. Coleman arrived towards the end of a period of consolidation as the Committee found its feet. While, Stokinger arrived the year before the Committee was expanded in size from 8 to 14 members and two years after TLVs had had their first official federal state recognition. This recognition stimulated interest in TLVs and this interest coupled with the character of Stokinger probably accounts for the jump in productivity. There is a noticeable decline in productivity in Stokinger's mid-term, from 1968-1974. This is probably a reflection of two processes: the individual work commitments of Conference Committee staff running up to and immediately after the OSHA Act and the establishment of NIOSH and OSHA, and a temporary loss of confidence and direction on the part of the Committee.

Since 1980 the Committee would appear to have run out of steam: productivity over Elkin's and Carter's periods of office fell to 0.25 and 4.75 TLVs/year respectively.

Turning to Figure 7.2 two observations can be made. Firstly, the number of TLVs revised in any one year has never greater than 10% apart from 1963 when 24 ceiling values were assigned, in addition to 18 other revisions. On average the percentage of TLV's revised from 1946 - 1970 was 2.6% per

year. Secondly, a cycle of revision with a variable period is evident. This suggests that revisions were treated as discrete projects and fits in well with the view that the Committee was small with limited resources. Revision were not be done on a continuous basis but only in certain years with rests in between. It is important to remember that in 1946 the whole Committee only numbered 5 in total, it increased to 8 in 1956 and 14 in 1962.

7.3 International Influence of TLVs

ACGIH TLVs have been, and still are, the most influential OELs in the Western world. They were taken up unofficially and then officially by many countries in South America and Europe and their use also quickly spread in Canada, Australia and Japan. Table 7.4 summarises information on the first use of TLVs and Appendix 5 provides more detail, including, in most cases, a summary of the development and use of TLVs and OELs in each country.

Table 7.4 - Use of TLVs in Other Countries

(See Appendix 5 for details)

Country	Date of first use - unofficially	Date of first use - officially	Notes
Australia	1948	1956	
Belgium	?	?	
Canada	1946 (?)	1971/2	Different territories took up
Denmark	Early 1960's	1968	
Holland	Mid-1950's (?)	Mid 1950's (?)	
Finland	Late 1950's	1962	
France	(1960's ?)	-	See Appendix 5
Italy	1969	1975 (?)	See Appendix 5
Japan	1952	1952	
Norway	1950	1950	
South America	Late 1940's	1950's (?)	Bloomfield describes the training of IH's for various South American countries, ACGIH (1984a)
Spain	1962	1962	
Sweden	1950's	1969	
Switzerland	1946	1946	
United Kingdom	Early-mid 1950's	1960	
West Germany	1958	1958	

Apart from the countries listed in Table 7.4 many developing countries have adopted some TLVs via the International Labour Offices (ILO) Model Code of Safety Regulations which adopted TLVs in 1949 MENDELOFF (1988). Supporting this view is a review of the use of OELs in developing countries Noweir states that "... there is a tendency to use the USA Threshold Limits as the most acceptable criteria for the control of industrial exposures". Because they are regarded as "realistic thresholds" unlike the "Russian criteria" which would result in the "over protection of health", p142, NOWEIR (1975).

In the late 1940's Bloomfield went to various South American countries as an adviser and trainer. Many government and local state employees would have been trained as IHs in the United States. And, it is clear that close links were made between the (N)ACGIH early in the life of the Threshold Limits Committee. The same year that the first MAC list was published Conference membership was opened up to all those working in "the United States, Canada and Latin American countries". ACGIH (1946). Such countries would, at least unofficially have used ACGIH TLVs. The logo of the ACGIH would appear to suggest that the Conference, like the US Federal state, has always regarded South American countries as within its "sphere of influence".

The summary data in Table 7.4 shows a general pattern: TLVs were used unofficially in the 1950's, and in some cases even earlier, and then their use was officially sanctioned in the late 1950's or early 1960's. After adopting the TLV list some countries supplemented the list with a few of their own OEL's and after an additional period many countries, particularly in Europe set up their own standard setting committees. West Germany did so in the late 1960's after using TLV's for about a decade; in the case of the UK it took over two decades. Even when nominally independent of ACGIH influence it is clear that most, if not all, countries follow the TLV Committee and produce lists whose OEL values vary little from TLV values COOK (1987). Some countries acknowledge this influence publicly others do not, thus authors in Italy, Switzerland, Denmark and Japan admit that the majority of their OELs are TLV influenced or derived whereas for instance West Germany and the United Kingdom do not. The question of the way individual national OEL committees track ACGIH TLV values is examined in more detail.

The use and influence of ACGIH TLVs in other countries parallels in many instances their use and influence in the USA. They were used unofficially at first and then gained official acceptance. Coleman, chairman of the TLV Committee from 1952 - 1961 provides a summary of the position in 1952. The year before, 57 State IH units had been canvassed, and, out of the 48 who responded 38 used only the current TLV values and the other 10 units were guided by them, ACGIH (1952). They gained significant Federal recognition when incorporated in the Walsh-Healey Act in 1960 and further Federal recognition when OSHA adopted the bulk of the 1968 TLV list in 1971. And the parallel continues, in that once an alternative official state administrative mechanism had been set up to develop OELs, on paper at least it might be thought that TLV's had had their day and would be supplanted. In reality, as in other Western countries TLV's still have a large influence and the ACGIH TLV Committee still wields a very significant power over the OEL standard setting process in the Western world.

7.3.1 The IH Profession, the OEL Paradigm and the ACGIH TLV

Having charted the use and development of TLVs in other countries it is worth asking a question: Does the acceptance of the OEL paradigm and the use of OELs require the existence of an IH profession?

The short answer to the question is, no; other professions and/or organisations will accept the OEL paradigm and will use OELs. This answer needs some qualification however as it is clear that some countries adopted TLVs far more readily than others. It would appear that the OEL paradigm and OELs gained faster, more widespread acceptance, if an IH profession or some equivalent profession or group, even if small, existed. This provides a good explanation for the difference evident between France and West Germany. In France although some environmental engineers use TLVs there is no formal acceptance of the validity or use of OEL's as a method of limiting risk to toxic substances. The medical profession more or less completely dominate the field, or using a term from Part 2, the "ecology" of occupational health. This dominance has inhibited, if not completely prevented, the development of IH or a professional group which overlaps with IH, and OELs have therefore never had professional support. In West Germany, in contrast, although there is no IH profession equivalent to that found in the United States there are at least two coherent professional groups with status and power where fields of interest overlap with IH and who have accepted the OEL paradigm and the setting and use of OELs. These two groups are the safety engineering profession and the toxicology profession.

A second question which is worth asking at this point runs as follows: Is the OEL paradigm and the OELs used in other countries the same as that promulgated by the ACGIH?

At first each country embraced the TLV list and its associated paradigm but in some cases there was a idiosyncratic national way of defining and using TLVs. Thus they were referred to at first in the UK as Maximum Permissible Concentrations (MPCs) and by definition therefore as ceiling values. It took some time for the correct ACGIH "line" to be established. Later, as various countries moved to be, formally at least, independent of the ACGIH subtlety different OEL's and reinterpretations of the OEL paradigm became evident. Thus, in the case of Control Limits in the UK and TRKs in West Germany the social, and more importantly, the economic influence on these OELs are made explicit. There is no contention, as for instance there is in the case of ACGIHs TLVs for carcinogens that the OELs are set purely on the basis of scientific evidence.

Finally, one can also view OELs as simply a tool with which a company, or industry or country define and limit the amount of effort (time, money and planning) which is expended on controlling a toxic substance. In this context, the OEL paradigm offers the promise that the OEL (derived from a TLV) represents no or little health risk and the actual number implies that a uniformity of international standards represents equivalent control costs. Thus, national governments in responding to public concern about toxic substances in the environment and at work, which grew in the 1960's and 1970's by adopting TLVs can be seen as aiming for economic parity as regard to the costs of controlling toxic substances at work, with the major economic power in the Western World, the USA. For the ACGIH, it was a matter of being in the right place, at the right time. The OEL paradigm developed in the USA, interpreted by the ACGIH TLV Committee and defended in the 1970's by key US IHs rode on the back of the widespread use and acceptance of the TLV list. The role of the professions discussed earlier in this sub-section needs to be seen in this wider, economic, context.

7.4 Descriptions and definitions of the TLV - contradictory claims and mixed messages

7.4.1 Introduction

From the examination of the works of key individuals which were examined in Part 3, it is clear that the OEL paradigm can be interpreted and viewed in a variety of ways. This flexibility is entirely in line with Kuhn's view of how a paradigm holds together a scientific community, yet at the same time allows novel interpretations, which, in turn, allows scientific problems to be solved.

This section is concerned with the way the ACGIH TLV Committee has defined, interpreted and reinterpreted TLVs and the OEL paradigm since the Committee was formerly set up in 1942 (see Table 7.2). As has been shown, the TLV and the descriptions and definitions the ACGIH has used have been, and still are, enormously influential. An explanation of how these definitions have evolved has relevance not just for a better understanding of the TLV but also for a deeper understanding of other national systems of OELs.

7.4.2 Committee Annual Reports and TLV Prefaces

Each year from 1946 onwards the Threshold Limit sub-committee chairman presented a report to the annual conference meeting. The reports reveal an evolving definition of TLVs and provide clues as to how the Committee members viewed TLVs in practice. The following analysis is based on these reports and the various TLV list prefaces. Reports and prefaces up to the late 1960's will be examined as by this time the definition of the TLV had stabilized, see Table 7.5, and, also by this time, most, if

not all, foreign countries had incorporated the TLV list. The first preface, published in 1953 is reproduced in Figure 7.3 and will be referred to regularly in this section.

Figure 7.3 - The First TLV list preface published

THRESHOLD LIMIT VALUES FOR 1953

Adopted at the Meeting of the American Conference of Governmental Industrial Hygienists, Los Angeles, April 1953

"Values are given in the following tabulation for the maximum average atmospheric concentration of contaminants to which workers may be exposed for an eight-hour working day without injury to health.

These values are based on the best available information from industrial experience, from experimental studies, and, when possible, from a combination of the two. They are not fixed values but are reviewed annually by the Committee on Threshold Limits for changes, revisions or additions as further information becomes available. Threshold limits should be used as guides in the control of health hazards and should not be regarded as fine lines between safe and dangerous concentrations. They represent conditions only within which it is felt that workers may be repeatedly exposed, day after day, without their health being adversely affected. It is felt, at the present time, that workers should not be exposed to a working environment containing any of these substances in excess of the value indicated".

*Published in "Industrial Hygiene and Occupational Medicine", 1953, Volume 4, 296-298.

Table 7.5 Changes to TLV List prefaces 1953 -

Date	Notes
1953	**First Preface published** in ACGIH Transactions and Industrial Hygiene and Occupational Medicine.
1954	Air pollution proviso added.
1956	TWA defined more exactly. Short term excursions above TWA discussed.
1958	**Preface completely rewritten. The words "nearly all" are introduced** into the definition and the need for persons trained in the field of IH is emphasised.
1963	"C" values defined, mixtures discussed. Use of TLV's in assessing continuous exposure discouraged.
1964	**Preface completely rewritten.** First paragraph, defining the TLV, remains the same from this data onwards. Sentence emphasising that it is "enlightened IH practice" to work to below the TLV included.
1967	Paragraph on hyperreactors introduced.
1969	Stokinger (the current chairman) makes reference to two of his recent papers which review evidence of genetic "hypersusceptibility". Paragraph argues that such tests will increase the "coverage" of the TLV's. Sentence concerning "enlightened IH practice" rewritten and made more explicit - all contaminants should be controlled to levels which are ... "as low as practicable".

1973 Pennsylvanian Short Term Limits (STL's) mentioned but not listed. Biological Limit Values (BLV's) described and their usefulness discussed.

1976 Rearrangement of definitions to include STEL's.

1981 Discussion of BLV's extended.

1983-84 Excursion limits more exactly defined.

1984-85 Biological Exposure Indices (BEI's) first listed and described and defined in a separate preface.

1985-86 Additional emphasis on the need for TLV's to be used by people trained in industrial hygiene.

7.4.3 Definitions

When Fredrick was appointed to be chairman of the Threshold Limit Sub-Committee in 1942, he issued a statement which clarified the sub-committee's objectives. He said that the committee would issue "working limits" based on the best evidence available, each year and went on:

"A plan of action which this committee might follow to advantage would be similar to that of the **International Committee on Atomic Weights**, which makes annual revisions, incorporating or considering new information which has appeared during the year", (Fredrick, 1942; quoted in PAULL (1984)) (Author's emphasis).

His choice of analogy is striking especially in the light of later statements made by him and other chairmen of the Threshold Limit Sub-Committee. The statement suggests he viewed threshold limits as akin to Atomic Weights. Given that he was a chemist by original professional background and had not been involved in standard setting perhaps this is understandable[1] . By the time he presented the first ACGIH list of MACs four years later, his claims were more carefully worded:

"Dr Fredrick: Considerable difficulty attends the fixing of satisfactory values for maximal allowable concentrations of chemicals in respirable atmospheres because of the lack of sufficient toxicological data and the lack of a uniform definition of the maximal allowable concentration concept. One concept is that the MAC value should represent as accurately as possible that concentration at which a worker exposed for a sufficient period of time will just escape physiological or organic injury and occupational disease. A second concept is that the MAC should represent some fraction of that concentration which will injure the worker in order to allow a margin of safety in the design of protective equipment and guard against possible synergistic effects in the case of multiple exposures. A third concept is that the MAC should perform the functions of the former concepts and in addition provide a work environment free of objectionable but non-injurious concentrations of smokes, dusts, irritants and odours. Obviously all of these concepts cannot be fulfilled with the establishment of a single value. MAC values in use at the present time represent examples of all of these concepts.

The committee feels that the establishment of dual lists or a single definition of the MAC is not possible at the present time". ACGIH (1946).

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1 Unfortunately there are still many chemists nowadays who still regard TLVs as something akin to atomic weights.

Frederick thus did not regard TLVs as based on some uniform practice. For some there was no margin of safety and people "just escaped ... occupational diseases". For others there was a 'margin of safety' whereas other TLVs protected against 'objectionable but non-injurious concentrations'.

A year later Fairhall, who took over from Frederick, as Chairman of the Committee on Threshold Limits gave a fairly lengthy report to the Conference, ACGIH (1947). He dealt with the same problem of definition as Frederick had:

"There are many terms applied to the meaning of the expression "threshold limit". Some consider it as a "toxic limit"; others include the expression "nuisance values", while still others go further and feel that we should include "sensory perception" in our definition. **As I view the matter I would define the maximum allowable concentration value as "that amount of gas, vapour, fume or dust which can be tolerated by man with no bodily discomfort nor impairment of bodily function, either immediately or after years of exposure"**, p43, ACGIH (1947), (Author's emphasis).

Fairhall's definition is absolute; TLVs are health based OELs which not only protect the exposed individual's health now but also offer protection over many years of exposure. Fairhall's view of what a TLV should be follows Fredrick's earlier views and their views are clearly expressed in the TLV definition included in the first published preface in the TLV list, Figure 7.3.

However, it is also clear that Fairhall understood that the TLVs put forward by his Committee were going to be used and enforced by State IH units and the question of practicability had to be addressed. He dealt with this question later in his 1947 report and again the year after. In 1947 he said:

"There is one further point I should mention, although it is rather obvious. **That is the necessity of preserving a balance between a suitable maximum allowable concentration value and the effect of attaining this value upon the manufacturing operation or process itself".** It not only requires a most careful and thorough study of plant conditions, but also requires the exercise of very mature judgement", ACGIH (1947) (Author's emphasis).

After his report there was some discussion and KM Morse, who was chairman of the Executive Committee and became, a year later, the chairman of the Industrial Ventilation committee, made a statement and asked a question which cut to the nub of Fairhall's statement:

"I think that we have all come to interpret these various terminologies, toxic limits, MAC values, threshold limits, as meaning much the same thing. I don't know whether the report of the committee intended this list of limits to be based on physiological response or whether they are limits of suggested levels of good engineering practice. Would you say a word on that, Dr Fairhall?"

Dr Fairhall replies:

"I haven't given much thought to the separation of the various values", ACGIH (1947)

A year later Fairhall returned to the question of balance:

"In view of the annual revision of threshold limit values, it has been the purpose of the Conference to seek values which, on the one hand protect the individual workman, and on the other would impose no impossible burden on the manufacturer. This balance is difficult to achieve", ACGIH (1948)

A little later he goes on to make a statement of faith which underlay his view of TLV setting and which Stokinger was to repeat, in very similar words, in the 1960's:

"There is no industrial poison so potent, so virulent, that it cannot be manufactured safely under carefully controlled conditions".

Fairhall goes on to elaborate his general view of the use and handling of toxic substances in industry:

"The manufacture and widespread use of such a substance as lead tetraethyl and the preparation of extremely poisonous war gases are extremely good illustrations of this. In fact, the chemical manufacturing industry in general has been able to maintain a high standard of safe industrial practice by the installation of suitable control measures ... There is no reason why such safe industrial control measured cannot be extended to industry in general whenever industrial poisons occur", ACGIH (1948)

At first sight Fairhall's position would appear to be self contradictory. On the one hand, TLVs were set to protect people's health but on the other they were also set with a view to what exposure levels could be achieved in industry, ie. on the practicability of the standard. His belief may have been not so much that the practicable levels achieved now were safe but that practicable levels were possible which were safe. Hence Fairhall's emphasis on the need for, "a most careful and thorough study of plant conditions" and Morse asking the explicit question; Which ones are based on "good engineering practice"? Fairhall's reply suggests that he did or could divide, some at least, of the TLVs on this basis. There remains then the question of how Fairhall reconciled these apparently contradictory positions in his own mind. He did so because he ascribed implicitly to the OEL paradigm and in fact outlines in his 1948 report the first two tenets of the working definitions of the paradigm explored in Part 3:

1 Toxic substances can be used safely.

2 IH control methods are adequate for all toxic substances.

These are the key to the explanation of how Fairhall reconciled the apparent contradiction. He believed that no substance was too toxic that its effects could not be prevented by the application of standard IH control techniques and that TLV values were set at levels which required the application of such techniques but no more. Looking at this view from the control direction he was saying implicitly; if standard, and perhaps rigourous IH control techniques, are applied to the control of toxic substances then the resulting levels of exposure will not constitute a significant risk to health.

Fredrick, Fairhall, and other chairmen of the TLV Committee including Elkins have never said this explicitly and yet it is the only explanation which allows one to reconcile statements which say that TLVs protect the health of workers and that they are practicable standards for industry to attain. And this would appear to be the view of conference members generally. In a survey of 50 state IH units on the 200ppm trichloroethylene TLV: "... 23 indicated that the present value was satisfactory **and practical** ...", ACGIH (1958) (Author's emphasis).

7.4.4 The Message of the TLV List

"... it is a figment of the imagination to think that we can set down a precise limit below which there is complete safety and immediately above which there may be a high percentage of cases of poisoning ...", p30, ACGIH (1948)

Up until 1953 the TLV list was published with no comment. It was presented as simply a list of TLV values in parts per million (ppm) or milligrams per cubic metre of air (mg/m^3). It was originally intended mainly for use by State IH units but was quickly picked up by others in the USA and abroad. State IHs might have been expected to know that the limits referred to 8 hour TWA's but others could not know that and the original name, Maximum Allowable Concentrations (MACs) certainly suggests that the ACGIH OELs were limits on peak and not day long TWA exposure. When the limits were eventually described in 1953, (Figure 7.3) the question of peak or TWA was still left open to ambiguous interpretation, a point I will return to. The limits are described as based on "industrial experience" and/or "experimental studies".

"... not fixed values but reviewed annually ..."

"to be ... used as guides in the control of health hazards and should not be regarded as fine lines between safe and dangerous concentrations".

Offering protection to people exposed daily at the TLV value "... without their health being adversely affected".

What is the message in the way the TLV lists were presented and the way they were first described?

Firstly, for those who were or are convinced of the need or utility of OELs, a bald table of values conveys a certain message and confers a certain status on the figures. The reader or user has to take them at face value and is almost bound to ascribe a greater precision and status to the numbers than if the whole process by which the figures were arrived at was laid bare. For those few people who attended the annual Conference meetings there would be some information but not much. And from 1950 onwards documentation on some of the list was compiled and was available to Conference members. But, for most users, until 1953, there was only a simple, list of numbers.

Almost from the start the Committee was criticised, and not simply by people or groups sniping from the sidelines, for publishing raw tables of figures. Henry Smyth (Junior), a firm supporter of OELs made some very cogent criticisms of ACGIH TLVs. He objected to tables of figures with no information on how the Committee arrived at the numbers listed saying that; "No oracular or *excathedra* statement on health deserves serious attention", Smyth (1956).

Earlier in 1949 Smyth had chaired a committee of the 9th Congress of Industrial Health on "Hygiene Standards of Exposure" along with, amongst others, Cook, Drinker and Stokinger. Smyth objected to the words "maximum" and "allowable", arguing that they were misleading. And, Yant, a year earlier, made a similar point. He felt that:

"These phrases (maximum allowable concentration (toxic limit) ... convey to many persons an understanding that such standards are rather precise critical values just below which a person is safe and above which he will be injured ...", Yant (1948)

Smyth reiterated the point in the mid-1950's; "... phrases may also contain the words maximum, threshold and limit. These words all imply that below the concentration specified, human response is negligible, above the concentration it is dangerous", Smyth (1956).

A similar point was made from within the ranks of the ACGIH during a heated debate on the introduction of the silica dust formula. A Mr Barrett had "no quarrel" with reducing the limit for silica but he objected to the formula as it "... implies a certain amount of precision ... that we don't have". He went on: "... in our introductions to our TLVs we indicate that they are guides. We are not trying to pinpoint a specific number, and yet you can take that formula and come out with 22.5 or 87.3 or some other magic number", ACGIH (1962).

The problem is that by presenting a table of figures with no indication of how the figures were arrived at the ACGIH was making "ex-cathedra" statements and the numbers were almost bound to become treated and seen as, in some way, magical. And the qualifications the Committee introduced in 1953 did little to undermine this presentational effect. The words "threshold" and "maximum" are potent and imply, as Smyth and Yant both pointed out early on in the life of the TLV Committee; a dividing line. Also the absolute protection that the TLVs were described as offering reinforced this perception. This description is given in the first sentence of the preface (Figure 7.3) and to go on, three sentences later, and to state that the limits "... should not be regarded as fine dividing lines between safe and dangerous concentrations", was either ignored completely or caused confusion, as Smyth pointed out.

Presenting the OELs as tables of numbers; using words like "threshold" and "maximum", claiming that the values represent, in effect, safe concentrations and stating that many TLV's contain "safety factors" (see sub-section 7.4.5) the user is almost bound to ascribe a magical quality to the figures and

to do just what the Committee warns against and what Smyth feared was happening: below the TLV was safe and above it was dangerous.

A secondary effect of plain tabular presentation was to cause confusion over measurement. The initial name MAC and the confused description in the first preface meant that many regarded TLV's as peak values. The confusion was widespread and probably explains why the Spanish authorities could define the TLV as a peak value when the list was incorporated in labour legislation in 1961, (see Appendix 5). Papers written by hygienists in the early 1950's regularly complain of the misuse of TLVs and the incorrect measurements and comparisons people made. The Committee had to remind the Conference itself of the need to adhere to its own Manual on Uniform Industrial Hygiene Procedures in 1955. And, in the rewritten preface of 1958, attempted to clear up the confusion by stating clearly that TLVs were TWAs and then goes on to discuss excursions above the TWA. But, in a real sense, the stage was set and the confusion started with the first publication of a table of MAC values in 1946[2].

As if to reinforce and the ensure the safe/dangerous view of TLV's the Committee claimed that the values were "reviewed annually". This suggests a certain fine tuning and that each value is considered afresh each year. The reality, given the number of Committee members and the number of TLV's was that individual or small groups of substances were considered as separate projects repeated over 2-3 years as Figure 7.2 suggests. The process of change was passive and no reconsideration of a TLV would occur unless new evidence was presented to or discovered by the Committee. Describing TLVs as "reviewed annually" is misleading and suggests a far more active process than was, or is, the case.

7.4.5 Learning from "experience"

The information available to the Conference Committee on which to set TLVs in the 1950's consisted mainly of animal text data, some irritancy tests on human volunteers and the results of a limited amount of epidemiological research, mainly conducted by the PHS investigating the chronic health effects of few substances such as siliceous dusts and chromium compounds (see PHS publications list in Appendix 1.0). Yet, throughout the Threshold Limit Committee reports in the 40's and 50's and in every TLV list preface the phrase "industrial experience" or simply "experience" is used. Stokinger used it in the 1950's when describing the work of the Committee and used it regularly when defending the approach of the ACGIH and the OEL paradigm in the 1960's. The phrase has

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2 The Committee and adherents of TLVs such as State or Federal IHs, have repeatedly tried to counter the "message" of their own limits. For instance, the NIOSH training manual for occupational health staff in the late 1970's covers very similar ground to the TLV Committee of the mid 1950's NIOSH (1979). There is a strong continuity of confusion.

been discussed in Appendix 3 where Stokinger's work and ideas were considered. It is considered again in this chapter because it was used regularly by the Committee chairmen in the 1940's and 50's and is given an important and central status. It is important to get a better grasp of exactly what the phrase meant and the Committee reports in the annual Transactions provide some insight into the possible meaning of "industrial experience" or simply, "experience".

The experience of the Conference membership is a repeating theme in the TLV Committee chairmen's annual reports and the phrase "industrial experience: has always been, and still is, part of the TLV preface and Table 7.6 lists the dates and quotations in which "experience" is mentioned.

Table 7.6 "Experience", "Judgement" and TLV's

Quotations from the ACGIH Transactions and TLV Prefaces

DATE	QUOTATION
1947	"The IH is in contact with not one plant, but often a number of plants, using a given toxic substance. He knows, as no one else knows, the actual aerial concentration of contaminant encountered in practice. And he is in contact with the individuals exposed and therefore some learns whether the concentrations measured are causing any injury or complaint. His judgement and the combined judgement of this entire conference group is therefore most valuable in helping to formulate maximum allowable concentrations",
1949	"Experience of many IH's in plants where hazardous substances are in use has been very helpful ... it is to be hoped that individual members of the Conference will continue to accumulate data and to correlate these data with occupational illness so that TLV's based upon human experience will be arrived at with more precision".
1952	"Each year, IH personnel observe exposures to toxic materials in industry and the results and effects of these exposures ... This experience based upon exposure of humans under actual working conditions, is most valuable in arriving at reliable TLV's".
1956	"While animal experimentation provides valuable data ... only human experience can decide whether exposures previously reported for animals, can be applied to human workers. Because of this, the valuable experience gained by State IH units is the type of information upon which such values have been or should be based".
1958	"Threshold limits are based on the best available information from industrial experience (and) from experimental studies ..." .
1964	"Threshold limits are based on the best available information from industrial experience (and) from experimental human and animal studies ..."* .

* This form of words has remained the same to date (1988).

Fairhall, in his 1947 chairman's report gives a good description and an insight into his view of the IH's "experience" (see Table 7.6). An IH with long-standing experience of certain industries and processes knows the size and type of exposures that can occur, and may know if he or she has

approached the workforce whether workers have complaints. This information would clearly be useful to the Committee when considering TLVs for materials whose only known health effects were short-term or acute in nature, for instance irritation or dizziness. And this is the kind of information IH units offered when arguing for lower TLVs. Thus in the survey of State units, in 1957, concerning the trichloroethylene TLV, two units argued for a lower limit based on "flushing" symptoms amongst workers who drank and were exposed to 150-200ppm (they wanted a limit of 100ppm) and the reported death of a worker from acute heart failure.

With health effects which took time to show themselves (eg. pneumoconioses) or occurred frequently due to other causes (eg. sore throats, headaches, indigestion) the link between exposure and symptoms would be less clear. And with diseases such as cancer, where the period between first exposure and diagnosis could be 20+ years, no link could be made by the individual IH, or in all but the case of a very rare cancer, could the individual occupational physician make a link.

As Patty put it, when comparing the US IHs approach with the more backward Europeans:

"The shortcomings of such a plan of approach (regulations simply based on medical examinations) obviously are in evaluating the first signs of absorption - a trick physicians in the United States have not been able to turn to their complete satisfaction. **The toxic materials that give rise to recognisable, specific dependable clinical signs in advance of serious injury are disappointingly few"**, p12, PATTY (1948), (Author's emphasis).

In the 1940's, 50's and 60's and, perhaps, to a lesser extent in the 1970's the Governmental, and indeed the company IHs experience would consist of a knowledge of, "... actual aerial concentrations encountered in practice ..." and whether individuals complained of acute symptoms. If the health effect caused by exposure to a substance required years to occur or was latent in character then it would be missed by the IH. Day to day experience, or even a company wide survey would not reveal such a health hazard. Although the alert IH, who perhaps collaborated with the occupational physician, and conducted surveys would have a suspicion or inkling of a link between exposure and a chronic health effect if the link was strong and the incidence was high in the current workforce. If the health effect or disease caused workers to leave the industry, simple surveys of the current workforce would miss or severely underestimate the incidence.

A small number of surveys were conducted by State occupational health units and by similar units in some of the larger companies and concomitantly a small number of the substances on the TLV list would have been covered by such surveys.

Based on what IHs did over the period in which "industrial experience: is repeatedly offered as the proper basis of TLVs by the Chairman of the Committee (1947-1960) the conclusion is: that IHs "experience" consisted of a knowledge of processes, and exposure and the short term health effects

which resulted in complaints. Industrial physicians may have had better knowledge of more subtle, chronic conditions, but even here without carefully planned epidemiological surveys many of the connections between exposure to a substance or process and a disease would have been missed.

The "industrial experience" of the Conference membership could thus alert the Committee to conditions which resulted in short term health effects such as irritation but not long term chronic health effects. However the way the word "experience" or phrase "industrial experience" is used by the Committee in its reports and in its description of the process by which TLVs are set implies much more. Taken at face value, without considering the limited view an individual IH had, the continual repetition of "experience" or "industrial experience" suggests that the Committee would have spotted any health effect caused by a substance, if it was there to be spotted. This was not, and still is not, the case. Individual, or in many cases, collective "industrial experience" has only limited power to see the connection between exposure and health effect, if the effect is common in the general population, chronic or latent.

The conclusion then is that the repeated use of "experience/industrial experience" lulls the reader, and perhaps lulled the Committee itself, into a false sense of security. And again, as with the tabular presentation and the absolute definition in the preface, reinforces the idea or belief that the TLVs are safe levels of exposure which, in Fredrick's words, 26 years after he chaired the first TLV Committee; "... and made it possible for the IH method to prevent chronic degenerative health failure ...", p43, FREDRICK (1968).

7.4.6 The Evolution of the TLV Preface

At the start of the Threshold Limits Committee, Fredrick, the first chairman, would appear to have believed that MAC's could be set with a similar precision to atomic weights and, by implication the limits had a similar quality to them. Fairhall, the next chairman, did not go as far as this but did define a MAC in absolute terms as, effectively, a safe standard. Later, both chairmen appeared to move away from such optimistic and firm positions, discussing the various bases on which MACs/TLVs were set and considering the question of practicability. However, when it come to writing the first preface the Committee reverted to the absolute definition:

"Values are given ... for the maximum average atmospheric concentration of contaminants to which workers may be exposed for an eight-hour working day **without injury to health.** ACGIH (1952) (Author's emphasis).

This definition simply reinforced the presentational effects already discussed. It is also a sign of the Committee's and the IH and IT professions' confidence in their OEL paradigm and was shared by most hygienists at the time (see, for instance, Elkins views on MACs in 1948, in Chapter 6).

They believed in the paradigm and the project of IH and IT:

"The purpose of the industrial hygienist is no longer merely to 'lock the door after the horse has been stolen' but to anticipate and prevent harmful situations, or to control them before serious injury results" ... p15, PATTY (1948)

And these "harmful situations" could be identified and avoided by the use of toxicology the IH's industrial experience and MACs/TLVs. As the quotation at the beginning of this chapter indicates, Fredrick firmly believed, as late as 1968, that the hygiene profession's project had been fulfilled:

"TLVs have made it possible for the industrial hygiene method to prevent chronic degenerative health failure ...", p43, FREDRICK (1968)

7.4.6.1 The inclusion and meaning of "nearly all"

Five years after the first preface was published it was rewritten to cope with complaints, misunderstandings and, "apparent confusion which has developed in respect to the interpretation of TLVs", p45, ACGIH (1955).

One of these "confusions" was that below the TLV was safe and above it was dangerous. The first sentence of the new preface tries to clear up this confusion:

"Threshold limits should be used as guides in the control of health hazards and should not be regarded as fine lines between safe and dangerous concentrations", ACGIH (1958)

And the next sentence defines the protection offered by the TLVs:

"They represent conditions under which it is believed that nearly all workers may be repeatedly exposed, day after day, without adverse effect", ACGIH (1958)

The next sentence goes on to state that TLVs are 8 hour TWAs.

By rewriting and reordering the preface the Committee hoped to clear up the "confusions" which had occurred as to the status and use of TLVs. Protection is not promised to all workers but to **"nearly all workers"**. The one word, "nearly" represents an important shift in the TLV definition and marks a break with the absolute definition of the past. This sentence, defining the TLV, which was first published in 1958, has remained the same through various preface restructuring (see Table 7.5) to date (1990). The inclusion of the word "nearly" could be seen as a faltering in the confidence of the Committee in their claims they previously made for their TLVs. It all depends on how they, and others, defined "nearly". Before considering this question a short consideration of the timing of the charge is in order. Quite why the Committee made the break in 1958 is not clear. It is clear from the

Transactions that people were treating the TLV figures as safe/dangerous concentrations and as peak and not TWA figures and a clearer definition were required. On the question of the inclusion of the word "nearly" this was probably due to sustained and effective criticism by people within the IH and IT professions such as Smyth. His paper, "Improved Communication - Hygienic Standards for Daily Inhalation" has already been mentioned in this Chapter and Appendix 3. Apart from some incisive words on semantics and "ex-cathedra" statements he also addressed the TLV preface of 1956 directly and the TLV definition specifically saying:

"Careful study of the data which supports the currently accepted values suggests that no such description can truthfully be attached to most of them", p4, SMYTH (1956)

In the rest, of what is a long paper of 56 pages, he considers the evidence on which each TLV is based. He demonstrates his contention that the absolute TLV definition does not hold and that some people's health will be affected at the TLV.

It may be that this one paper by Smyth persuaded the TLV Committee to change the TLV definition. His paper was certainly regularly referred to by Stokinger in his papers in the 1960's.

As to what the words "nearly all" were meant to signify that is much more difficult to define. The word "nearly" was included because the absolute TLV definition was untenable. But "nearly" can be interpreted many ways and its inclusion in a preface which was similar in content to the first and with a tabular OEL list probably did little to change the message conveyed to most TLV users. After all, "nearly" can mean "most" and "most" can soon come to mean "all". After a while though, and especially in the 1960's people outside the IH profession did start to ask for a more exact description of what the words "nearly all" meant: Did they mean 1 in 10, or 1 in 100 or 1 in 1000? In the late 1960's organisations such as the British Occupational Hygiene Society (BOHS) started to try and define what "nearly all" meant in quantitative terms for some specific substance.

For the Committee itself the phrase probably did not have any exact meaning at first except to say that "some people" would be affected in some way when exposed at the TLV, for some substances. It also signifies that the Committee realised it was more difficult than had been believed in the early days of the Committee to identify threshold levels of exposure. It does not mean that the Committee broke with the OEL paradigm.

It was introduced because the absolute definition of TLVs was untenable but probably, and more importantly, because of the Committees need to "balance ... a suitable maximum allowable concentration value and the effect of attaining this value upon the manufacturing ... process." ACGIH (1947). It allowed the Committee to set OELs which offered protection to workers health but which were also at the same time feasible. A point which is explored further and amplified in Chapter 8 and

9. As long as "nearly all" was not defined the Committee could rely upon most users interpreting it as meaning small to vanishingly small. And statements such as the one by Fredrick at the start of Chapter 6 suggest that many, if not all, Committee members regarded the change in wording a simple matter of fine tuning the definition. Perhaps in preparation for the imminent inclusion in Federal legislation. The fundamental quality and protection of the TLV remained untouched.

For one very influential person in the 1960's "nearly all" came to have a very special and exact meaning. For Stokinger, the longest serving member and chairman of the Committee, the people who suffered at TLV levels of exposure, which the phrase "nearly all" implies, were peculiarly different in some way from the rest of working humanity; they were genetically hypersusceptible. The development of this view has already been discussed in detail in Appendix 3.0 (section 3.2). By the late 1970's anyone who suffered ill health at or below the TLV was judged by Stokinger, to be hypersusceptible. His view, like Fredrick and Fairhall's before him, was that the absolute definition of the TLV was correct. The problem was not with the TLV's but it was with certain genetically different individuals. He added specific sentences listing the sort of tests that could be done to screen for certain hypersusceptibilities in the 1967 and 1969 prefaces. Once these tests were applied and the hyper-reactors were weeded out the "coverage" of the TLVs would be complete. His view was not shared by his predecessors and contemporaries such as Smyth, Hatch and Elkins.

Since 1958 the preface has evolved: Stokinger added material on hyper-reactors; ceiling values and skin notation were introduced; later STELs were added and more recently BEIs. But the fundamental definition of TLVs has not materially changed since 1958. There is strong continuity between the sub-committee on Threshold limits of 1942 and the TLV Committee of 1990. The admission that "some people" will be affected can be seen, especially in the light of the vigorous paradigm defence of the 1960's and early 70's, as a matter of adaptation. The OEL paradigm remained undamaged.

7.4.6.2 "Experience" and "Fine lines"

The major emphasis in this sub-section has been on what the "experience" of the IH profession could tell it about the health effects of toxic substances at work. The "experience" of the profession has also probably had other important effects which explain the cageyness with which hygienists on the TLV Committee and the profession as a whole have approached the single figure TLV.

The Conference, ever since the first TLV preface in 1953 has warned against treating TLVs as fine dividing lines, and, from the second preface in 1958 and increasingly so from then on, has also emphasised how the TLVs should only be interpreted by people qualified and experienced in IH. They have never said explicitly what connection there is between these two statements though the implication in, for instance, much of Stokinger's writing is that the toxicological basis and soundness

is assessed by the experienced IH. Considering the day to day practice of an IH a different and additional and perhaps more plausible, interpretation of the connection between "fine lines" and IH "experience" is possible.

The IHs "experience" of the work environment from the earliest days of the profession to the present is that it is very variable. There is considerably interday and intraday variation in sampling results from the same place or individual. This knowledge would, and still does, have two effects on the IHs view of OELs. Firstly, the idea of setting an exact OEL based on limited exposure data looks difficult, if not impossible. Secondly, enforcing an exact OEL, given the inevitable variability of the work environment, begins to look like a nonsense. The IH knows from his or her experience that, even without any of the other qualifications, the variability of sampling results means that any OEL can only be an approximation.

This, almost "tacit knowledge", buried within the consciousness of the "experienced" IH may in fact be the main reason why the ACGIH has warned against treating TLV's as fine lines between safe and dangerous concentrations. This "experience" does not mean that there is a break of faith with the OEL paradigm, in fact quite the reverse. The OEL paradigm tells the IH profession that thresholds exist and can be set by their Committee. But the "experience" of the profession also says that the variability of the work environment makes the identification and certainly the enforcement difficult, if not impossible.

This knowledge from "experience" may be the major explanation for how the hygiene profession could believe in the existence of single figure OELs while at the same time warning against the error or "confusion" of treating them as "fine lines". "Experience" told the profession that in the reality of the workplace such "fine lines" could not be measured or enforced. This knowledge was, and is, probably more important to many IH's than doubts over the toxicological basis of TLVs.

7.5 Conclusions

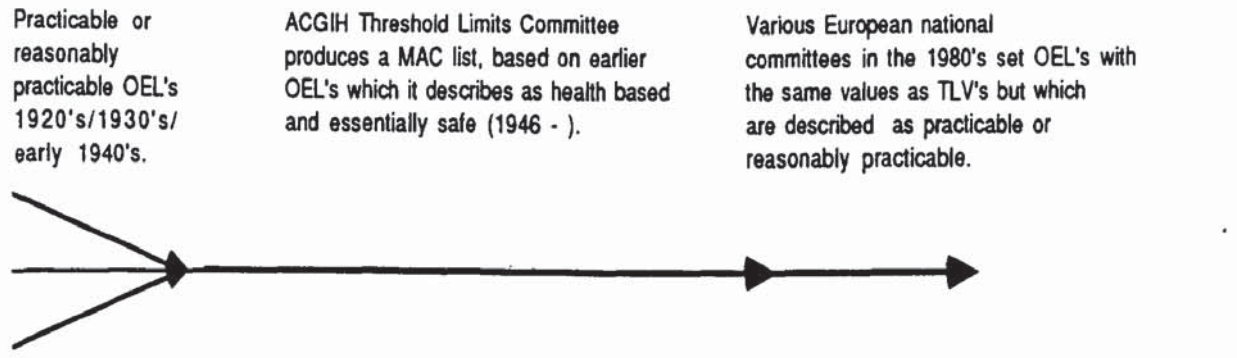
The ACGIH regarded itself as non-partisan and in fact, recognising the conflict that existed in the workplace, the TLV Committee regarded itself as protecting "the interests of the workers". Being a scientific profession and not wishing to explicitly be seen to be addressing social and economic questions which, in any event would slow it down, the Conference deliberately chose to go down the technical route.

In this chapter the slow development of what has always been a small voluntary committee has been explored. The smallness and the nature of the Committee have clearly had effects on the rate of change and innovation and limited the Committee's ability to initiate change. Despite these limitations

the TLVs influence has spread far and wide especially where there has been either a professional group or groups to foster its use.

There has always been though a problem of mixed and confused messages inherent in the way the ACGIH presented and defined the TLV. These contradictions have been explored and it is clear that the ACGIH has done something more than simply set OELs to protect workers health. The evidence would suggest that the ACGIH TLV Committee took, in the early 1940's, a collection of OELs arrived at by others, some of which were "health based", some of which were set to protect health to a degree but were also practicable and some of which were simply practicable, and called them all health based MACs to which people could be exposed "... without injury to health". Throughout the 1940's and 1950's this position was maintained with an adaptation being added to the TLV definition in the shape of the word "nearly". However, in essence, the ACGIH have always projected the TLV as, for all intents and purposes, a health based, scientific OEL which protects the health of all workers except a few hypersensitives. In the 1970's and more so in the 1980's various national committees, particularly in Europe, were set up and their work has, to a degree, undermined the image and definition of the TLV. This has come about in two ways. Firstly, various committees such as the West German DFG explicitly say that for substances which are proven carcinogens there is no known threshold of effect and the only way of setting an OEL is on technical feasibility. Secondly, committees such as ACTS, the DFG and the Dutch Commission on OEL's (see Appendix 5) specifically set certain OELs on the basis of health effects and social and economic factors. The values for these limits are, in general, similar to or the same as ACGIH TLV's. And the suspicion is thus raised; similar to that discussed when various ASA and ACGIH limits were compared in Chapter 9, that the ACGIH has also, like ACTS and the DFG been taking into account social and economic factors in the setting of certain, at least, of its TLVs.

Put figuratively the position looks something like so:



Part 3.0 of this thesis explored how the industrial hygiene and toxicological professions developed and felt their way towards, what was to become the OEL paradigm. When the contents of the paradigm were examined it became clear that it contained a contradiction or confusion born of a belief

that thresholds must exist and the perception by IHs of the need to set attainable standards. The OEL paradigm does not see this as a contradiction and the problem appears not to exist. The contradiction, put succinctly, is: How can all TLVs be safe, as well, at the same time, as being practicable? This chapter has already discussed how people like Fairhall probably reconciled the two. But, turning to the TLV Committee as a whole there would appear to have been a systematic mismatch, over a number of decades, between the definitions of the TLV, particularly the first definition, what the Committee says it has done and what it has actually done. How can this mismatch have been sustained for so long? Part of the answer is to look at how the Committee has set TLVs in practice and is the project of the next chapter. However, quite a bit can be said about the practice and thinking of the Committee at this point.

Any explanation has to account for the continuity of the Committee's behaviour over several decades. It is clear then that an explanation based on the behaviour of disingenuous individuals could not hold water, particularly as it is clear that the ACGIH has, and probably still does, regard itself as the protector of "workers interests".

The best explanation for the Committee's behaviour lies in considering the OEL paradigm, the practice of IH and IT, the attitude of Committee members to science, the image and message of the TLV and the external pressures with which the Committee has had to cope.

7.5.1 The OEL paradigm, images of science and "experience"

The TLV Committee has always been a firm adherent and advocate of the OEL paradigm in its original, pure form. Although other committees, in more recent years, have modified their interpretations of the paradigm, to allow for carcinogens for instance, the TLV Committee has stuck firmly to the earlier, original interpretation formulated and espoused by IHs and ITs in the USA in the 1940's. The ACGIH TLV Committee is an organisation with its roots firmly in the 1940s.

IHs, ITs and other science based professions following the OEL paradigm would not see things this way, ie. science as based on paradigms and communities. As was discussed in Chapter 2.0, they would, and do still, see science in a far more inductive and positivistic way. For IHs, ITs and the TLV Committee science is essentially cumulative and unified and proceeds by induction based upon raw observational data or, for some by falsification of simple theories. Given this view facts are very concrete and theoretical explanations are not simply the most coherent current explanations consistent with the communities paradigm, they are the scientific truth, they are unassailable, or, as Stokinger would say "indisputable" reality. The simple, inductive, unified view of science is probably the mechanism by which people like Smyth and Stokinger transformed theoretical possibilities such as "resistant" liver cells in Smyth's case or "genetically hypersusceptible individuals" in Stokinger's case

into "scientific facts". A theory which fitted "the facts" was not simply one of a number of possibilities which could and should be subject to continued falsification tests. A theory which fitted the "scientific facts" was a "scientific theory" which the two authors cited rapidly convinced themselves were "the scientific explanation", for the "facts". A view of science which sees it as unified and cumulative will tend to regard a theory as true or false and a step on the way to the one true explanation of the "facts". All past theories are seen as wrong, as mistaken. The more modern scientists with a more Popperian view would be more tolerant of a variety of competing explanations or theories which explain or account for experimental observations, but would still see scientific work as a method of working towards the one true explanation of how the world works.

The upshot of this inductive view of science as the path to truth is that IHs in general and the ACGIH TLV Committee in particular would have regarded the OEL paradigm as the scientific and unquestionable, truth. For them there has never been any confusion or contradiction. Somehow the TLV Committee members have reconciled the contradictions inherent in the OEL paradigm. A large part of the explanation lies in the "experience" of the IH profession especially the early "exemplary" experience. Committee members know that MAC/TLVs if applied would, and indeed had, greatly improved working conditions. They firmly believe that IHs in conjunction with ITs could use animal tests to predict and prevent harm in human beings and these two professions grasped this belief to themselves as an area of their exclusive expertise. And finally the continued "experience" of the Conference IHs as has been discussed, told them that TLVs were safe.

Contributory factors which helped cement the TLV Committee's view especially as the membership turned over and new members arrived would be the Committee's own TLV definitions and the message given by the way the limits were presented. In a sense the Committee became a victim of its own propaganda.

It was also a small voluntary body which, from the early 1960's came under increasing external pressure. Partly this was from individual industries lobbying for certain TLVs but it also came as the Committee realised its world-wide influence. It must have realised in the 1960's that the numerical figures it chose could and would have a big impact. It has a huge responsibility not only to protect workers health but to chose TLVs which did not "over protect". A small voluntary organisation is in no position to stray from the accepted wisdom. Indeed as is clear from Chapter 5.0 Stokinger regarded himself almost as a defender of the faith: defending the Committee, the TLV and the OEL paradigm from the attack by the "non-toxicological" hordes.

7.5.2 The Function of "Confusion"

Through the five decades of its existence the Committee has regularly tried to clear up the "confusions" which people or organisations have about TLVs. My argument here is that the "confusion" is inherent and no amount of explanation will clear it up. However, for a scientific profession like IH the "confusion" also has a functional, though not consciously created, purpose. Because of the "confusion" it is possible to argue, and the Conference has always done so, that only professionals can assess the imponderables and difficulties when putting TLV's into practice. And they do this based on their qualifications, ie. their membership of the profession, and their "experience". The "confusion" surrounding TLV's has been a good thing for the IH profession.

CHAPTER EIGHT

THE PRODUCTION OF TLVS

8.1 The OEL Paradigms and Practice - Introduction

In Part 3 the development and interpretation of the OEL paradigm was explored. The analysis showed that IHs and ITs can operate in two parallel and sometimes conflicting modes: as both scientists and professionals. The main difference between the two being that the latter have definable client groups. The professional pressure, both external and derived individually from a persons conscience, to take account of the needs of the client groups, varies depending on the client groups involved. Because of such needs and the practical problem solving ethos of the IH profession, questions of practicality, cost and social issues became embedded in and integral to the OEL paradigm. The conclusion was that IH and IT subsume economic and social issues within what, at first sight, would appear to be the actions of applied scientific professions engaged in a scientific process.

This part of the thesis has explored the development and workings of the TLV Committee, the way the Committee has defined and presented TLVs, the international influence of the TLV Committee and how the Committee appear to have reconciled the needs for practicable standards and to protect workers health. The confusion embedded in the OEL paradigm and the tensions of IHs as both scientists and professionals is clear to see. There is still the question of how the Committee has worked in practice and exactly what sort of process TLV setting is.

This chapter is concerned with these broad practical issues and will examine other particular items of interest such as: the decision making process of the Committee; how substances were and are chosen; the relationship of the Committee to Federal, Local and International organisations; the relationship with other professional organisations; the influence of industry, the influence of individuals and other issues.

The chapter is organised as follows: The Minutes of the TLV Committee are described, analysed and discussed. The results of this analysis are further discussed in the context of other researchers work and the earlier analysis of the OEL paradigm and the TLV Committee. Finally, some concluding remarks are made pulling together the threads of analysis of the influence and the actions of the TLV Committee and the ACGIH.

8.2 Analysis of TLV Committee Minutes

The Minutes of the TLV Committee from 1962 to 1984 were made available to me by Mr William D Wagner during a visit to the ACGIH and NIOSH in Cincinnati in 1984. In all 31 sets of Minutes were made available ACGIH Minutes (1962-84). They are more or less complete for the 1960's and 1970's but there are gaps particularly in the 1980's. An estimated 10 sets of Minutes are missing from the archival material.

In the early 1960's the Committee met for two days a year once a year, almost invariably in Washington. From 1966 the Committee met twice a year for two days and increased in size. Specialist working groups were set up. More recently the Committee has been able to call upon the resources of the Conference's Executive Secretary, W Kelly and business is more carefully managed. The majority of Committee members were located in the Eastern State of the USA perhaps reflecting the old geographical pattern of industrialisation and Washington was a convenient meeting place. More recently the venue has been in a more central city reflecting the more geographically dispersed location of Committee members. Also, since about the mid-1970's the Committee has increasingly used outside consultants to supplement its expertise. Some of these changes will be discussed further.

The Minutes are remarkably consistent in style reflecting the continuity of recording secretaries. William Wagner undertook this task from the late 1950's until 1976 when he was replaced by Leonard Pagnotto. The Minutes cover the two days of the Committee meeting and are about eight pages in length. They record the people present, the issues discussed and the substances considered for revision or addition. Sometimes the positions that various members took are recorded and occasionally, especially when consensus could not be agreed, the arguments for and against a particular TLV are listed. Actions to be taken by a particular person are recorded after each substance and may include a reminder that so-and-so company or person should be contacted or so-and-so has agreed to write the documentation. An example of the ACGIH TLV Committee Minutes is given in Appendix 4.

The Minutes were combed for two exact types of information. Firstly I noted each time the Committee was approached by an outside organisation, usually a company, with a suggestion for a TLV or with information on which a TLV could be based. Secondly, I noted where the Committee contacted an outside organisation, usually a company, for information on or experience with a particular substance. In this way I wanted to get a quantitative estimate of the factors which have

influenced the setting of TLVs over a 22 year period. All such instances are presented in Table Appendix 4.1 and the frequency by various sub-categories are presented in the Table Appendix 4.3.

Secondly, the Minutes were read to determine how the Committees policy and process had changed over a 22 year period in an attempt to gain a greater understanding of the practical issues raised at the start of this chapter. The results of this selection process are presented in Table Appendix 4.2.

8.2.1 Factors Influencing TLV Setting Process

In 1962 there were about 282 TLV's and in 1984 there were about 636: In the 22 year period covered by the Minutes about 354 TLV's were agreed and published by the Committee.

Table App 4.1 lists chronologically excerpts from the Minutes describing the instances where the Committee was approached by an external organisation or sought information from such organisations. One hundred and eight such instances are recorded. Most concern industrial companies, either producing or using a substance, but there are instances where initiatives have come from other organisations and agencies such as ANSI and the UK Factory Inspectorate. When the Committee wished to get information from a company it usually approached an individual hygienist or physician known to one of the Committee members. In the case of large companies such at Dow and Dupont the same contact names crop up repeatedly. The Committee has clearly developed a network of reliable contacts in particular companies. The 108 instances were further categorised according to how the information affected the Committee deliberations.

In 38 of the 108 instances the data supplied by industry appeared to be the main source of information on which the TLV was based. In 9 instances it is clear that the industry concerned determined the TLV level; the industry's recommendations being accepted without reservation by the Committee. There are 4 examples where the Committee ignored particular companies objections. There were only 3 explicit instances where the Committee follow the range of exposure levels in industry though there are many instances where the "industrial experience" available to the Committee would contain this information.

Allowing for the missing Minutes the results of this analysis were corrected (see Note 7, Table App 4.3) and I estimate that of the 354 TLVs set between 1962 and 1984, in about 40% of the cases industrial data was sought or supplied and in 13% of the cases the TLV was based mainly on data supplied by industry. These percentage figures are corroborated by a recently published study, CASTLEMAN and ZIEM (1988). In this study the authors examined the 1986 TLV Documentation

noting instances where the Committee had ".....placed important reliance on unpublished corporate communications....." CASTLEMAN AND ZIEM (1988). They found 104 instances on which, ".....important or total reliance was placed....." In 1986 there were 637 TLVs. Thus, according to the authors about 16% of all the TLVs were strongly influenced by industrial data. Although the authors judged the influence of industrial data with a different criterion and by means of a different route, they arrived at a similar percentage figure. The type and quality of data supplied by industry are discussed in the next sub-section.

The influence on industrial representation and industrially supplied data has been touched on earlier. This analysis gives some quantitative feel to the scale of the influence. It also indicates that in somewhere between 60% and 87% of cases the TLV Committee would appear to have set a TLV or altered a current one on the basis of either information in the open literature and/or the special knowledge of individual Committee members. For instance, the results of toxicological work in NIOSH (and the PHS IH Division before it became NIOSH) would have been available to Stokinger, indeed he directed this effort for many years. This does not mean that the Committee took no account of the practicability of the TLVs. The literature, particularly the IH reports, would give a clue as to likely current exposure ranges. The Committee has always believed that its efforts will protect the health of "nearly all" and are, at the same time, feasible. What the analysis shows is that in between 13 and 40% of cases industry determined or influenced the practicability judgement and in 60-87% of cases the Committee had other means of making this judgement.

8.2.2 ACGIH Standard Setting, Policy and Process

In addition to the analysis of the Minutes described in the previous section, the minutes were searched for items which related to the workings of the Committee, its policy and process were noted. All such items in the minutes are listed chronologically in Table App 4.2 together with comments.

In the following sub-section some broad behaviour patterns and qualities of the Committee based on Table App 4.2 are discussed.

(i) A Voluntary Committee

"..... you realise that we do all this in our, call it spare time, most of us don't have time ..... during our working day ....." WAGNER (1984).

The TLV Committee has always been small and voluntary with, until recently, little administrative support. The ACGIH relied upon the goodwill of the PHS which allowed the Committee to meet on its premises, released personnel for meetings and sanctioned the use of its library and postal services. Also, limited secretarial services were available to type Committee Minutes and organise mailouts. For the first three decades of its existence the PHS and NIOSH gave the Committee more or less wholehearted support, and the Committee relied upon this tacit subsidy. This is why it came as such a shock to the Committee when the director of NIOSH, Dr Finklea, forbad NIOSH employees to use NIOSH facilities or do ACGIH work during working time in 1977. Not only was this a shock in terms of lost resources but it must have been an even more fundamental shock to these Committee members who had previously worked for the PHS before it became NIOSH. Up until 1977 and certainly up until 1970 when the OSHA act was passed, such individuals could and did believe that the Committee's work was a positive aid to the PHSs IH effort and with the creation of NIOSH they could believe they were at least on parallel parts with similar if not identical interests. Finklea and others wanted to put ACGIH at a distance because of possible "conflicts of interest" in that the same people might be developing NIOSH Recommended Exposure Limits (RELs) and TLV's for the same substances. The edict applied particularly to W Wagner and T Lewis but it surely was a sign to the whole Committee that it was now on a different trajectory from NIOSH and OSHA. Since the Reagan administration the edict has been, if not withdrawn, much less stringently applied.

Coupled with the size is the fact that the turnover in Committee personnel has been relatively low. People come onto the Committee and tend to stay 5, 10, or 15 years: for example Coleman, Stokinger, Elkins, Wagner, McDermott, Gross, Wands, Mastromatteo and Zeim. The membership of some, like Elkins and Stokinger span a very large proportion of the Committee's existence. The long continuity of membership of this small committee has made for a tight knit and loyal group of people who trust each other to make the right decisions, and defend those decisions once made. One can only guess at the sort of group dynamics and culture that prevailed but certainly during the reign of Stokinger as Chairman, people would refer to his expertise and position.

(ii) Committee contact network

Stokinger was the head of PHS (later NIOSH) Industrial Toxicology and sat on various national committees including the ASA (later ANSI) and National Academy of Science (NAS). Other individuals on the Committee such as Elkins and McDermott were renowned IH's and the PHS at Federal and State level would have many contacts with their opposite numbers in industry. The number and variety of contacts certainly comes across in the Minutes. Whenever a new substance is considered or an old substance is reconsidered, one, two or occasionally three people are listed as

useful contacts, usually in companies using or producing the substance. It is also clear that the Committee was reliant on the services and goodwill of certain individuals in the larger chemical companies such as Dow and Dupont. The names Zapp (Dupont), Morgan (Dupont) and Rowe (Dow) crop up repeatedly and in fact in the 1970's both Morgan and Rowe joined the Committee as industrial liaison members.

(iii) Choice of substance

The problem of how to select substances for TLV development is a recurring theme. Various suggestions were put forward:

1963 The problem was specifically addressed under the heading "More systematic approach to TLV List". Two selection choices were put forward, one based on tonnage production of chemicals, the other via a survey of the ACGIH/AIHA membership.

1964 A proposal to select substances based on the US Tariff List of organic compounds plus experience of State units.

1967 Discussion under the title of "Systematic Additions to List". A proposal to select substances based on Chemical Weekly Buyers Guide, plus Merck Index, foreign publications and contact with industry.

1968 The most sophisticated scheme considered is proposed by A Hosey. He proposes selection based on five criteria:

a A population index
b An incidence and prevalence index
c A trend index of chemical usage
d A quantity index
e A disability index

By confirming the indices he hoped to develop a priority rating.

1978 Masstromatteo pointed out that the USSR had MAC's for 450 substances for which there were no TLVs. He suggested that the TLV Committee could consider those substances.

In the 22 years covered by the Minutes at least 5 proposals have been put forward as methods by which the Committee could select substances. It is not at all clear whether any of the 5 proposals were ever used. Judging from the way substances appear in the Minutes it seems unlikely. Selection appears to be relatively arbitrary and to be based on information in the open literature or via State unit or PHS reports on complaints; or well documented harmful effects; or materials which come out positive in animal toxicity tests, (although these are often tested because they are related to materials which have known toxic effects); classes of substance such as pesticides; suspect substances pointed out by epidemiology in a certain industry (rare) or occasional requests from users/manufacturers.

No standardised selection method based on the size of the population exposed, the likely health risk and the occupational health impact has been applied. Being small, voluntary and with strictly limited resources it is not surprising that ACGIH never felt able to tackle this problem. It was obviously considered but no simple plan-of-action was forthcoming.

The problem of which substance to select out of the thousands of possibilities and the setting of resource priorities generally are ones faced by all regulatory agencies. Although NIOSH made a stab at it by listing what it considered the ten most important occupational diseases, the selection of substances for which OEL's should be developed is still unresolved. It is a problem that will not go away and I return to it in the last part of this thesis.

(iv) Attitudes to carcinogens

In 1963, the year after Stokinger became Chairman of the Committee the first 4 carcinogens with no TLV number assigned were listed in Appendix A of the TLV Booklet. The number was increased to 9 in 1969. The substances were classified as carcinogenic; "Because of the high incidence of cancer either in man or animals.....".

In 1973 a change in policy occurred: 5 Human Carcinogens without TLV numbers (Category 1a), 4 Human carcinogens with TLV numbers (Category 1b) and 8 Experimental Carconigens (Category 2) were listed.

In 1975 Category 2 was changed to A2 and retitled "Occupational Substances Suspect of Oncongenic Potential for Workers" based on "limited epidemiological evidence" or animal data. The Committee felt confident of its assigned TLVs stating that they; ".....are below the threshold of response for inducing cancer in workers ....."

In 1976 a set of "Guidelines for the Classification of Experimental Animal carcinogens" was added to Appendix A by which substances could be classified as having high, medium or low potency.

The categories remained the same until 1987/88 when a much shorter and simpler Appendix A was published. Substances were divided into:

A1 - Confirmed Human Carcinogens
A2 - Suspected Human Carcinogens

The definitions are more circumspect in the recent TLV Booklet; "Where a TLV has been assigned, it does not necessarily imply the existence of a biological threshold....." and it concedes that; "Scientific debate over the existence of biological thresholds for carcinogenics is unlikely to be resolved in the near future". The Booklet cites Guidelines published in the 1986 Documentation. While carefully argued to present the problems in assessment, the role of Toxico-Kinetics, genetic and epigenetic action and other factors, the Committee does not in fact let go of the idea of identifiable thresholds of carcinogenic effect. Animal tests can still be used "....to estimate the threshold of neoplastic response....." and the last paragraph of the discussion contains the boast that; ".....only the TLV Committee provides recommended exposure limits based only on health effects". P A-10 ACGIH (1986).

From the Minutes it is possible to see when some of the changes in Appendix were initiated and who were the prime movers. In 1974 Zavon was critical of the inconsistency of the Committee's policy towards carcinogenics. By 1975 Wands had developed a 3 tier division of potency based on animal tests which was accepted by the Committee and the Conference and published in 1976. A year later in response to the approach to carcinogen of NIOSH, which did not differentiate between animal and human carcinogen, Elkins who became Chairman that year polled the Committee on whether it ".....believed thresholds exist for carcinogens". The result was that:

"The majority voted yes, but were in agreement that there may not be a practical threshold in some instances. In those instances the effect level and zero concentration may be too close to distinguish" (Item 120, Table 8.2). (Which substances the Committee members had in mind was not made clear).

Up until 1963 substances with a carcinogenic action were not treated differently from other substances with different toxic effects. But, as was seen in Part 3 the issues surrounding carcinogen and the debate which raged in the scientific and wider community from the mid-1960's, all the way through the 1970's threatened the very basis of the TLV Committees raison d'être and the OEL

paradigm to which it subscribed. Stokinger was in the forefront of the paradigm's defence and it is not an exaggeration to claim that the Committee's public attitudes and pronouncements on the subject of carcinogens were explicitly based or strongly influenced by his attitudes. This is not to say he defended it single handed – there were clearly supporters of his view on the Committee, (probably the majority). However, he was known for his strong chairmanship and the Committee members' views, as expressed in published papers, appeared to follow his closely. In the 1950's he adopted the no-threshold view for carcinogens derived from his knowledge of ionizing radiation and the views of the ICRP. He also regarded all animal carcinogens as potential human carcinogens. This cautious approach is reflected in the preamble to the first Appendix A: Animal and human carcinogens are not differentiated. However, by the early 1970's Stokinger did differentiate and the 1973 Appendix A reflects this. Also by this time he believed in "practical thresholds" for carcinogens and the first 5 carcinogens with TLVs are listed. As Stokinger's beliefs changed, as his attitudes hardened and his confidence in the defencibility of the Committee's position grew so did the published views of the TLV Committee. As was seen in Part 3 and Appendix 3.0 Stokinger was perhaps the staunchest defender of the pure form of the OEL paradigm. It would be unthinkable to believe in a no-threshold effect for a certain type of toxic action. For Stokinger to admit this would mean that his primarily "health based" TLV's would need to be close to zero and this would not only not be feasible, it would undermine the OEL paradigm and, from his viewpoint it was unnecessary.

It was only when Stokinger left the Committee that the obvious doubts that the majority of Committee members had in 1977, ".....that there may not be a practical threshold in some instances", eventually surfaced 10 years later in the more circumspect Appendix A. Even so, the belief lingers on in a more carefully argued and less dogmatic form.

(v) Judgement

The Committee rarely had enough information on which to set a TLV and the quality and quantity varied greatly. At various times this gave rise to proposals for a change in TLV definition which will be reviewed later.

The message or effect on the tabular presentation of TLVs (and other OELs) and the various TLV definitions have already been discussed. The Committee itself knew that absolute claims were unwarranted if only because of the variable information it had to use. In 1971 a proposal to add a statement to the TLV Preface was put by Caplan and accepted (see Items 39; Table 8.2). It was published in 1973 and went as follows:

"The amount and nature of information available for establishing a TLV varies from substance to substance. Consequently the validity and precision of the estimated TLV is also subject to variation and the Documentation should be consulted in order to assess the extent of the data available for a given substance".

Even so, the Minutes do not make clear how the Committee chose a particular TLV number, though the quotation implies that it is simply a matter of looking at the "data". Sometimes the TLVs arrived at were not justified by the Documentation, a point made by the ASA committee in 1970 and accepted by the TLV Committee (Item 1, Table 8.2). In other instances and, using the same Documentation the Committee favoured different TLV numbers, on different occasions, for instance in the case of benzene (Item 138, Table 8.2). In areas of scientific uncertainty where there are little data available individual judgement is likely to vary. However, what is also clear from the decision on benzene in 1978 is that even when there is a large amount of information available, judgements vary. Sometimes the process is explicit, the number is chosen first and the Documentation is written in such a way as to justify the number. The judgement is justified by a rewriting of the Documentation often recorded in the Minutes with the phrase ".....to revise documentation in support of the new/present value .....". For instance in 1974, when it was decided to lower the benzene 25ppm ceiling value to a 10ppm TWA value, Elkins was asked to "Modify Doc to support reduction". And, in 1975 when the 2- hexanone TLV was reduced from 100 to 25 ppm the Minutes note that; ".....as written, the Doc does not support a 25 ppm value.....". In other cases Committee's judgement is stark and simple thus, for instance, propylene oxide was judged to be half as toxic as ethylane oxide and TLV's were at 20 ppm and 10 ppm respectively.

In 1977, the question of how much confidence the Committee had in particular TLVs was discussed. The minutes record the specific question addressed "..... should we introduce a note on the confidence of specific TLVs"?. The issue was never resolved, though interestingly the Dutch, several years later did evolve a classification system which grades the information on which their OELs are based, AI (1986).

I am not suggesting the variability of judgement is somehow wrong. But the fact of its existence and the variable quality and quantity of data available has allowed the Committee considerable leeway in exactly which TLV number to choose. And it is perhaps at this point in the decision making process of the Committee that the question of the feasibility or practicability of a particular limit intrudes. The data do not "speak for themselves", the Committee always have a range of numbers from within which to choose and feasibility will inevitably suggest a judgement from within an even more limited range of numbers. The question of the factors which may affect an individual's judgement are

examined in the discussions section. The point I wish to make here is that "judgement" is neither a neutral word or a neutral process.

(vi) Epidemiological and Toxicological Studies

Industrial data, field surveys and epidemiological studies are important to the Committee and until the advent of NIOSH the major source of such data were the producers of the substances being assessed.

The Minutes regularly mention studies in progress, mainly animal tests but sometimes epidemiological surveys. Such studies are influential, as was shown earlier in this chapter in the discussion of Table 8.1. The point I wish to make here is that the committee's dependency on one, perhaps two studies in progress delays the decision making process. Thus the Minutes record in 1973 and 1974 how the Committee waited for the results of the Dupont epidemiology on dimethyl-sulphate exposed workers. And, in 1976 a decision on the nickel TLV was postponed pending the results of industry sponsored epidemiology on exposed workers. Waiting for results not only delays the standard setting process but it has another, perhaps more important effect. It ensures the committee puts great reliance on the studies in question, and possibly focuses their attention away from other studies and evidence.

(vii) Research

The Committee has never had a research budget. It has relied upon the results of research published in the open literature and initiatives taken by the PHS, more recently NIOSH and industry. If no information was available and no organisation was prepared to finance research then no TLV could be set. Tobacco dust is a good example. The Committee wished to set a TLV for tobacco dust and as there was no published information it approached the industry's Institute. No information was forthcoming either because the industry would not release the data or because it did not exist. Whichever was the case the Committee was not in a position to set up its own research programme and the question of a tobacco dust TLV was dropped.

The Committee was interested in fostering research. In 1963 it was suggested that ".....problems of interest to the Committee....." were publicised and very occasionally specific projects were suggested. Thus, in 1976, the armed forces requested TLVs for 3 substances, (Item 93, Table 8.2) and Wands suggested a meeting to discuss ".....the type of data/research needed to develop values." Even here the Committee is relying upon the armed forces to do or to commission the research.

The Committee, up until recently, has always had a close association with the PHS and NIOSH and, especially during Stokinger's reign, questions of interest to the Committee would become research projects in the PHS toxicological laboratories. But even given this subsidy there have always been severe limits on the number and depth of studies the PHS could do which would benefit the Committee's TLV programme. For a large number of substances the Committee has had to rely upon the information, such as it was, in the open literature and information that certain industries were prepared to release. The Committee has always been a passive user of information and not an active producer. This passivity has bred a certain dependency which has reinforced a certain Committee organisational culture which is discussed in the concluding section of this chapter.

(viii) Analogy, relativism and arbitrariness

According to Stokinger approximately 25% of TLV's are based on analogy (Stokinger 1968a). Various examples occur in the Minutes. Thus in the late 1960's the TLV for maleic anhydride (MA) was set at 0.25 ppm by analogy with phthalic anhydride (PA) which had a TLV of 1.0 ppm. Based on its irritant properties MA was judged to be four times more irritant than PA and the PA TLV was thus divided by four. Similarly the propylene oxide TLV was revised downwards from 100 ppm to 20 ppm in 1979 because it was judged to be half as toxic as ethylene oxide, the TLV of which had been reduced from 50 ppm to 10 ppm in the same year. Basing a large number of TLVs on analogies means that if a stem substance TLV is reduced, so should all the branch substances fall by the same proportion. Widespread use of analogy is a legitimate strategy in setting OELs where there are little toxicological or other data to go on. However, the practice does introduce a certain relativity into the actual TLV numbers chosen.

Apart from relativity there is a certain arbitrariness in some of the TLV numbers selected. Thus in choosing a TLV of $3mg/m^3$ for calcium oxide the Committee used the TLV of calcium hydroxide as a benchmark. Calcium oxide was judged to be more irritant than calcium hydroxide and its TLV should therefore be lower, and 3 is smaller than 5, but where did 5 come from? In all probability because 5 = 10 + 2 and 10 is the "nuisance dust" standard applied to all biologically inactive dusts. This figure itself is based on a relatively arbitrary judgement. The "nuisance dust" level used to be $15mg/m^3$. Having chosen $10mg/m^3$ as the overall top limit for particulates, all other particulate standards relate in some simple arithmetic way to 10. The actual numbers derive from a certain arithmetic neatness and not an exact toxicological judgement.

(ix) Safety factors and sailing close the wind

Safety factors have always figured in the Committee's descriptions of its own process. Stokinger referred to them regularly. In 1956, in a discussion of carcinogenisis he advocated the use of 100-fold to 500-fold safety factors when extrapolating from animals to humans, STOKINGER (1956). In his paper describing the "Principles and Procedures" of the Committee he states that safety factors ranging from 2-10 are applied to animal data depending upon the seriousness of the potential risk. STOKINGER (1965a). Seven years later he discussed safety factors again describing them as a "cushion of protection", where, "The magnitude of the safety factor depends upon the seriousness of the response." p 156 STOKINGER (1972a). He goes on to give a few examples but then ends by saying that, ".....safety factors are all judgemental values derived from the long experience of the TLV Committee members; no values predetermined according to category and degree of action are used". p 156 Ibid. This last statement contradicts the first. Perhaps the first description is what he would have liked to have done and the second is a better description of the relative arbitrariness of the process in practice.

Later in a discussion of its approach to potential carcinogens the Committee harks back to the earlier Stokinger position and states that it takes the lowest level known to cause cancer in animals and ".....divides by an arbitrary factor, such as 100 or 1000." ACGIH (1986).

Safety factors are referred to or implied in the Minutes. Examples include:

1977 Carbon disulphide. TLV = 20 ppm. "....cardiovascular effect reported at lower range of concentrations studied which start at 10 ppm". (Item 117). Safety factor unknown but not large.

1977 Ethyl bromide. TLV = 200 ppm. Little or no safety factor.

1977 Antimony oxide production. Exposure since 1961 about 0.5 mg/m^3. No excess cancer detected in workforce therefore TLV set at 0.5 mg/m^3. Safety factor unknown but not large.

1978 Nickel. Exposures in the range 0-2 - 2.0 mg/m^3. No excess nasal cancers detected in a study of the workforce. TLV set at 1.0 mg/m^3. Safety factor unknown but not large.

1979 Methyl chloride. TLV lowered from 100 to 50 ppm. Neurological effects reported at 150 ppm. Safety factor - ≤ 3.

1979 Trichloroethylene. TLV reduced from 100 to 50 ppm because of neurological effects reported at 100 - 150. Safety factor ≤ 2 3.

In his analysis of OSHAs regulatory policy on toxic substances Mendeloff points out that since OSHA adopted the 1968 TLV list ACGIH has reduced 100 of the TLV's adopted. The median reduction was 50% and in 12 cases the reduction was by 90%, MENDELOFF (1988). Keeler and others studied the changes in TLVs from 1947 to 1981. They concluded that, "Changes in TLVs are infrequent. Less than one third of the substances ever listed changed TLVs from their initial listing." KEELER et al (1982). This corroborates the findings in chapter 7. (Section 7.2). TLV's are rarely revised and when they are the evidences show that TLVs generally fall by 50% or sometimes 75%. Only when a substance becomes classified as a potential carcinogen or sensitizing agent does the limit fall by an order of magnitude. And even then this does not always occur.

This static picture with a slow rate of change of TLV values and relatively small changes when they do occur reinforces the belief that TLVs are in someway, fundamental limits and that when the Committee makes its small changes there are course corrections as they trim close to the threshold of effect. Another way of viewing this is to say that the Committee sails close to the wind. It makes course corrections if there is strong evidence of health effects occurring at or close to the current TLV value.

(x) Process based TLVs - way forward or dead end

In 1974 Stokinger introduced the idea of process based TLVs which did and still does cause much confusion. Five years later four substances had TLVs "tailored" to specific processes:

1 Antimony trioxide: Handling and use 0.5 mg/m^3 Production = 0.5 mg/m^3 A2.
2 Arsenic and compounds = 0.5 mg/m^3 Arsenic trioxide production = A1A.
3 Cadmium oxide fume (CdO) = 0.05 mg/m^3 Cdo production = A2.
4 Chromates = Soluble = 0.05 mg/m^3 Dissoluble 0.05. A1a. Chromate ore processing = 0.05 mg/m^3. A1a.

Why did Stokinger introduce TLV's "pegged to a specific process"?

There is no indication in the Minutes which record very little discussion of the issue. The underlying reason though is probably to do with the different approaches of toxicology and epidemiology.

From the very early days of the profession hygienists have worked with and relied upon industrial toxicologists to determine the harmfulness of particular substances. In the early days of the Committee there were very few epidemiological studies to look to and the Committee relied upon smaller scale prevalence or field studies such as there were. As more large scale and systematic epidemiology was done the difference between the approach taken by toxicologists and the Committee became clearer.

Epidemiologists deal with work environments and processes. Sometimes processes only produce one main contaminant in which case, if health effects are detected in the exposed population, then it is reasonable to argue that the contaminant caused the effect, especially if a close response relationship can be demonstrated. But often more than one contaminant is present or the contaminant is produced under unique conditions, for instance, high temperatures. Where a number of different potentially causative agents are present or the processes which generate a specific contaminant are significantly different, epidemiologists tend to be conservative and are often loathe to extrapolate across all processes using or producing a particular substance. This problem is particularly acute for processes which generate complex mixtures of contaminants.

The dilemma for the OEL setting committee, using epidemiological studies, is that it is usually concerned to set an OEL for a substance yet the epidemiologist can only look at processes. The conservative and most easily defended approach for the epidemiologist is to say that his or her study only applies to the process examined or exactly similar processes. This approach though is frustrating for the hygienist wishing to set a standard for a substance.

Stokinger tried to solve the conundrum by going along with the epidemiologists conservatism and set TLVs for substances but then limited the applicability of those limits to certain processes. The reality is that the approach of the toxicologist and the epidemiologist is in certain instances, irreconcilable. Stokinger confused the picture with his process limited TLVs. They are neither TLVs for specific substances because the implication is that there is something special about the process to which they are tied. Nor are they limits based on specific processes because they refer to individual substances which implies that they should apply whenever the substance is used or generated.

Process based TLVs do not solve the conundrum and in fact have probably caused more confusion. One in particular, for arsenic, certainly caused a lot of friction between the Committee, NIOSH and OSHA, (see CASTLEMAN (1988)).

(xi) OEL differences and co-ordination with other standard setting bodies

There is regular mention inthe Minutes of the need to liaise with or influence other standard seting bodied where their limits differ from the ACGIH. The chronology in the Minutes reflects the history of the various bodies.

The ASA 2-37 Committee was set up before the ACGIH Committee and various differences of approach have already been described. The differences were resolved in the early 1960's by negotiation and compromise between the two bodies. The only mention of the ASA in the Minutes is the record of a letter in 1966 telling the Committee that the ASA is thinking of setting a MAC of 10 ppm for chloroform and asking the Committee to consider reducing its TLV of 50 ppm (Item 10, Table 8.2). Twelve years later after an initial reduction to 25 ppm in 1974, the Committee reduced the TLV to 10 ppm.

In 1967 there are two separate mentions of the problem posed by low limits for air pollution set by a PHS committee, (Items 16 and 20, Table 8.2). Stokinger is recorded as saying that: "These values are so low ..... that they may ultimately undermine the TLVs ....". He wrote a letter to the Surgeon General expressing concern and suggesting a formal method of liaison be established between the two Committees.

The PHS started setting limits for environmental exposure, to some of the same substances that the ACGIH Committee had dealt with, in the mid 1960's. Both Stokinger, in the Minutes, and Hatch in his papers describe the problem and sought to justify why there were such large differences between TLV's and ambient air standards. Various explanations were and are put forward but the tension still exists nowadays between the EPA and OSHA. While ambient air standards have not "undermined" the TLVs they have had a corrosive effect on the belief in TLVs and related OELs.

The first mention of the West German DFGs (German Research Association) OEL setting Committee in the Minutes is in 1972. Stokinger appears to have been concerned that the DFG had banned certain carcinogenic substances and had written to Dr Henschler, Chairman of the DFGs MAK Committee. By 1972 the DFG had struck out on a fairly independent path from the ACGIH. For instance in 1968 it introduced its own allergy notations and in 1970 the TRK (Technical Guide Concentration) for carcinogens, (see Appendix 5 for more details). Stokinger was clearly concerned with rumours surrounding the introduction of the TRK. Henschler, in his reply to Stokinger, was able to put his mind at rest, (Item 44, Table Appendix 4.2).

It is clear that the ACGIH Committee felt it was important to co-ordinate its activities with the DFG particularly as the status and influence of this Committee grew in the 1970's. In 1974, a Dr J Knatel, working for WHO but also representing West Germany, called for the establishment of a "Cartel of European Countries" in developing OEL's (Item 63, Table 8.2). This never occurred but it was clearly a worry to the Committee. It was important that the Committee made links with the DFG, the main European alternative to the ACGIH. In 1979, Dr G Kimmerle attended the Committee for the first time and continues to be the "MAK Commission Liaison" person to the present day (1990). In April 1982 the Minutes record that "The TLV Committee has established good liaison with MAK Commission through George Kimmerle".

The ACGIH TLV and West German DFG Committees have developed a reciprocal relationship. While they do not determine the limits set by each other, they will know how certain studies are interpreted and will learn from each other. For instance, the DFG's approach to teratogens has clearly impressed the TLV Committee. The result of liaison between two groups of experts is a coming together of outlook and a tendency not to stray too greatly from the decisions of the other Committee. This "anchoring" effect on expert judgement is discussed in more detail in the last part of the thesis. The main point which I wish to make here is that there is a tendency for expert Committees to keep an eye on what other expert Committees, dealing with a similar field, are doing. There is a tendency to develop a common vision and this would certainly be reinforced by having membership overlap. In the 1970's and early 1980's one could say that the TLV and DFG Committees formed a "Cartel or two", while excluding the Swedish Committee which worked apparently, independently during the same period.

The most important differences of judgement which have occurred in the lifetime of the Committee are the differences in OELs set by OSHA and NIOSH, and TLVs. From the Minutes, NIOSH's REL's (Recommended Exposure Levels) caused more heart searching and discussion than OSHAs PELs (Permitted Exposure Levels). This would be so for several reasons. Firstly NIOSH RELs were often lower than ACGIH TLVs, secondly the REL's were produced at a far greater rate than the PELs (> 100 by 1985) and lastly key members of the Committee worked for NIOSH and were part of the OEL setting structure. In 1977 the Committee explicitly addressed the differences between RELs and TLVs for some substances. Under the sub-heading, "Reconciliation of TLVs and NIOSH" they discussed benzene, carbon disulphide and arsenic and stuck to the TLV value for each. The Minutes include the statement that, "NIOSH recommends 1 ppm because it believes carcinogens have no threshold levels." Whereas ACGIH TLV Committee believe that in practice thresholds do exist. A little later in the Minutes the results of a poll are recorded. The Committee was asked whether they believe in thresholds for carcinogens:

"The majority voted yes, but were in agreement that there may not be a practical threshold in some instances. In these instances the effect level and zero concentrations may be too close to distinguish." (Item 120, Table Appendix 4.2).

The Committees views are not homogeneous and the idea of a "practical threshold" being close to zero suggests that the difference between NIOSH's position and that of a significant number of Committee members may not have been that great. The big difference was perhaps not so much attitude to carcinogens as the remit of the two organisations. ACGIH was and is committed to setting limits which, at the same time as offering protection, are also feasible to implement. NIOSH attempts to set OELs based on health effects alone. Whether this happens in practice, or is even possible in theory is discussed in the last part of the thesis. Whatever is the case NIOSH RELs are substantially lower than ACGIH TLVs. Analysis of the 150 NIOSH limits show that 86 comparisons are possible, and among these RELs are 40% lower than TLVs, CDC (1986). A little later on in the same Minutes the Committee considered seven more substances that NIOSH had recently reviewed. In two cases the Committee decided to reduce the TLV towards the NIOSH figure (Item 121, Table Appendix 4.2). Of the other 5 substances, two were carcinogens. This and the fact that REL's are on average 40% lower than TLVs would suggest that the Committee comes to different judgements on the same evidence that NIOSH considers ie differences in approach are not confined to apparent differences in belief concerning carcinogens. The ACGIH TLV Committee comes to more conservative judgements on the likelihood of a health risk existing than the equivalent NIOSH Committees. This conservatism is almost certainly due to the constraints imposed by the needs of feasibility.

A year later Elkins, the Chairman of the Committee points out that 17 OSHA PELs are lower than the corresponding TLVs. He asks the question, ".....should (ACGIH) consciously recommend a higher level than OSHA"? This issue was not resolved apart from an agreement to add a statement in the preface to the TLV's that ".....Government regulations may differ from TLVs." The PEL - TLV differences did not seem to exercise the Committee anything like as much as the REL - TLV differences, probably for the reasons already outlined. NIOSH, in the earlier shape of the PHS IH Division, was the birthplace of the ACGIH. The ACGIH - NIOSH differences as well as being numerically large also touch rather raw nerves - it was all a bit too close to home.

The Committee noted and took account of the work of other OEL setting Committees. It did not automatically fall into line with them, in fact, it probably, given the status of the TLV, expected the reverse.

Occasionally the Committee clearly does not wish to be seen to be out-of-step. For instance in 1982 the Minutes record that the main Committee asked its Inorganic Substances Sub-Committee to think again concerning arsenic. It did not want the Conference to be out of line with both the MAK Commission and IARC over the human carcinogenicity of arsenic.

(xii) Confidence in the TLVs - the definition revisited

Ever since its inception the TLV Committee has considered and set single figure OEL's with all that that implies, (see Chapter 7). For the first 7 years a simple table of figures was produced and the definitions of what the TLVs represented was discussed at annual conference meetings. The definition which comes out of the discussion is that TLV's (or MAC's as they were called at first) both protected all workers health and were practicable. When the first definition was published in 1953 the Committee produced an absolute definition of the TLV - workers could be exposed "without injury to health". There was no mention that the TLV's were also practicable. However, at regular intervals the Minutes record a certain chafing against the blanket definitions of the TLV and on occasions there is even a call for a break with the single figure limit. The dates and subjects discussed recorded in the Minutes are listed below:

1964 Long discussion on whether the "primary effect" of a substance should be listed as Smyth suggested in 1956. Idea rejected.

1967 "Segregation of Limits". Committee opposed to listing TLV's on a "basis of comfort and/or good industrial hygiene practice".

1973 A 3 tiered classification was proposed, 8 hour TWA, 15 minute TWA and a ceiling level to protect against different effects.

1974 Discussion of the need to add Medical Surveillance (MS) notation ".....to certain substances which require MS of workers exposed to concentrations at or near the TLV".

1975 Medical Surveillance was raised again the next year, Zavon did not like it especially as OSHA's proposals on the subject were "out-of-hand". He suggested a statement be developed, ".....regarding MS with emphasis and reference to those substances which have limited data/validation for the adopted value".

1977 "Validation of TLV's - should we introduce a note on the confidence of specific TLV's? Discussion but no resolution followed".

Smyth, as has already been described, did not like simple tables of OEL's. They offended him because he understood just how inaccurate was the process by which the figures were set, and the presentation for him was almost a falsehood. In 1956 he proposed 2 levels of standard - one which would cause no injurious effect and one higher than that which should not be exceeded. The idea was that the distance between the two standards gave a measure of how strictly the first standard should be enforced. He also proposed that, in conjunction with the 2 standards, the "Most important Effect of Inhalation" should be listed, SMYTH (1956). The proposal, in 1964, that the "primary effect" of substances should be listed with the TLV's is a direct descendant of Smyth's idea.

Three years later another piece of déja vu appears in the Minutes. Someone proposed that the TLV's should be segregated into those that were based on toxic effect, those that are based on comfort and those that are based on "good industrial hygiene practice". Twenty years early Morse and Fairhall discussed a similar idea. The proposal was rejected but it does indicate that TLV's were not simply based on an assessment of toxic effects, some were based solely on comfort and others solely on what could be achieved by the then current "good industrial hygiene practice".

The point is reinforced by the discussion surrounding Medical Surveillance (MS) seven years later. The proposal was that certain substances should have an MS designation. The reason for choosing a substance would be because, as Zavon put it, there was ".....limited data/validation for the adopted value". The Committee knew or at least suspected that, for certain substances, health effects would occur at or near the TLV. To have adopted the MS designation proposal would have meant publicly admitting that the Committee knew which TLV's would or could result in demonstrable health effects. The vague, blanket phrase "nearly all" in the TLV preface would have come to mean something very concrete. Users of TLV's with MS designation would start to ask the Committee what proportion of people exposed at or near the TLV would be effected and in what ways? More serious than that, some would start to ask why the Committee was setting TLVs at which it was known health effects would occur. The Committee shelved the idea of Medical Surveillance (MS) notation.

The minutes show that that Committee did not have a uniform view of the TLVs it produced. Some were set on the basis of known toxic effects, while others were set on the basis of good industrial hygiene practice. In some cases the limit was thought to protect those exposed against any toxic effect, while in others the Committee knew or suspected that toxic effects would occur. The

Committee also had, as the 1977 quotation shows, more confidence in some limits than in others. A cursory glance at the TLV Documentation shows that data quantity and quality vary enormously. The proposal to introduce some measure of confidence was a valid move but again, as with Medical Surveillance or the proposal to segregate limits put forward 10 years earlier, it posed a problem for the Committee. As with the other proposals, to publicly admit that some limits were more valid than others would undermine the apparent homogeneity and status of TLVs. To allow the doubts expressed at Committee meetings and recorded in the Minutes to surface would have meant a major break with the past. To understand why one only has to imagine the sort of questions which would be asked if TLVs were segregated, and MS designations and confidence categories introduced. Users would breakdown the TLV list into those that were based on toxic effects and those that were not. They would ask, for those limits with an MS notation, what effects were expected at TLV levels and why the Committee had allowed this and there would be a tendency to downgrade the TLVs in which the Committee had the least confidence. The hegemony of the TLVs would have been destroyed.

Although the principle definition of TLVs has remained the same since 1964 the preface has grown in length. Some of the doubts expressed in the Minutes have echoes in carefully phrased qualifying statements, some of which were added to the preface before the discussion recorded in the Minutes. This latter point is a puzzle. The main qualifying statements include:

some TLVs protect against impairment of health "....whereas (others give) reasonable freedom from irritation, narcosis, nuisance or other forms of stress....."

And the committee accepted that some would suffer

".....discomfort, aggravation of a pre-existing condition of occupational illness." ACGIH (1964).

A year later the reader was urged to consult the Documentation to find out the type of response, " against which the limit is safeguarding the worker." ACGIH (1965).

Four years later the Committee changed the wording of the statement urging the user to get as far below the TLV as practical. The introductory phrase sums up the Committee's view of its OELs.: "In spite of the fact that serious injury is not believed likely as a result of exposure to the threshold limit concentrations....." ACGIH (1969). The message being that TLVs are basically safe. This phrase remains in the preface to date (1990).

In 1972 another qualification was added which stated that ".....the amount and nature of information (on which TLV's are based) varies....." and it goes on to suggest the user consult the Documentation. As a consequence of the variability of the information, ".....the precision of the estimated TLV is also subject to variation.....". This statement prefigures the discussion in 1977 in which it was suggested that the Committee assign a note on the confidence it attaches to each TLV. The Committee never went this far, it was only proposed to admit that information varied but it left it to the "buyer to beware".

And finally although a medical surveillance was never introduced its limited use is implied in a recent addition to the preface. Various causes of, ".....hypersusceptibility or otherwise (unusual response) "are listed together with the admission that "adverse health effects" may occur at or below the TLV in such people. The Committee go on to state that: "An occupational physician should evaluate the extent to which such workers require additional protection", ACGIH (1987). No guidance is given on how a physician might do this or for which substances it might be appropriate. It is though an interesting extension of a more generalised use of a view first propounded by Stokinger. The implication is that people who react at or below the TLV are not necessarily genetically hypersusceptible but they are, for some reason, "unusually susceptible". The formula first used by Stokinger is extended and used to insulate against the effects at or below the TLV for the susceptible. The problem is not with the TLV's it is with such unusual people.

The current preface is a more qualified and less confident affair than the first, published in 1953. But, for the reasons already stated the qualifications make little difference in the users mind or cause some confusion. Whichever is the case the basic message remains unchanged.

(xiii) Individuals

It is not clear how the Committee in the 1960's and the first half of the 1970's arrived at decisions. Occasionally the Minutes report a "consensus" being arrived at, and recommendations made by individuals appear to have often "gone through on the nod", a point that will be explored further. Somehow, on difficult issues, a consensus was always reached. The Minutes cover the period from 1962-1984 and Stokinger was Chairman of the Committee from the start of this period to 1976. For these 14 years covered by the Minutes reflect his leadership style. Stokinger brought William Wagner onto the Committee, having worked with him in the PHS/NIOSH. Wagner was then the Recording Secretary over the period of Stokinger's tenure. Wagner in describing how the the Committee had expanded and evolved, had the following to say about Stokinger's style:

".....it's more democratic now in the sense of voting than it was under Herb Stokinger's reign."

MP - Didn't there used to be voting then?

"There used to be voting but it was a sort of quasi-vote. Herb would often overrule a unanimous vote. (Laughter). Not really, but he was a very strong leader ..... it was autocratic or dictatorial or however you want to say it ..... he wielded a pretty heavy hand in terms of what the final TLV would be, but at the time he was writing most of the documentation and because of that he had a much sharper image of what he thought the values should be....." WAGNER (1984).

The strength of Stokinger's character, his evident intelligence and productivity and the strong belief, once he had made his mind up that he was right, come across in his written papers and have already been remarked up on Wagner's comments confirm the suspicion that during the period of Stokinger's "reign" the TLV Committee was very much "his" Committee. Clear evidence of his autocratic style is to be seen in the Minutes. In 1977 Elkins took over from Stokinger and in this year the first vote on an issue is recorded in the Minutes (Table App 4.2, Item 120). From this date onwards various other votes are recorded (Items 132, 133, 134, 140 and 166). In 1979 the Committee formally agreed a policy on voting; a hand vote of Committee members present and if this was tied, an official poll of all the Committee members.

The TLV Committee has always been small and voluntary. Large responsibilities devolve onto individuals to write documents and run sub-committees. Although the final responsibility for putting forward a TLV value to the Conference (or nowadays to the Executive Committee) is the Committee's, in reality it is often the individual who assesses the literature and puts forward a figure, who decides the TLV value. In theory the Conference Executive Committee or the Annual Conference meeting could overturn a Committee decision. Although some Committee recommendations have been discussed they are rarely, if ever, overturned. The influence of certain individuals over the setting of particular TLVs is clear from the Minutes. Individuals such as those listed clearly had a strong interest in particular substances and the TLVs for those substances were, in a very real sense, their creations which are accepted without discussion. The following are examples of individuals heavily influencing specific TLVs:

Caplan	-	Styrene TLV raised 50 to 100 ppm. (1969)
Mastromatteo	-	Nickel TLV remains at 1.0 mg/m^3 (see items 121 and 122, Table 8.2)
Wegman	-	Toluene diisocyanate
Amdur	-	Sulphur dioxide
Wagner	-	Nitrogen dioxide. He recommends 3 ppm which is accepted without discussion and he prepares documentation.

Where the person had no abiding interest in a substance, but had been allocated it, there was more room for debate and change. Individuals in the Committee, because of its size and voluntary status, could wield considerable power.

(xiv) Industry and the TLV Committee

(i) Lack of Data

Sub-section (xii) showed that the Committee was concerned about the validity of some of the TLV's. It has had to deal with the perennial problem that often there were no data or only poor quality data available. Stokinger described the problem in several of his papers in the 1960's and makes similar comments to the Committee in the early 1970's. In reacting to a complaint from industry that there are too few TLV's he reiterated ".....the absence or lack of acceptable data to justify establishing limits for many suggested substances. Industrial contribution of substances for consideration is essentially non-existent." (Item 33, Table 8.2).

This lack of data, whether from industry or other sources, meant that when the West German DFG examined the TLV Documentation in the early 1970's through into the 1980's it was found that, ".....less than 10% of the respective exposure limits are based on appropriate and sufficient animal tests and/or field experience." HENSCHLER (1984).

(ii) Industrial Data

As has been shown the Committee was reliant upon industrial data for a significant percentage of its TLV's and solely reliant in some cases. The quality of the data made available to the Committee by industry is an important issue.

Apart from the fact that little data from industry was forthcoming the Minutes do contain a revealing footnote. In 1969 the following reminder to Committee members was included in the Minutes; "NOTE: In requesting data from industry, attempt to get more specifics on nature of study, survey or industrial exposures." (Item 27, Table App 4.2). It has already been shown that the Committee regularly made contact with individuals working in particular industries for data. The note gives a clue to the quality of data supplied: It was often vague as to the details of the study, the survey methods used and the exposures experienced by the workers. This view is corroborated by some recent work on corporate communications. The authors of the work tried to obtain copies of internal industry surveys or communications which were cited in the TLV Documentation. Companies were

approached for specific references on 67 substances. Of the 17 corporations approached, 9 responded wholly or partially, CASTLEMAN AND ZIEM (1988). The results were revealing but often created more questions than answers. Thus one of Dow's reports on methyl chloride came to the conclusion, based on routine medicals that there was, "no evidence of over-exposure." The question that the two authors ask is, "What tests were conducted and what analysis was carried out by Dow?" Ibid. That is, what credence can be placed on such *"ex cathedra"* statements? Very little it would seem. Castleman and Ziem list extracts from the internal reports they received. Thus, Zapp, (Dupont) in arguing that methylene bis (4 - cyclohexylisocyanate) was less irritant than TDI cited a company subacute study of dogs. He argued that the negative results outweighed the positive evidence from other studies, ".....because of the greater number of animals involved". Castleman and Ziem point out that the Dupont study used 4 dogs. In other cases the "communications" referred to in the Documentation turn out to be material safety data sheets which, necessarily, consist of bald unverifiable statements. Many if not most of the studies of human health effects consist of small surveys of the companies workforce with no follow up of people who leave or retire. Such studies would be of low power and sensitivity. Most of the reports examined had not been published in refereed, peer reviewed journals.

The Castleman and Ziem study explains why a "Note" appeared in the 1969 Minutes, the author clearly felt that the industrial data the Committee was receiving was of low quality. What the study does not explain is why the Committee continued to solicit and use such data. This point is considered a little later, but first it is worth considering the two way influence of the ACGIH on industry and industry on the ACGIH.

(iii) Mutual but uneven influence

The Notice of Intended Changes (NIC) was introduced in 1967 in an explicit attempt to get more response from industry. In fact the original introduction, which was to be included in the TLV list preface, was specifically asking for industry to help and offering the promise that it could "shape the deliberations of the Committee". The introduction was never published in this form but it gives a clue to the Committee's view of the NIC, (see Item 15, Table 8.2). The first 16 substances on the NIC list were published in 1968 and resulted in "A flood of communications.....". Wagner made the following comment in a discussion of the NIC:

"If the industry recognises that this setting of a TLV may well end up as a promulgated standard by OSHA and it's going to mean a cut into the profit column you'd be surprised how often that triggers a response". They came saying, "Hey, we've got 25 years of workers exposed at a concentration

that may be double what is recommended (in the NIC) and here is the data. The Committee certainly weighs all of that and they have changed their values.....". WAGNER (1984).

The Committee thus encouraged the involvement of industry and in some cases goaded it into action by putting TLVs on the NIC list. In this sense the Committee influenced industry. But, it is also the case that industry had a significant direct influence on the Committee. Trying to gag the size of this influence is difficult. The analysis of the Minutes earlier in this chapter shows that in the case of 40% of the TLVs set between 1962 and 1984 industrial data were sought or supplied and in 13% of the cases the TLVs were set mainly on the basis of data supplied by industry. Castleman and Ziem arrive at a comparable figure of 16%. But the indirect influence of industrial data and representation may be far greater than these figures would suggest.

(iv) Consultants

Since 1971 the Committee, in the TLV booklet, have formally listed liaison people separately from members of the Committee eligible to vote. The three people first brought onto the Committee were Padden (representing the Unions) and Rowe and Torkelson (representing industry). The co-option of representatives of industry and unions was an attempt by the ACGIH Committee to become more representative and less expert. Almost certainly this was a response to the imminent arrival of the OSH act which stressed the need for consensus structures.

Up to 1975 the position did not change but from this year onwards the Committee had no Union representative. At the same time people who were not listed as Committee members were called "Consultants". Initially these consultants were in fact the old industry liaison people but as the list of consultants grew this group became more heterogeneous. More industrial consultants were brought on board but also retired full members were included: people who could not attend every meeting but who did useful work and with whom the Committee wished to maintain contact. Also, it may be that members moved to the consultants' category when they were employed by a company and were not eligible for ACGIH membership. For instance, Mastromatteo worked for INCO from 1976-1979 and was moved to the consultants list in 1977. From 1976 the affiliations and status of Consultants are not given and there is no simple way of classifying this group. What can be said, and is depicted in Table 8.3, is that the importance of consultants to the Committee grew from 1977 onwards.

Table 8.3: Consultants on the TLV Committee

Year	Nº of Committee Members Exclud.	Nº of Consultants	Consultants as a % of total committee	Comments
1971	15	3	17%	1 Union 2 Industry
1972	16	3	16%	1 Union 2 Industry
1973	16	3	16%	1 Union 2 Industry
1974	16	3	16%	1 Union 2 Industry
1975	17	3	15%	No Union 3 Industry
1976	16	3	16%	Non-members listed as consultants for this date.
1977	18	5	22%	
1978	-	-	-	Missing values.
1979	20	7	26%	
1980	20	8	29%	
1981	18	8	31%	
1982	15	11	42%*	Wagner & Lewis moved off Committee.
1983	17	9	35%	Wagner & Lewis back on Committee.
1984	16	8	33%	
1985	15	10	40%	
1986	15	9	38%	
1987	17	11	39%	
1988	18	4	18%	A response to Castleman and Ziem

* This % figure is an artifact. Wagner and Lewis were forbidden to be members of the TLV Committee by NIOSH. By listing them as "Consultants" the NIOSH edict was circumvented.

The proportion of consultants on the Committee increased from a steady 15-16% from 1971-1976 up to a maximum of 40% in 1985. In 1988 the figure fell to 18% and was probably a response to the Castleman and Ziem paper which heavily criticised the Committee for the use of unacknowledged industry based consultants. Not all people listed as consultants were industry based but it is probably fair to say that the majority fell into this category. Some have certainly had a great deal of influence over the setting of certain TLVs. The person allocated responsibility for evaluating the literature and writing the documentation on a particular substance was in a powerful position. This person would invariably recommend a certain TLV and, being familiar with the literature, would be in a good position to defend the value. Also, if they had access to unpublished industry based studies they would be in an even more advantageous position. From the Minutes (1962-1984) it is possible to identify certain individuals, who were industry based consultants and who wrote and co-ordinated the development of many TLVs. Table 8.4 is a table produced by Castleman and Ziem. It shows that two people in particular were in a position to be highly influential: Torkelson a toxicologist at Dow, and Morgan, a hygienist at Dupont. Between them they prepared the

documentation and, in many cases, proposed a TLV value for 55 substances including, as Castleman and Ziem put it, ".....major products of their own companies and new products about which little or nothing had been published." The Dow products included, in Torkelson's case, Tordon, Ruelene, Dursban and Plictran. The Dupont products included, in Morgan's case Lannate, Hyvark and Karmex.

Table 8.4: Substances assigned to industry based consultants (taken from CASTLEMAN and ZEIM (1988) table II

SUBSTANCE	PERSON
2,4,5-T	Rowe
ethylene glycol	Torkelson
vinyl chloride	Torkelson
methyl bromide	"
propylene glycol methyl ether ("DowanolPM")	"
methyl chloride	Torkelson
1,2 dibromoethane (ethylene dibromide)	"
1,2 dichloroethane	"
o-chlorostyrene	"
methylene chloride	"
1,2,4 trichlorobenzene	"
vinylidine chloride	"
dicyclopentadiene	"
clopidol ("Coyden")	"
tricyclohexyltin hydroxide ("Plictran")	"
chlorpyrifos ("Dursban")	"
picloram ("Tordon")	"
dimetholate	"
3,5 dinitro-o-toluamide ("Zoalene")	
dimethyl sulfate	Morgan
tris(2,3-dibromopropyl phosphate)	Morgan and Torkelson
styrene	Torkelson
bis-chloroethyl ether	"
1,2,3 trichlorobenzene	"
chloroform	"
dipropylene glycol methyl ether ("Dowanol DPM")	"
ethanolamine	"
2-chloro-6-trichloromethyl pyridine ("N-Serve")	"
crufomate ("Ruelene")	"
chlorodifluoromethane	Morgan
chromates	"
methomyl ("Lannate")	"
perfluoroalkanos	"
cyclopentane	"
m-xylene, α,α - diamine	"
bromacil ("Hyvar X")	"
diuron ("Karmex")	"
dioxane	Torkelson
calcium hydroxide	"
cyclopentadiene	"
dibromochloropropane	"
cyanamide	Morgan
azordrin	Zavon

dicrotophos ("Bidrin")	"
m-phthalodinitrile	"
isophthalonitrile	"
dioxin	Torkelson
phosgene	Morgan
m-toluene diamine	"
hexamethyl phosphoramide	"
formamide	Morgan
dimethyl sulfoxide	"
dichloromonofluoromethane	"
4,4-methylene bis (2-chloroaniline) ("MOCA")	"
tetramethyl thiourea	Zavon
hexachlorobutadiene	Torkelson
3-amino, 1,2,4 triazole ("Amitrol")	"
deodorized kerosene	"
toluene	"
acrylonitrile	"
1,3 dichloropropene	"

Simply because a person employed by a company is involved in assessing the toxicity of that company's products does not mean that that assessment will be distorted. However, as has already been discussed in sub-section (vi) judgement has an important role to play where decisions have to be made in areas of scientific uncertainty: and choosing a TLV figure certainly comes under this heading. Also, a professional, as was explored earlier must be mindful of his or her clients' needs and may be inclined especially where large costs are involved, to shade his or her judgement in a certain way, giving the benefit of the doubt to the product in assessing ambiguous data. Indeed, the fear that decisions may consciously or unconsciously become partisan was the basis for the initial separate formation of the ACGIH and AIHA. It is also implicit in the formation of the TLV Committee. Only neutral people not employed in private companies can be full members with voting rights. Be that as it may, it is quite clear that individuals such as Torkelson and Morgan have had a very significant effect on the Committee's TLV decisions. The fact that the Committee allowed this is understandable. Rather than corresponding with individuals in various companies why not invite active people from the largest companies to participate, especially from the two companies with the most substantial industrial hygiene and toxicological traditions? Such individuals would be regarded as an asset and would certainly have privileged access to in-house company reports and data, and not simply from within their own company. Thus, Scheiderman (Metropolitan Water District of Southern California) had a lot of trouble getting proprietary information on economic poisons (Item 155a, Table 8.2) whereas Kimmerle (Bayer Company) and Torkelson (Dow Company) would seem to have had little problem producing a list for pesticides and industrial chemicals, (Item 162, Table 8.2). Such consultants have had an indirect and unquantifiable influence on the Committee and have acted as conduits for in-company reports and assessments.

Occasionally, the Minutes show, they have had a very direct influence. Torkelson in 1976 chaired the sub-committee on Hydrocarbons and Chlorinated hydrocarbons. In his covering letter he says: "I have asked the American Petroleum Institute to have representatives present who can discuss the results and conclusions of the extensive research they have sponsored at the Carnegie - Mellon Institute .... Mr Disbennett will be sending you a set of reprints. **It is important that we be familiar with these since they will be the basis for any changes we make in TLV's for these hydrocarbons."** (Author's emphasis.) (Item 96, Table App. 4.2). The sub-committee considered the TLV's for 144 hydrocarbons and chlorinated hydrocarbons at its October meeting. Torkelson chaired and directed the Committee and the major part of the evidence to be considered came from Dow and API sponsored research. How much this influenced the decisions of the sub-committee is impossible to say. They covered a large number of chemicals in a short period of time (2 days) and there would have been very little time for debate. A forceful assessment by a leading toxicologist such as Carpenter, one of the API representatives, might well swing an assessment a certain way in such circumstances. Whether this matters or not depends upon how one regards applied scientific knowledge. If you believe it is untouched by the conditions under which it is produced then the factor described would not be regarded as important: A scientific fact is a scientific fact. However, the Committee certainly did not take this view else why did it exclude industry representatives from the main committee body and not allow them to vote?

In addition, it is clear from the earlier discussion of applied sciences and profession's that where there is a spectrum of opinion, the point on the spectrum that a professional applied scientist adopts will, in most cases, be related to the interest of the professions most important client group. In the case of a company representative, the main client is the company he or she works for.

The Committee faced still faces a quandary. They want access to the information and resources industry had to offer: "They (the Consultants) represent the large companies ..... Dupont, Dow, Monsanto and Hooker chemicals. In terms of the variety of chemicals that they manufacture and the research facilities they have to test these materials, toxicologically there is a wealth of information from these companies that can contribute to the deliberations of the Committee". WAGNER (1984).

The Committee, at the same time, understood the position of company hygienists and toxicologists, therefore, they debarred them from voting. But this ignores the fundamental reason why such people were debarred in the first p'ace - because such people tend to take up positions which serve their clients best where there is a legitimate range of opinions. Such people are in an especially powerful position when it is their laboratories or personnel who have produced the evidence over which the debate ranges and there is no independent research with which to make comparisons.

As the last sub-section showed, individuals on the Committee, became, or were brought onto the Committee because they were experts on certain substances or certain fields. It is quite clear from the Minutes that certain people dominated certain decisions. It seems highly likely that people such as Torkelsan and Morgan exercised similar power given the factors already discussed and they way the Committee delegated power to individuals.

The influence of consultants, particularly those employed by industry did not go unnoticed. Elkins, a long standing member of the Committee and one of the "grandfathers" of IH on the USA, in his letter of resignation to Stokinger in 1975, made the following observations: "

"In looking over the new documentation I was taken aback by that for ethylene glycol; the limit of 100ppm was found intolerable by sedentary volunteers in a few minutes (or seconds). I believe that {the industry representative} recommended this figure. In spite of his knowledge he seems to come up with some recommendations for TLV's that are way too high, in my judgement. The same can be said for most of the other industry representatives we have had. **In many cases they recommend a TLV much above the action levels used in their own plants.**" Quoted in CASTLEMAN and ZIEM (1988).

Similarly in 1980 when Dr Hector Bleijer resigned from the Committee after 10 years as a member, he complained of what he described as "... an increasingly stronger pro-industry bias, particularly among almost all the Committee consultants and among the members who consult privately for private industry." He went on to blame this pro-industry bias and repeated "unnecessary" disagreements with NIOSH and OSHA for having made the TLV committee and ACGIH appear "anti-NIOSH, anti-OSHA, and anti-labor". Quoted in CASTLEMAN AND ZIEM (1988).

It is worth noting that Dr Blejer resigned when consultants made up some 29% of the Committee. The percentage was to rise still further in the next few years and with it, by implication the influence of industry based consultants.

Having tried to gauge the influences of industrial consultants on the Committee's decision making process, it is worth considering why the unions did not get more involved. From 1971 to 1975 David Padden of the United Autoworkers Union (UAW) representing the AFL-CIO (the US national confederation of unions) sat on the Committee as the Labor Liaison member. When he retired no replacement was found although the Committee did make some effort to do so. Why was no suitable person found? Partly this would be due to the fact that few unions employed suitably qualified people and those that were employed were run off their feet. But there is another reason which meant that unions did not make a great effort to get representation on the Committee.

William Wagner discussed it during his interviews with the author. He believed that union representatives were in a difficult position on the Committee because, "... TLVs are probably going to be higher .... than the Union would subscribe to, because I'm sure they would like to see a zero as the number, or some number that is very low and probably is lower than the TLV Committee would consider. And that could well place the union representative in an awkward position amongst his members". WAGNER (1984).

Wagner's view is probably correct, the union representative probably did feel uneasy about some of the TLV figures which the Committee agreed to. Yet he had no right of veto, but simply by being part of the Committee he was implicitly lending the AFL-CIO's agreement to the values set. Wagner's view that the union position would ideally be zero level of exposure is a revealing comment and will be examined again.

Before all the strands of the TLV Committee's process are pulled together into a set of conclusions, the next sub-section will examine three examples of industry's direct influence on the setting of a TLV.

(v) Examples of influence

(i) Dimethyl sulphate (DMS)

In 1970 the TLV for the material was 1ppm. In the late 1960's animal test in Germany indicated that DMS was carcinogenic and the DFG lowered its MAK from 1ppm to 0.01ppm in 1971. The first mention of the animal tests is recorded in the Minutes in April 1972. This entry and the others relating to DMS are summarised below:

April 1972.	Morgan - DMS is an experimental carcinogen. It is given an NIC of A2. Dupont study shows no effect on the workforce.
November 1972	Morgan - Dupont will supply environmental data.
May 1973	Morgan - Dupont will have DMS analytical method ready by June.
November 1973	Morgan - Dupont epidemiological study finished, it will be forwarded to Stokinger.
April 19874	Morgan - "Studies indicate expected incidence of non-DMS malignancies."
April 1974	Recommended changes listed at the end of the Minutes list DMS as 1973 1ppm 1974 = 0.01ppm, the same as the DFG MAK.

November 1974	Stokinger - "German analytical data not reproducible. No value assigned for A2."
May 1975	Morgan - Suggests a change from the A2 no TLV value position, to a ceiling value of 1ppm, (+ A2 notation ?).
April 1976	Morgan - No change from 1975 value – should be 1ppm (A2).
November 1977	Carcinogenic substances sub-committee - Subcommittee recommends a value of 0.1ppm. "This will provide a wider margin below Druckrey's lowest activity concentration of 3ppm and will avoid irritation. Morgan to prepare documentation.

In 1977 DMS is listed in the TLV booklet with a new value of 0.1ppm TWA and a STEL of 0.5 ppm (A2).

The Minutes show J. Morgan was responsible for DMS over the period 1972-76 during which time the new TLV was developed. He was a hygienist, and one of the first industrial liaison people and was employed by Dupont, the sole manufacturer of DMS in the USA.

The development of the DMS TLV was slow and the influence of Morgan is clear. Indeed at one point, in May 1975 he suggested the same numerical figure for the DMS TLV as it had before it was identified as an animal carcinogen. It was only the intervention of the Carcinogenic Substances Sub-committee which reduced it to 0.1ppm. The Dupont in-house epidemiology was also clearly influential. Intially, in 1972, DMS was listed in the NIC as a possible human carcinogen, CASTLEMAN AND ZIEM (1988). But the Dupont evidence changed Stokinger's mind. In 1973 the NIC notation was changed to A2 (experimental animal carcinogen) and Stokinger wrote to OSHA objecting to their Emergency Temporary Standard (ETS) for DMS, quoting the unpublished Dupont survey. The Dupont epidemiology is central to the view of the Committee and is almost certainly the reason why the eventual TLV set was X10 higher than the German MAK. Castleman and Ziem obtained a copy of the original Dupont survey, it consisted of a study of lung cancer amongst 143 workers. Two deaths from cancer had occurred, which Dupont argued was normal for such a population. However, the population was neither divided up in terms of time since first exposure or duration of exposure to DMS. And there was no follow-up of people who were exposed but who had left or retired. All these omissions, together with the small size of the population studied, would drastically reduce the accuracy and sensitivity of the Dupont epidemiological study.

It is fair to say that Dupont had a large influence on the setting of the DMS TLV.

(ii) 1, 3-Butadiene, (1,3-BD)

Up to 1982, 1,3-BD was regarded as a relatively low toxicity substance, causing narcosis and irritation at high concentrations. It had a TLV value of 1,000 ppm. Long-term animal studies, sponsored by the International Institute of Synthetic Rubber Products (IISRP), were completed in 1982 following earlier published evidence that 1,3 BD was mutagenic. The IISRP study indicated that 1,3-BD was carcinogenic. The industry reduced its internal OEL from 1,000 to 100 ppm in 1982 WAGNER (1984). In 1984 other studies in the National Toxicology Programme (NTP) had confirmed that 1,3-BD was an animal carcinogen and additional work show it to be teratogenic in rats. Wagner, who prepared the revised documentation for 1,3-BD recommended an A2 notation (experimental animal carcinogen) and a TLV of 10ppm. Both his recommendations were accepted, and in 1986 the new value joined the main TLV list. When asked whether the limit of 10ppm was achievable Wagner replied:

"Oh yes .... that is one of the reasons we chose 10. The industry is around 3-4ppm, so there is no problem in achieving it, in fact they could probably get lower if they had to." WAGNER (1984).

In 1984, the unions representing the rubber workers, the main group exposed to 1,3-BD, wanted OSHA to set a limit of 0.1ppm and NIOSH issued a Current Intelligence Bulletin suggesting that levels be reduced as low as feasible.

In the case of 1,3-BD there was less direct industrial influence on the TLV Committee than with DMS. None of the industry liaison people directly represented the manufacturer or users of the substance. However, Wagner and the Committee knew what industry was doing via the IISRP and knew what current exposure levels were. By choosing 10ppm the Committee was setting an easily achievable limit which, according to Wagner, was already being complied with by the industry.

(iii) Rhodium

The rhodium TLV was first set in 1967 at 0.001mg/m^3. It was based on the irritant effects of rhodium salt mist reported by Ratney, one of the Committee members. As platinum compounds were known to have anti-tumour properties it was though that rhodium might have similar properties. Some antitumour effects were found but feeding studies in rats, published in 1971, also showed that rhodium compounds were carcinogenic in mice. Later work showed that rhodium compounds were mutagenic. In 1980 the Committee put rhodium metal fume and dust TLV on the NIC list suggesting the level be raised from 0.1mg/m^3 to 1.0mg/m^3.

Apparently when Johnson and Mathey (UK), the main production company for rhodium heard that the TLV might charge they sent representatives to address the Committee on 2 or 3 occasions. "They had a tremendous amount of data that they made available to the TLV Committee. They actually wrote a documentation of the recommended TLV which was certainly put in for consideration, and their account was certainly persuasive. I would say that our TLV is the result of their input and their recommendations." WAGNER (1984).

In 1982 the rhodium metal TLV was raised from 0.1 to 1./0mg/m^3 and "insoluble compounds" rose to a similar value in 1984. Also in 1984 the TLV for soluble rhodium compounds rose from 0.001 to 0.01 mg/m^3. From the rewritten documentation it is difficult to work out why the TLV's for rhodium and its compounds were raised. The only evidence offered is that rhodium does not cause sensitization like soluble platinum salts. The evidence of carcinogencity and mutagenicity seems to have been completely ignored.

As with 1,3-butadiene there was no one on the Committee representing the rhodium producing or using companies. However, the main company with an interest made very effective representatives to the Committee.

The initial soluble compounds TLV of 0.001mg/m^3 set in 1967 was certainly based on limited evidence. But, given the new evidence of carcinogenicity and mutagenicity one would have thought the TLVs should either have remained the same or been reduced. What seems to have happened is that by concentrating on the sensitizing effects of platinum and the lack of a similar effect from rhodium and its salts, Wagner and the Committee became convinced that rhodium had been too severely judged in the past. In the process of focussing on sensitization the other potential latent effects became downgraded and, in effect, ignored. Whatever the explanation it would certainly appear that Johnson and Mathey, the major producer of rhodium and its salts, determined the TLVs for this element and its compounds.

(vi) The TLV Committee and industry

This sub-section has tried to develop some feel for how influential industry has been on the setting of TLVs and how that influence has occurred. A quantitative and qualitative picture has been sketched and the view developed that industry has had a very signifincant influence on the development of a large proportion of TLVs and has been determining in certain cases. How has this influence been exerted ? Various ways have become clear in this analysis and itemising them clarifies the picture. Industry has influenced the TLV Committee in the following ways:

(i) by supplying data or "industrial experience". This information came in three forms:

(a) Assessment of the effects of exposure usually based on medical surveillance of the current workplace.

(b) Measurement data on current exposure levels.

(c) Some analysis of the feasibility of attaining a certain exposure level and possibly the cost of reaching a proposed TLV.

(ii) By supplying interpretations and overall judgements on the data supplied and other research work. This was done via industry experts who addressed the Committee or sometimes by means of specially prepared briefing documents, for instance, Torkelson's Hydrocarbon sub-committee briefing or the rhodium documentation prepared by Johnson and Matthey.

(iii) In a way related to (ii) but distinct from it ,industry influenced the Committee via its "Consultants". Such people were at times in charge of the development of TLV's for substances of which their companies were major producers. While such people often supplied unique in-house data, the quality of the data was suspect on occasions and the individual tended towards a lenient interpretation of the effect of their companies products in their charge. As Elkins put it, ".... he seems to come up with recommendations for TLVs that are way too high, in my judgement. The same can be said of most of the other industry representatives we have had. In many cases they recommend a TLV much above the action levels used in their own plants." ELKINS (1975) quoted in CASTLEMAN and ZIEM (1988).

To understand how a Committee which started life with the more or less explicit project of "protecting the workers' interests", could allow itself to be so influenced one has to consider the internalised beliefs of the Committee and its nature and position in US society. This is a good point to pull together the threads of the analysis of the ACGIH TLV Committee.

CHAPTER NINE

THE ACGIH TLV COMMITTEE — DISCUSSION AND SOME CONCLUSIONS

"Originally, preventing impairment of health was virtually the only consideration in the selection of the proper limiting air concentrations... Nowadays however, ... more subtle effects on health ... are also considered."

"The Committee is keenly aware of the gravity of its acts... and its final decisions must be ... unfettered by considerations other than the health and well-being of the industrial worker."

"It should be noted also that these limits provide safety without overprotecting the worker or over engineering plant processes..." STOKINGER (1955, 1964 and 1968a).

"The Committee idealistically functions as occupational health professional by recommending TLV's to protect the health of workers **without regard** to economic or technical feasibility." LEE (1987) (Emphasis in the original).

9.1 Introduction

This thesis started by considering the absolute and the more qualified statements made on behalf of TLVs and the process by which they were set. The ambiguity created by the various contradictory statements was highlighted and the evident tensions in the standard setters minds were identified as a creative avenue to explore.

Also, at the start of this thesis in Part I the theoretical framework to be used to analyse the development of OELs was laid out. Put in a nutshell, the conclusions were that theories and paradigms have proponents and no change occurs unless a particular group within the scientific community develop and put forward alternative theories or an alternative paradigm.

In Part 2 the groups were identified which were of particular importance to the development of OELs – industrial hygienists and toxicologists. From the theoretical framework detailed earlier it was clear that such groups of applied scientists were at one and the same time professional scientists and scientific professionals and could operate in two parallel and sometimes conflicting modes. The OEL paradigm was forged and promulgated by these two professions and the analysis in Part 3 examined some of the contradictory currents within the paradigm from the applied scientific/professional dichotomy. It became clear that the question of practicability, costs and social issues were embedded in the paradigm and had been present ever since it crystalised. The inclusion of these issues had a certain inevitability about them given the applied nature of IH and IT and the needs of the profession's clients. What was not clear at the end of Part 3 was the answer to the question; How has the paradigm been articulated in practice? Drawing on the analysis in the last three chapters it is now possible to answer this question. The quotations at the start of this chapter are a reminder that there has been and still is a large body of expert opinion which would deny any involvement of practical, social or economic issues in the setting of TLVs.

9.1.1 Chapter guide

This chapter considers various aspects of the workings of the TLV committee and the OEL paradigm. It started with the Committee and then goes on by means of a question and answer format to examine the nature of TLVs. The OEL paradigm and IH are then considered and this is followed by an examination of reasonably practicable OELs including TLVs. Other commentaries on TLVs are reviewed and discussed and some further comments on TLVs are made in the light of these comments. Finally the OEL paradigm is reconsidered and the Working Definition is re-drafted.

9.2 The ACGIH TLV Committee

9.2.1 The nature of the Committee

The Committee in its early days was small, consisting of five people initially. It expanded but has never been large. The turnover in key individuals has been low particularly, until recently, amongst Chairmen. The low turnover made for continuity and the small size and voluntary nature of the commitment produced a close knit group committed to the project of the Committee. It is evident from the Minutes that both on matters of policy and individual TLVs the Committee delegated to individuals. Resources were always limited. Even nowadays although the Conference provides some administrative support and travel and accommodation are paid, the effort and time of individual Committee members is still voluntary and unpaid.

Given its nature the Committee has made and still makes every effort to secure the co-operation of individuals and companies. The Minutes are peppered with references to contact "X" working for company "Y" to get information on "Z". The Committee has always sought to co-operate with and persuade the industries with which it has dealt. It has carried on the traditional mode of behaviour of its progenitor the PHS. The ACGIH and the TLV Committee is a 1940's organisation. It's paradigm is from this era and so also is its political philosophy. While at a formal level it keeps company professionals at arms length, it believes that co-operation between government and industry is natural and for the best. It is wedded to persuasion and pragmatic solutions.

9.2.2 Position of the TLV Committee

From its inception the ACGIH has been concerned to maintain and increase the status and influence of the IH professions in the US Federal and State governments. The very earliest meetings discussed professional qualifications and the need for uniform and professional state rules and enforcement. The analytical framework developed at the start of this thesis examined the behaviour of applied scientists as both scientists and professionals. One of the major differences between these two viewpoints is that professionals have clients and scientists, in theory at least, do not. The

needs of a professions clients shape that profession. In the case of the ACGIH TLV Committee the question becomes, who were the Committee's clients? And the answer is clear – the PHS and local state public health units, and on a wider perspective the answer is – Federal and Local government. The TLV Committee has functioned since the mid 1940's as a surrogate Federal OEL standard setting body.

Indeed, right from the start the user of MAC's/TLV's could be forgiven for thinking they were officially blessed by the Federal government. Up until 1950 apart from the Conference Transactions the only other place the list was published was in the PHS's IH Bulletin. This was the unofficial/official sanction that Henry Smyth objected to so strongly.

The Committee was never directly financed by its clients but it was subsidised – PHS buildings and secretarial facilities were used and individuals were released to attend meetings. it is also probable that TLV Committee business was officially built into individual work plans. Certainly Stokinger's work and research priorities fit into this category.

As a Governmental OEL standard setter the TLV Committee members had to consider the countrywide implications of their decisions. The next sub-section makes clear that the Committee was faithful to the OEL paradigm and the Minutes and Wagner's evidence all indicate that TLV's were set to protect health **and** to be practical or feasible. Because the Committee was, in effect, working at the Federal level the practicability of a TLV had to be considered across all industries handling or using a particular substance.

9.2.2.1 Dependency and the limits of difference

The TLV Committee has always been small, minimally resourced and voluntary and yet it set itself tasks to complete on a national scale. It could call upon the resources of the PHS and State IH units but these were always stretched. It therefore did what the PHS did before it and approached industry for information and embarked on what were, in some cases, collaborative ventures. While industry clearly lobbied for a particular TLV in many instances, especially once an imminent TLV change was announced in NIC, it rarely volunteered information and suggested a TLV be set. It was usually the Committee that chased industry for information. This and the co-operative policy of the Conference often put the Committee into a dependent position. This does not mean that industry controlled the Committee but such a relationship necessarily set limits to how different the Committee's and industry's interpretations could be. It would be difficult for the Committee to regularly stray too far from industry's assessment of where a TLV should be set. This applies particularly to those few large chemical companies such as Dow, Dupont and Hooker which supplied the bulk of the "industrial data".

Dependency does set limits on difference and the examples at the end of the last chapter, together with the earlier analysis of the Minutes show that in a significant number of cases industry determined the TLV set.

9.2.3 The internalised beliefs of the Committee

The OEL paradigm was developed and expressed by the IH and IT professions in the USA. By the 1940's it was firmly established and the ACGIH "Threshold Limits Committee" was the organisation set the task of putting the paradigm into practice. From the analysis of the pronouncements of the early Chairmen of the Committee and the development of the TLV definition it is clear that the Committee believed implicitly in the OEL paradigms (see Figure 9.1). It was the almost unconscious metaphor that informed their practice. It is also clear from the analysis in Part 3 that individuals have differed in their interpretation of the paradigm and have changed their own interpretations at different times. Paradigms are not monolithic and determining. The Minutes also reveal a heterogeniety of paradigm interpretation and sometimes, as in the vote on whether thresholds exist for carcinogens individuals stretch their belief in the paradigm to the limit. Even so, in its practice of setting TLV's for carcinogens the Committee has demonstrated and continues to demonstrate its continuing faith in the OEL paradigm. Other bodies, such as the DFG in West Germany have rejected Tenet 3 (see figure 9.1) and do not attempt to set health based OEL's for carcinogens.

One thing clear from the Minutes is that the Committee have never had a problem reconciling Tenets 1 and 2.] in the early days of the Committee identified it as a challenge which he dealt with by emphasising the "necessity of preserving a balance" and the line has continued to the present day: "There is an element of practicality associated with the TLV's and that has persisted and I hope it always will...' WAGNER (1984).

The surrogate Federal OEL standard setting role ACGIH set itself reinforced this view. The paradigm promised that TLVs could protect health and be feasible. The pseudo-Federal position of the Committee told it that TLVs must be feasible.

Having stated that the Committee followed the OEL paradigm and believed, (a) that toxic substances could be used safely by using IH control techniques; and (b) that threshold doses exist for all toxic substances, (Tenets 1-3), there remains the question of how they followed Tenets 4 and 5. They did so by a combination of factors.

Firstly, there was the remit of the Committee. Put simply, the purpose of an OEL setting committee is to set OELs. That is what people expect and that is what such committees set out to do.

Figure 9.1 The OEL paradigm Working Definition

1. Toxic substances can be used safely.
2. IH control methods are adequate for all toxic substances.
3. Threshold doses exist for all toxic substances.
4. Standard setters can identify threshold exposures (doses).
5. The major consideration in the setting of OEL's is the health effects of toxic substances.

The various sources of pressure to "name a number" have already been described. And the surrogate "Federal" status of the Committee was certainly important. Once the TLVs became well known, TLV users expected the Committee to produce TLVs and the Committee has adopted a very pragmatic approach:

"My philosophy is that some number is better than none, and so OK you make a mistake when you first put it out, that's why it goes on the intended changes for two years" WAGNER (1984).

This pragmatism is also to be seen in the TLV numbers chosen. As was described earlier, numbers such as 5.0, 2.5 and 1.0 and the simple multiples and fractions where analogy is used owe more to arithmetic neatness than any fundamental toxicological knowledge.

Pragmatism has clearly been an important factor when one considers Henschler's evaluation of the data available to the TLV Committee:

"A review of existing documents which were produced to explain or justify the setting of occupational exposure standards reveals that those who performed this work in the past argued rather generously, to say the least.... Only too often, the documentation closes with phrases like 'in the Committee's view a threshold limit value of such and such is regarded to protect sufficiently from harmful effects'.

He goes on to say:

"A careful review of some 150 occupational chemicals in the German list of MAK values (based largely on TLVs) revealed less than 10% (are) based on sufficient animal tests and/or field experience." HENSCHLER (1984).

The second fact which enabled the Committee to reconcile Tenets 4 and 5 was the belief in thresholds of effect. This clearly comes into its own when coupled with the third important factor, the role and status of "experience". The central importance of this word, especially when coupled to another word, "industrial", has already been examined in Chapter 7 (Sub-Section 7.4.5) and was repeatedly emphasised by Stokinger in his writings. The reality is that "industrial experience" has only limited power to see the connection between exposure and health effect, especially if the effect is chronic or latent common in the general population. A very significant proportion ($\geq 40\%$) are based on the short-term irritant properties of the material. If Henschler is correct, for most TLV

decisions the "industrial experience" was minimal or of low quality, and the analysis of the Minutes reinforces this view. Yet "industrial experience" is one of the touch stones of IH:

Stokinger, Hatch and Smyth appeal to the evidence of "industrial experience" as proof that TLV's are well founded.

The words have been used in a similar way in the TLV preface since 1958.

They regularly crop up in the Transactions of the Conference. And they occur in the Minutes in the form "... industrial experience shows no effect at exposures of "X" ppm."

Wagner gives a strong feeling of the importance he attaches to "experience":

"....we have (other) industrial hygienists, McDermott was one who had very wide experience in the field in the State of Michigan which is heavily industrialised. The spectrum of industry that he has experienced is huge, that kind of person is invaluable...."

"When you have industrial hygienists for the State of Massachusetts or Vermont... you are talking about a multiplicity of industries, every industry you can think of is covered by these people, either out in the plants taking samples, looking at the workers ... or in the labs. .... The kind of stories these guys used to sit and tell us about. Old time industrial hygienists – when I first came on board they talked about these industries and the fact that they would be called in and there was mercury running all over the place... it was hands on experience." WAGNER (1984).

And the Committee also valued the experience of its industrial consultants:

"He's (J. Hammond) Chairman of that sub-committee, Miscellaneous Hydrocarbons. He's a professor at the University of Texas, but he was formerly with the Exxon Corporation, so he knows the petroleum industry and benzene, toluene and xylene... and George Rausch is also a member of this sub-committee and is very knowledgeable of the petroleum industry, he was formerly with the Ethyl Corporation as medical director. So these fellows know what they're doing." WAGNER (1984).

And Stokinger in a review of the development of the TLV Committee speaks in similar eulogistic terms:

"So, the founding fathers, like the founding fathers of the 13 colonies, planned well, for their original plans of action are those still followed. But the TLV Committee has added one essential ingredient more, the experienced judgment from long years of on-the-job observations in the fields of industrial hygiene and toxicology." STOKINGER (1981).

The word "experience" and the words "industrial experience" were imbued with an uncanny quality, they were invested with far greater power to detect the occurrence of harm than was or is the case. And the quotation from Wagner reinforced points made in Chapter 8 concerning the importance of industrial liaison consultants. It is evident that such people's experience could wield much power in the Committee. They had far more "hands-on" experience and knowledge of the processes in their industry than any other members of the Committee could ever hope to gain.

The pragmatism and mission of the Committee, coupled with a strong belief in the paradigm, particularly the belief in the existence of thresholds and the implicit promise that all toxic substances could be handled safely by use of IH control methods, and a unified belief in the power of "experience", has enabled the Committee to set TLVs on the basis of very little evidence. The Committee also knew that if the TLVs were enforced then conditions in some parts of industry would be improved greatly over the conditions reported by the "old time industrial hygienists". By combining a firm belief in thresholds with an almost reified vision of "experience" the Committee could believe it had identified the threshold of effect. "Industrial experience' told them a certain level of exposure caused no effect, so, ipso facto, that exposure level must be below the level of effect, the "threshold level". Any doubts individual Committee members had were covered by the phrase "nearly all" in the TLV definition, which for a significant proportion of the Committee meant all people apart from 'hypersusceptibles'.

By describing the Committee's internalised beliefs in such stark and condensed terms it is quite possible to believe that the Committee members were either naive or malign or both. This not the case but I do believe that the analysis and explanation of the Committee's behaviour offered by the description of the Committee's internalised beliefs is coherent and fruitful. It offers the best explanation of the Committee's behaviour and is typical of human belief and thought. It is quite possible for a human being or group to function with a belief system which appears coherent from inside, yet, when viewed from outside appears to be incoherent and flawed. Such belief systems are not created and maintained in a vacuum. They are continually reinforced and maintained by their own internal dynamic and by external circumstances.

So far, in this sub-section, the nature, position and internalised beliefs of the TLV Committee have been considered in isolation. In reality they have always interacted and reinforced the practice of the Committee. The Committee was formed by a small, young profession based on people employed by the PHS and local State IH units. Its clients were the Federal and individual State and it kept faith with the PHS's co-operative and cajoling approach to industry. It inherited a paradigm which was forged during the development and consolidation of the new profession of IH. The nature, position and beliefs of the TLV Committee all stemmed from the same roots and they fed each other.

What is clear is that the Committee has remained remarkably faithful to the OEL paradigm which crystallised in the late 1930's/early 1940's. Put another way – for the TLV Committee the paradigm has undergone no revolutions – no fundamental change. Though the preface introducing the TLVs is more qualified and the world in which the Committee operates is more complex, the Committee remains a firm believer and advocate of the paradigm. Perhaps the clearest piece of evidence for this assertion is the Committee's insistence on practical thresholds for carcinogens. This continuity of behaviour is perfectly consistent with a Kuhnian notion of the behaviour of a scientific group. Apparently contradictory evidence and theory is explained and incorporated in such a way as to

remain true to the paradigm. Paradigms are not given up lightly by scientific communities and rightly so – it is the very rigidity and tenacity of the scientific group to maintain a certain paradigm which enables the scientists to identify important and consistent anomalies which may ultimately lead to a paradigm shift. However, in the case of an applied scientific profession like IH and IT there are not simply the internal arguments within the community to contend with. There is also the pressure on the profession of the clients needs. These needs, partially or wholly satisfied by OELs such as TLVs, act as an additional brake on paradigm change. Thus, although other committees interpret the paradigm differently and some are now stretching it to the limits (to the point perhaps where an alternative paradigm needs to be articulated based on alternative exemplars and standard setting processes), such an alternative will require more than a shift of perception within the applied scientific communities of IH and IT. It will require a change in the standard setting behaviour of Committees in many countries and a change in the perception of government and other client groups. The issues this raises are discussed further in the concluding part of this thesis.

9.3 Threshold Limit Values

9.3.1 How important are TLVs?

TLVs became the dominant OEL's in the Western world in the 1950's and 1960's. Section 7.3 and the associated Appendix 5 shows just how influential TLVs have been. Most countries started with the TLV list and either continue to use it or have developed their own OEL setting structure. Some countries such as Sweden, West Germany and Holland have developed a measure of independence but others, the majority, in fact, still tend to follow the ACGIHs lead.

The TLV Committee acted as a surrogate Federal OEL setting body for over two decades. Once the OSH Act was passed by Congress and OSHA and NIOSH were officially established one might have though that the Committee would have disbanded. As is described in Chapter 7 for a time it looked like this would happen but then it became clear that OSHA's rule making process was a very slow business and the TLV Committee got its second wind. Even so, officially, OSHA IH's enforce OSHA PEL's and this policy would, one might think undermine the status of TLVs. This would appear not to be the case. The OSHA "IH Field Operations Manual" is quite specific about the importance of ACGIH TLVs:

"For a substance having an ACGIH Threshold Limit Value (TLV) ... but no OSHA PEL, a citation for exposure in excess of the recommended value shall be considered under Section 5(a) (1) of the act." US DEPARTMENT OF LABOUR (1980).

Section 5a(1) of the OSH Act states that:

"Each employer:

1. Shall furnish each of his employees employment and a place of employment which are free from recognised hazards that are causing or are likely to cause death or serious physical harm to his employees". OSH Act (1970).

If there is an OSHA PEL and a TLV, the IH is still encouraged to use the TLV:

"If an employee is exposed to concentrations of a substance below the PEL but in excess of a recommended value (eg. ACGIH TLV or NIOSH recommended value), a citation for inhalation cannot be issued. However, the Industrial Hygienist shall advise the employer that a reduction of the PEL has been recommended. NOTE: In these situations a 5(a) (1) citation of inadequate work practice may still be considered." US DEPARTMENT OF LABOR (1980).

How many OSHA IHs follow these recommendations is another question, but the point is that they are encouraged to do so and have the option enforce ACGIH TLVs where they are lower than the PEL.

Apart from OSHA's policy private industry has a policy of following and adhering to the TLVs.

Mendelhoff cites a statement from an IH working for Dow:

"... most companies in the chemical industry, at least the large and medium sized ones, have occupational health programs staffed by professional IH's who establish control programs often targeted at or below the ACGIH recommendations." MENDELOFF (1988).

From the evidence and arguments developed in the last chapter this would not necessarily be that difficult a policy for industry to enforce particularly for those TLVs which Dow has had a major say in shaping.

Since OSHA took over the 1968 TLV list of 414 substances and transformed them into legally enforceable PEL's ACGIH has lowered 100 of the original TLVs and set another 200+. In that same time OSHA has reduced the values of 10 of the original PEL's (TLVs) by means of a lengthy rule-making procedure. In the USA there has been much debate about how to speed up the rule-making and the risk evaluation process which the courts now insist on. One proposal which has been widely canvassed has been put forward by Mendeloff, a widely reputed policy analyst at the University of California. He puts forward a number of proposals but the main one of relevance in this context is that he suggests that OSHA do again what it did in 1970 ie. it converts the current TLV list into OSHA PELs with the exception of the 10 PELs already reduced. This conversion process is apparently in motion and is known within OSHA as the "PEL Project", PENDERGRASS (1988). Some of the arguments he uses to justify his proposals are mentioned later in this chapter. What is relevant here is that such a proposal should again be seriously made close on 20 years after TLVs first became the official Federal OELs.

The evidence is unequivocal: TLVs still have a very high status both internationally and in the USA.

9.3.2 The image of the TLV's

The way TLVs have been described by the various chairmen of the Committee (especially the early ones), in ACGIH publications and by committee members has already been examined in Chapter 7. But these definitions should not be viewed in isolation from the way TLVs are and have been presented. The definition and description of the TLVs Committee's process and the symbolic presentational effects interact and reinforce each other to project a certain image of TLVs.

The Threshold Limits Committee published its first list of OELs in 1946 which it called Maximum Acceptable Concentrations (MACs). They were published as a bald list of numbers with no preface. The name and the single figure, tabular presentation led the users to regard them as health based limits giving absolute protection from harm. This effect was reinforced by the first definition published seven years later which offered complete freedom for any health effect at exposures at or below the TLV.

The description of how the TLVs were arrived at itself reinforced this view. The Committee considered all the evidence and industrial experience and, in addition, reviews the TLVs annually. In some descriptions, especially those of Stokinger, the routine use of safety factors is implied.

Although the definition was amended by the inclusion of the phrase "nearly all" in 1958, 12 years after the first list was published, and the current Preface is more qualified than the first, the image of TLVs remained and remains essentially intact. The definition, the descriptions of Committee process and symbolic effect of the way TLV's have been and are presented, project a certain image: TLVs are set in an exact manner, by professional people who have access to sufficient information of good quality to set OEL's. The Committee regularly revises the levels and is only concerned with scientific evidence on health effects. TLV's are health based, safe exposure levels. The "articles of faith" type paper examined at the start of this thesis continue to reinforce this view right up to the present day.

The reality of TLVs is different. The early history of OELs and the discussions of the importance of practicability in the setting of single number OELs in the Conference Transactions and later in the work of Smyth, Stokinger and Hatch all suggest that the practicability of control was taken into account in setting TLVs. The analysis of the Committee's process, the influence of industrial consultants and the ethos, nature and position of the ACGIH all reinforce this view.

The tabular, single number presentation drew strong objections right from the start and alternatives were regularly canvassed in the Committee, but never actioned. The quality and uneveness of information has regularly concerned Committee members. The use and application of "safety factors" has been uneven and patchy and does not appear to relate in a systematic way to categories

of health effects as Stokinger in particular has implied. Finally, the values are not reviewed annually as has been claimed by the Conference up to 1970.

9.3.2.1 TLV Review – no change, slow change, small change

In Chapter 7, figure 7.2 showed the number of TLV's revised each year from 1946-1970 based on ACGIH records. The proportion of TLVs revised was, an average of 2.6% and there was a clear, but irregular cycle of revisions, as if it was done as discrete projects. This cycle is what one would expect of a small, voluntary committee.

Keeler and co-workers examined the changes made to TLVs from 1946 to 1981. They concluded:

"Changes in the TLV's are infrequent. Less than one third of the substances ever listed changed TLVs from their initial listing." KEELER et al (1982).

They also noted that, "40% of the reductions were to half the initial standard."

In a more limited analysis of the reductions in TLV's since the OSH Act Mendelhoff found that: "Of its roughly one hundred changes, the median reduction was 50 percent. In only about a dozen cases was the reduction 90 percent or greater." MENDELOFF (1988).

The reality of the review process is that the large majority of TLVs never change once set. The rate of change is slow though it has been increasing in recent years (see Sub-Section 7.2) and it is cyclical. When a TLV reduction does occur it is normally to half or a quarter of the original value. The review process could be characterised as slow, sporadic and conservative. It has none of the active feel that the phrase, "these values are reviewed annually" implies.

The reality of TLVs and the process by which they are set differ from the image, the description and delivery of TLVs projects. A large chunk of this thesis has been a concentrated effort to disentangle the image and the reality.

Important questions can now be asked and fairly comprehensive answers given.

9.3.3. Question 1 – Where do TLVs come from?

They are the product of a select grouping within the applied scientific profession of IH- the ACGIH. This body has remained faithful to and defended the OEL paradigm. By remaining true and defending the paradigm and the TLV, the Committee has maintained a remarkable continuity of action. TLVs are the product of a small, voluntary, professional organisation whose major clients have been the US Federal and local state, and which has acted as a surrogate Federal OEL setting body.

9.3.3.1 Question 2 – Are TLVs health based OELs

From the foregoing analysis it is clear that observed or potential health effects are not the only factor which the Committee consider when deciding on a TLV. The feasibility of attaining and the possibility of complying with the TLV are also important. The question of how the balance is struck is left until the next sub-section.

Before health based limits are considered one important assumption must be made explicit. It is assumed that if only the health effects of a substance are considered and the standard setters make no consideration for how easy it is to control exposure down to the level chosen, then such a limit would be difficult to attain for many current production processes. New, redesigned processes might be able to meet such a health based OEL but for many current processes the task would be difficult and costly and in some cases impossible.

9.3.3.1.1 There is a variety of evidence that TLVs are not health based (by which is meant, set only on consideration of proven or potential health effects).

The preface of the TLV list has become more circumspect with time. Although, for the arguments given above the phrase "nearly all" and the qualifications are probably ignored by many, the current preface does specifically state that:

"... a small percentage of workers may experience discomfort ... a smaller percentage may be affected more seriously". And individuals who are, "hypersusceptible or otherwise unusually responsive ... may not be adequately protected... at or below the threshold limits" ACGIH (1988a).

These qualifications plus even a cursory examination of the Documentation of Threshold Limit Values show that exposure at or below the TLV value for a significant number of substances will result in irritation other short term effects and possibly long-term effects. Keeler and co-workers came to similar conclusions:

"... the Documentation for the standards gives the impression that some discomfort and hazards to health are part of work for many workers whose jobs are in compliance with ACGIH recommendations." KEELER et al (1982).

9.3.3.1.2 A comparison with the risks calculated by other authorities at the TLV value gives some indication of the proportion of people who may be serious affected. For example:

(a) Asbestos

The current (1988) TLV for chrysotile asbestos = 2 f/cc

A recent UK estimate of the risk at this level of exposure for 35 years starting at age 20 puts the number of deaths from lung cancer and mesotheliana at 80/1000 men exposed or 8%, DOLL and PETO (1985).

(b) Benzene

The current (1988) TLV for benzene = 10ppm.

OSHA relied upon a risk analysis by Crump and Allen which used epidemiological data from a number of studies. The combined studies risk associated with 45 years, or a working lifetime exposure to 10ppm was 95 leukaemia deaths per 1,000 or 9.5%, OSHA (1987).

9.3.3.1.3 Another way of getting some measure of how far TLV's are from being health based limits is to compare the ACGIH list with NIOSH Recommended Exposure Limits (RELs). The NIOSH RELs are supposed to be based solely on an assessment of the risk to health, though even here an element of practicality creeps into the process, MAZZUCKELLI (1984).

NIOSH has produced about 150 standards for chemical substances and safety problems. Many of the chemical substance standards do not have specific numerical values but exhort the occupier to control exposure as low as is feasibly possible. A full list of the NIOSH limits was published in 1986, CDC (1986). Out of this list 86 comparisons with the 1986 TLV list are possible. The results of the comparison are as follows:

NIOSH (REL) > ACGIH (TLV) = 3
NIOSH (REL) = ACGIH (TLV) = 33
NIOSH (REL) < ACGIH (TLV) = 50

Not all the NIOSH limits are lower than ACGIHs. On average NIOSH RELs were 40% lower than ACGIH TLVs.

The comparison suggests that the need to take into account the feasibility of a limit affects the TLV Committee's judgement. Or it may be that the Committee takes a different view of certain risks, for instance from carcinogens, or is prepared to tolerate a lower degree of uncertainty as to whether a risk does or does not exist, than NIOSH. All of these reasons are probably relevant.

The evidence from the NIOSH/ACGIH comparison suggests that TLVs are not health based limits or else the Committee takes a different view from NIOSH of what should be regarded as a significant health risk.

9.3.3.1.4 Another way of answering the question is to systematically examine the evidence on which the Committee made its judgements. Smyth did this in 1956 and Henschler and co-workers did so as an internal exercise for the West German DFG Committee. There has only been one such evaluation conducted fairly recently which has been published: Roach examined the Documentation of the 612 substances on the 1976 TLV list. He found that of these substances 195, or roughly one-third made reference to quantitative data on "human experience". He examined the original references for 167 of these 195 substances and found 116 TLV's where there was both quantitative data on the concentration to which people were exposed and the number of people effected. Of the 116 TLVs which came into this category the human data for 59 was based on industrial surveys and for the other 57 it was based on laboratory studies of irritation amongst human volunteers. The 57 field studies were divided into those that referred to populations of greater or less than 200 people, and the studies of irritation were classified separately. The numbers affected above and below the TLV were recorded and Roach's tables are reproduced slightly modified as Tables 9.1 and 9.2.

Table 9.1 Number of people with health effects above and below the TLV*

Exposure Level as a proportion of the TLV	Numbers exposed	Number affected	Percentage affected	Number of substances documented in each exposed class
(i) Surveys based on >200 people				
<0.25	244	44	18%	2
0.25–0.49	-	-	-	-
0.49–0.99	549	30	5.5%	4
1.00–1.99	3,766	210	5.6%	3
2.00–3.99	555	348	62.0%	3
>4.0	3,853	1,768	46.0%	5
(ii) Surveys based on < 200 people				
<0.25	54	8	15%	3
0.25–0.49	9	1	11%	1
0.49–0.99	14	3	21%	4
1.00–1.99	217	91	42%	11
2.00–3.99	255	115	45%	9
>4.0	600	364	61%	14

* Based on ROACH (1980)

Table 9.2 Irritancy studies – proportion of groups affected above and below the TLV*

Exposure level as a proportion of the TLV	Number of substances where the majority were unaffected	Number of substances where the majority were affected	Number of substances where all the test population were unaffected
0.25-0.49	1	0	1
0.49-0.99	6	6	5
1.00-1.99	2	8	5
2.00-3.99	0	8	6
>4.0	1	1	7

* Based on ROACH (1980)

The field studies reported effects on lungs, blood, liver, kidneys, central nervous system, bones and skin. In the surveys of 200 people or more 9.3% of the people exposed were affected at exposure concentrations at or below the TLV. For surveys of less than 200 people the percentage was 15.6. In the field surveys of less than 200 people there appeared to be a clear relationship between the number of people affected amongst those exposed and their proportionate exposure above or below TLV value. Thus at <0.25 TLV, 15% were affected and this percentage rose steadily to 61% at exposure >4.0 TLV.

The analysis of the 57 volunteer studies of irritation indicate that in 12 (21%) of the studies either the majority or all the volunteers suffered irritations at or below the TLV.

Roach summarises his findings:

"The overall impression given by the data ... is that TLV's are generally higher than the no-effect threshold of many people and TLV's for irritant substances are particularly high.".

He goes on to remind the reader that whatever the "control limit" the employer has a duty to protect his employees. Thus he argues, "... it would be prudent to endeavour to keep concentrations below, say, one-fifth or one-tenth of ACGIH... TLVs." ROACH (1980).

This work is the most comprehensive analysis of the health effects which are likely to occur at or below the TLV which has been published. It is limited in that the author confined himself to data on "human experience" and thus to roughly $\frac{1}{3}$ of the TLVs and further selection meant that only 116 TLVs out of 612 could be assessed (20%). However, human data has always been paramount in the TLV Committee's lexicon and it has regularly claimed that human experience bears out its judgement. Roach's independent assessment of the TLV Committee's judgement is very important. His methodology appears sound and his conclusion ".. that TLV's are generally higher than the no-effect threshold..." would appear to be in agreement with the other evidence; that TLV's are not health based limits. Also, the percentage of people likely to be affected at or below the TLV gives

some quantitative feel for what the Committee mean by "nearly all" and a "small percentage". between 9 and 15% would, in the author's opinion be very significant percentages.

An Egyptian author surveyed various field studies conducted by himself and others of workers exposed to lead, benzene, aniline, irritant gases and vapours and various air contaminants in the rubber industry. The author of the study concluded:

"Data show that concentrations of air contaminants to which the workers ... were exposed were lower, sometimes well below, the corresponding TLV's. However, workers showed, in most cases, "deviations from normality" as compared to the control." NOWEIR (1975).

The author ascribes the results to a variety of factors mostly relating to conditions special to developing countries. He, at no point, considers the possibility that the TLV's may not be entirely health based limits and that the health effects he and his co-workers found were a consequence of this. While the factors that Noweir considers will exacerbate conditions and reduce the resistance of the exposed population the primary reason for the "deviations" detected is that TLV's are generally not set to prevent all health effects.

The answer to Question 2 must be that TLVs are not health based OELs and furthermore there is, in many cases, a significant chance of health effects occurring at or below the TLV.

9.3.3.2 Question 3 – How scientific are TLV's?

In his paper on the history and philosophy of exposure limits the long standing chairman of the West German DFG Commission outlines what he believes are the four pre-requisites for setting scientifically valid OEL's, HENSCHLER (1984). In summary they are:

(i) The toxic effect must be reversible.

(ii) The elimination of any induced alteration must be zero order ie. Elimination rate must be independent of concentrations so that no fraction of damage is carried over from one shift/exposure to the next, or

(iii) Alterations must reach a steady and tolerable state.

(iv) To accomplish (ii) and (iii) we must have sufficient knowledge or induction and reversion.

He goes on to point out that (ii) and (iii) are rarely available in detail and that compromises are required. He also points out that less than 10% of TLVs are based on "appropriate and sufficient data".

Applying Henschler's notion of "scientific" one is faced with the conclusion that TLV cannot, because of the lack of data, be scientifically derived.

In addition if scientific evidence were the sole basis which the TLV Committee used then TLVs would protect against all the known health effects of the substances assessed. This is not the case as the last sub-section showed. Also it is clear from the earlier analysis of the OEL paradigm, and the process of the Committee, as revealed in the Minutes, that issues of the practicality and feasibility of the TLV's intrude into the Committee's decision making process. Indeed the longstanding secretary of the Committee expressed fear at the idea of TLV derived solely on the basis of scientific criteria:

"There is an element of practicality associated with the TLVs and that has persisted and I hope that it always will. Rather than it becoming a science, because if it becomes a science you are going to see numbers like you've never heard of, there will be fractions of parts per million (femptograms) as TLVs and the world doesn't work that way. WAGNER (1984)

From the analysis in this thesis the setting of TLVs certainly uses and interprets scientific knowledge, but for the above reasons TLVs are clearly not solely scientific. But perhaps a more fundamental question to which an answer can be given concerns the status of the OEL paradigm.

9.4 The OEL paradigm and IH

9.4.1 Is the OEL paradigm a scientific paradigm?

"... men are as much 'victims' of their ideas as beneficiaries of them; traditions prevent men from seeing in their experience phenomena that an alternative tradition might lead them to confront" WOOD (1966) Quoted in GUTTINGS (1980).

This thesis has assumed that TLV setting was at base a process involving scientific assessment of risk. On this assumption a Kuhnian framework was constructed with which to analyse and understand the development of IH, IT and OELs.

Science was seen to be an activity undertaken by groups of people who were trained in and adhered to a paradigm. This paradigm was learnt from text books and by doing the science using exemplary problems, solutions and theories. By an examination of the writings of key individuals a "working definition" of the OEL paradigm was developed. This paradigm was then used to illuminate the working of the TLV Committee. The model fits IH and IT in many ways. And although, as applied sciences other imperatives, particularly professional imperatives were likely to intrude, up to this point a concerted attempt has been made to remain faithful to the Kuhnian framework.

This is the point in the thesis at which to reflect on how well the model fits and the inescapable (as Stokinger might have said) conclusion is that the Kuhnian model does not fit. Perhaps the most telling piece of evidence of all is the Committee's own behaviour.

Kuhn identifies one aspect of "normal science" as the exploration of new applications of a paradigm. Although IH and TLV's have become more refined over time the TLV Committee have never tried to apply the OEL paradigm to other spheres. In fact, although almost from the inception of the Committee various groups have used TLVs (MACs) to derive air and water quality standards, the Committee itself has always warned against such practices. It did so in the Transactions and then from 1958, in the preface to the TLV list. The question then arises as to why, if the OEL paradigm is a scientific paradigm has the Committee warned against doing what other scientists regularly do in their "normal scientific" activity? If the OEL paradigm is scientific and TLVs have been set essentially on the basis of the scientific evidence there is no theoretical reason why they should not be used in other contexts. TLVs could in theory be used to set air water and food quality standards. To do so would require extrapolation to longer periods of exposure, different routes of entry and absorption and a more varied population but, in theory at least, there is no reason why their use should not be extended. In fact, in the mid 1950's Stokinger did just this and derived some water quality standards, thereby going against his Committee's own strictures, but this was never done wholesale by the Committee - Why? The answer is that the Committee knew at some level, perhaps almost subconsciously, that it was not working to a health based scientific paradigm. Other factors influenced its decision making factors derived from the IH and ITs role as professions.

As well as being a science based activity, because IH and IT were applied sciences their process could also be viewed as governed by professional rules and ethos. A major, perhaps the major, difference between the scientific and the professional frameworks was that science was a relatively autonomous activity whereas professions had clients with certain needs.

Many of the OEL paradigms exemplars were forged in the 1920's and 1930's as IH and IT developed. When the IH profession and the paradigm crystalised in the late 1930's/early 1940's the paradigm was a product of the confidence of IH and IT and the pragmatic needs of an applied scientific profession. In a sense the claims IH made for its scientific abilities were oversold because of the pressures on the professional facet of IH, and IT, to serve the clients needs. Ever since the paradigm was forged IH and perhaps to a lesser extent IT, have been trying to reconcile the contradictions in the OEL paradigm, and the confusion that this has caused has allowed a range of different qualities to be projected onto TLVs.

Another interesting question to consider is, why do people inside and outside the Committee still claim that TLVs are essentially science based?

At one level they do so because of the potency of the word science and the image it conjures. Scientific facts have a special quality in modern society, different from other facts. They are regarded as objective knowledge and for all intents and purposes, as the truth. The scientist and the "scientific investigation/decision" have a special objective status. How this almost religious reverence for science has developed in the 19th and 20th century is outside the purview of this thesis. Whatever its aetiology its effects are everywhere to be seen. Most professions, and politicians feel on more secure ground if they can claim that what they are doing or saying is scientific or based on scientific facts.

Right at the start of the ACGIH Threshold Limits Committee, before it was formally started there was a debate within the Conference as to whether to set up a consensus or technical committee (Chapter 7, Section 7.2). The Conference opted for the latter. In doing so it was playing to the technical strength of the profession and it sidestepped the social questions concerning degree and acceptability of risk. It did so by firmly adhering to the OEL paradigm which offered the promise of there being no compromise between feasibility and threshold levels of effect. The question of the degree and acceptability of risks thus never formally arose. The internal beliefs of the Committee, particularly its reliance on the insensitive measure of "industrial experiences" enable the Committee to continue to believe in the paradigm. The project of the Committee was to set OEL's and the paradigm it worked to had the appearance of a scientific paradigm. This is what became, in Kuhn's words, "... the time tested and group licensed way of seeing." The problem was not so much with the Committee's vision, it was more to do with the deep mental model through which the Committee saw the world – the paradigm. The TLV Committee has, in an adapted version of Wood's quotation, "been a victim of its own traditions and paradigm and it has not seen phenomena it should have confronted."

There is another major reason why the Committee emphasised and emphasises the scientific way it arrives at its TLV's and that is less to do with the OEL paradigm and more to do with the professional project of the ACGIH. There is, as has been emphasised, a great deal of kudos to being classified as a scientific profession. With its technical and scientific ancestry such a claim was easy for IH and ACGIH to make and sustain. To insiders IH was, according to the ACGIH, the AIHA and the BOHS an "art and a science", to outsiders it was a science based profession - it was scientific.

With the election of President Reagan the pressure for OSHA, EPA and other US Federal agencies to present their arguments in quantitative terms, including cost-benefit, increased. In 1981, the National Academy of Science (NAS) was asked by Congress to examine the possibility of a unified inter-agency form of quantitative risk assessment. The director of the project made his views very clear on the possibility of scientific risk assessment. His remarks apply equally well to the setting of OELs. He "... labelled as "naive" the underlying promise (behind unified, scientific risk

assessment) that 'matters of science' could be segregated from 'matters of value' and left to an organisation primarily responsive to scientific authority".

In trying to make decisions under uncertain conditions the committee (set up by NAS) commented on, "... how difficult it is to disentangle the mixture of fact, experience often called intuition - and personal values". Quoted in LYNN (1986).

The last part of the quotation sums up the TLV Committee more or less exactly. The analysis has shown that the Committee:

- Has set TLV's based on very little or poor quality evidence (Henschler).
- Has taken the feasibility of the TLV into account and been able to balance this with claims that TLV's are primarily health based.
- Has, in a significant percentage of cases been strongly influenced by industry, accepting its judgement on risk and practicability.
- Has leaned heavily on a judgemental yardstick called "industrial experience" or simply "experience".

Yet at the same time, the process of the Committee is described as scientific. How the Committee could consistently do this has been explored but one of the probable reasons was vividly described by a US District Court Judge called David Bazelon. He said:

"... in reaction to the publics often emotional response to risk, scientists are tempted to disguise controversial values in the cloak of scientific objectivity, obscuring these decisions from political accountability." Quoted in LYNN (1986).

For a variety of reasons, including the pragmatic tradition of IH, the strict adherence to the OEL paradigm and the perceived group need to project IH as a scientific profession, the ACGIH TLV Committee has not been able to resist this temptation.

Returning to the question asked at the start of this sub-section, the answer must be: No, the OEL paradigm is not a scientific paradigm. It is a professional paradigm which contains scientific elements. If this description is accepted, the temptation is to try to extract and isolate those elements of the process which are strictly scientific from those which relate to issues of cost, feasibility, social acceptability and other "non scientific" issues. I like the NAS committee, am not convinced, that this is possible.

But there is an issue which needs to be examined at this point and that concerns science, industry and the influence of industrial consultants.

9.4.2 Science, industry and industrial consultants

In Chapter 8 the importance of industrial consultants to the TLV Committee was seen to be large as was their influence. It was also surmised, with some justification, that professional applied scientists would tend where there was a legitimate range of opinion, to take up positions which best served their clients needs. The implication was that the professional imperative would extend to areas of scientific uncertainty. Given that there is, as has been shown, a large amount of scientific uncertainty in the process of setting TLVs, this leaves a large amount of room for manoeuvre for professionals serving different client groups. What evidence is there that such systematic differences actually occur in practice? Because sociologists have until recently regarded scientific professionals and science as somehow special and not influenced by the societies in which they live and work, very little research has been done into the attitudes and beliefs of scientists. Attitudes and beliefs were thought to have no relevance to the practice of science. More recently, since Kuhn's work, the area has been opened up to far more questioning and critical examination, (see, for instance, COLLINS (1985)). However there is still very little work of relevance to IH, IT and OELs. One person whose work is relevant is Fran Lynn. She considered:

".... non scientific influences on scientists selection of assumptions that form the basis for quantitative risk assessment and risk benefit analysis." LYNN (1986).

Lynn performed empirical research to see whether there were systematic differences between the outlook of different groups in the occupational health professional community. She interviewed 136 individuals randomly selected from the ACGIH, AIHA, AAIH, and the AOHA, dividing the population into government, university and industrial personnel.

She found that industry IHs and IPs were three times more likely than government or university equivalent to believe that Americans were overly sensitive to health risks. Very few in any of the three categories wanted cancer prevention regulations weakened and most were prepared to pay more for consumer products as a result of regulations, the survey produced confused results on whether government should use cost-benefit analysis in preparing regulations. The majority of industry personnel (71%) were in favour while the majority of the other two groups were not. However, of the industrial group only 52% agreed with the statement "society must attempt to place an economic value on human life in order to allocate scarce resources." When asked to use the willingness-to-pay method (currently in vogue with economists), to select an acceptable annual probability of death, many refused and found such an exercise odious. It would seem that by saying "yes" to cost-benefit analysis people meant that to be symbolic of "a desire to lessen regulatory burden" or a sign of hope, "that a technique can be found to make environmental decision making easier". The industry group were in favour of methods which, "...provided objective answers to uncertain and value laden processes."

The sample population were asked whether they agreed or disagreed with the following statement:

"A substance which is shown to cause tumours in experimental animals should be considered a carcinogen, thereby proving a risk to humans."

They were also asked whether they believed in thresholds for carcinogens. There answers were as follows:

	GOVERNMENT	UNIVERSITY	INDUSTRY
Animal carcinogen = human carcinogen	69% agree	52% agree	27% agree
Thresholds exist for carcinogens	37% agree	61% agree	80% agree

The attitudes expressed correlated also with the political affiliation of the respondents as follows:

	CARTER†	REAGAN†
Thresholds exist	36%	63%
Thresholds don't exist	64%	30%

† Vote in 1980 Presidential election.

Similar patterns existed for the attitudes expressed towards Americans and risk, cost-benefit analysis and animal tests.

Lynn sums up for her findings thus:

"... in areas of science where there is no data to distinguish choices of models or assumptions, scientific choices correlate highly with personal political beliefs." p46/47 Ibid.

What conclusions can be drawn from Lynn's research?

The idea that the views of professions correlate with the needs of the professions client groups is borne out. The evidence supports the analytical framework developed in Part 2. Industrial consultants do not just present their companies' data; they adopt scientific positions which support their clients' case for a practical or achievable OEL.

How far do the different attitudes of the different groups examined by Lynn push the professions? Lynn's conclusions suggest that it is simply a matter of lack of data. That would suggest that if

there were more data the political attitude of the respondents would, ultimately, have no effect on their scientific beliefs. There would be a convergence of opinion. The paradigmatic/professional framework suggests that this is not and would not be the case.

From the Kuhnian paradigmatic viewpoint there are always justifiable differences of interpretation and of hypotheses based on the same paradigm and the same data. Schools of thought are completely legitimate within a Kuhnian framework and in reality one cannot even speak of value neutral "data". For data itself is produced via an observational/experimental methodology and different schools will argue about the validity of another school's methods and thereby attach more or less importance to the "data" produced.

From the professional viewpoint it is the duty of a professional to identify and serve the clients needs. Lynn's work suggests that the occupational health professions in industry interpret these needs in conservative terms. More data would not change this relationship or approach.

The paradigmatic and professional frameworks, coupled with the analysis in this thesis, plus the evidence of Lynn's research suggest that occupational health scientific professionals working in industry belong to or become converted to schools of thought which bolster a conservative view of the risks of exposure to toxic chemicals. Does the conclusion in this chapter that the OEL paradigm is a professional and not a scientific paradigm make any difference? The answer is that it does but only in the sense that a professional paradigm allows an even greater breadth of opinion than a scientific paradigm. But perhaps more importantly it legitimates the systematic differences between industrial, governmental and academic IH's and IT's such that these differences are not regarded as noteworthy. This would explain how the TLV Committee could encompass a very wide range of systematically different opinions and yet see no contradiction – it, "has not seen phenomena it should have confronted."

Industrial consultants have not just supplied the Committee with data and the industrial assessments of feasibility, they have injected schools of scientific thought and affected the scientific vision of the Committee.

The closer one looks at the process of the TLV Committee the more difficult it is to disentangle the mixture of scientific data, industrial experience, professional imperative, attitudes and the pressure of practicality.

What is of more immediate interest is the answer to Question 1, asked at the start of this section; what are TLV's.

9.5 Reasonably practicable OELs

9.5.1 TLVs are reasonably practicable OEL's

"The TLV Committee has not made it easy to ascertain the criteria it uses in establishing TLVs. It has made no explicit statement about the appropriate trade-offs between health and economic costs." MENDELOFF (1988).

A reasonably practicable OEL is one where the degree of risk posed by a substance is weighed against the feasibility and cost of controlling exposure to that substance.

From the Minutes examined in the last chapter and the discussion of this evidence it is clear that an "element of practicality", as Wagner put it is present when the Committee sets a TLV. Even where Stokinger is writing in scientific journals and projecting TLVs at their most scientific he still occasionally introduces the need not to "overprotect" the worker or "over engineer" the process. And the OEL paradigm offers a promise that risk and reasonable control measures can be reconciled. Add to this the very pragmatic, practical project of hygienists and its almost inevitable that the the TLV Committee will take feasibility into account in setting TLVs. Indeed, given the professions project, and the Committee's remit and paradigm it would be surprising if they did anything else.

Apart from the analysis of the Minutes there is other evidence that TLV's are reasonably practicable OELs.

Mendeloff had access to OSHA's hygiene database and he examined the sample records for 90 chemicals whose TLV had been reduced by the ACGIH since OSHA adopted the 1968 TLV list. Because the median reduction had been 50% he examined the records for samples which fell in the range 50-100% of the OSHA PEL for a 26 month period from 1979-81. Although the hygiene records were incomplete, the analysis did show that for 60 of the 90 chemicals all samples were below 50% of the OSHA PEL. Mendeloff comments:

"... it seems fair to conclude that for many hazards adoption of the ACGIH limits would make little difference". MENDELOFF (1980).

It would seem, from this evidence that compliance with TLVs can be attained without too much difficulty. Indeed the earlier quotation from Dow indicates that the larger companies, with professional staff, aim to comply with the TLV or some fraction of the TLV. This is certainly the practice in many such companies in the UK.

Another way of examining whether TLV's are set at reasonably practicable values is to compare the TLVs with other OEL lists which are known to be reasonably practicable.

In 1956 the UK Chemical company ICI produced a "Manual of ICI practice" which listed a range of air sampling and analytical techniques and included a table of "Toxic concentrations of various gases, dusts, fumes and metals in the workplace." STRATFORD et al (1956). The toxic concentrations were listed in three columns the last of which was entitled "Concentrations in the general atmosphere of the plant greater than those given below indicate unsatisfactory conditions." These were equivalent to the TLVs and in fact were replaced by the ACGIH values in the 2nd edition of the manual in 1965. It is safe to assume that ICIs toxic concentrations were reasonably practicable values based on the company's assessment of the risk each substance posed and the company's own practical knowledge of how difficult it was to control. Choosing where "reasonably practicable" lies will always produce a spectrum of judgements. From the ACGIH Minutes it would seem companies, in general, select from the high end of the spectrum. A comparison of the ICI list and the equivalent 1956 TLV's is given in Table 9.3. There are 55 ICI values of which 48 ICI/ACGIH comparisons are possible. The 55 limits cover materials produced or handled by the company. By 1956 the ACGIH had set 216 TLVs. The 48 ICI/ACGIH comparisons breakdown as follows:

ICI OEL = ACGIH TLV - 15
ICI OEL > ACGIH TLV - 16 (x 2.5 on average)
ICI OEL < ACGIH TLV - 17 (x 0.5 on average).

There is considerable overlap between the two lists; roughly one third of the TLV's were the same as ICI's internal OELs, one third were lower and one third were higher. About two thirds of ICI's OELs were the same value as the TLVs or lower. However, because of the proportionate differences, on average ICI's OELs were about 25% higher than the TLVs. The conclusion is that overall TLVs were somewhat more stringent than the in-house ICI company OELs, but that the considerable overlap would indicate that, in 1956, TLVs were reasonably practicable OELs pitched towards the lower end of the spectrum.

Another more contemporary list of reasonably practicable OEL's is the UK control limits:

"They are limits which have been judged after detailed considerations of the available scientific and medical evidence to be 'reasonably practicable' for the whole spectrum of work activities in Great Britain." HSE (1987).

Table 9.3 A comparison of the ICI "TLV" with the equivalent 1956 ACGIH TLV's

Name of Substance	ICI Value	ACGIH TLV
Acetic acid	20	10
Acetone	400	1000
Ammonia	100	100
iso-Amyl acetate	100	200
Aniline	10	5
Arsine	0.5	.05
Benzene (Benzol)	50	35
Bromine	0.5	1
n-Butyl acetate	200	200
Carbon dioxide	5000	500
Carbon disulphide	10	20
Carbon monoxide	50	100
Carbon Tetrachloride	50	25
p-Chloroaniline	2	–
(mono) Chlorobenzene	75	75
Chloroform	50	100
0-Dichlorobenzene	25	50
Ethanol (Ethyl alcohol)	1000	1000
Ether (Diethl ether)	500	400
Ethyl acetate	400	400
Ethylene chlorohydrin	2	5
Ethylene dichloride	50	100
Ethylene glycol dinitrate (Ethylene dinitrate)	0.5	–
Ethylene oxide	10	100
Formaldehyde	10	5
Hydrazoic acid	1	1
Hydrogen chloride	10	5
Hydrogen cyanide	10	5
Hydrogen fluoride	2	3
Hydrogen sulphide	20	20
Methanol (Methyl alcohol)	200	200
Methyl acetate	100	200
Nitrobenzene	1	1
Nitromethane	200	100
Nitrous fumes (as NO2)	10	5
0-Nitrotoluene	1	5
Phosgene	0.5	1
Styrene	100	200
Sulphur dioxide	10	10
Tetrachloroethane	10	5
Trichloroethylene	400	200
Toluene (Toluol)	100	200
(o, m and p) Toluidines	5	–
Xylenes (Xylols)	100	200
Xylidines	5	–
Chromates	0.1	–
Dinitrocresol (and salts)	0.5	2.5
Dinitrophenol (and salts)	1	2.0
Lead (and salts)	0.15	1.0
Mercury	0.1	1.0
a-Naphthylamine	0.01	–
b-Naphthylamine	0.01	–
"Psrathion"	1	10.0
Trinitrotoluene (TNT)	2	1.3

Vapours/gases = ppm Fumes/dusts = mg/m^3

There were 30 control limits in 1987 and 25 comparisons are possible with equivalent TLVs. The comparisons breakdown as follows:

HSE control limit = ACGIH TLV - 11

HSE control limit > ACGIH TLV - 10 (x 2.5 on average)

HSE control limit < ACGIH TLV - 4 (x 0.3 on average)

As with the ICI/ACGIH comparison there is significant overlap between the two lists. Some 60% of HSE's control limits are lower or the same value as the equivalent TLVs though the proportions are different. Roughly 45% of the control limits are same as the TLVs while 16% are lower. On average control limits are 60% higher than the equivalent TLVs.

This contemporary evidence again suggests that TLVs are reasonably practicable exposure levels. Given the relative ease of compliance with TLVs identified by Mendeloff this evidence also suggests that HSE's judgement of reasonably practicable lies at the higher end of the spectrum, and that it is more conservative than the ACGIH.

Keeler and co-workers approached TLVs from an economic, cost-benefit viewpoint. They were concerned that TLV's might be "unreasonable" and might cost too much to comply with compared with the benefits realised. This work has relevance to this analysis. In particular they compare the approach of OSHA and ACGIH and make the following comments:

"Compared to OSHA, the ACGIH is fortunate to have a blend of interests. The members are governmental and academic industrial hygienists. Their professional training and careers have been aimed at protecting workers.... However, their standards are voluntary and must seem reasonable to workers and industry if they re to be adopted and enforced."

They go on to mention industry lobbyists and how ACGIH is "freer of political pressure" (than OSHA) and conclude:

"Unlike OSHA, they (ACGIH) are not compelled by law to ignore economic costs in their standard setting. Indeed, reading between the lines of the Documentation one sees a rejection of the idea that chemicals can be controlled to a point that they have no observable effects on comfort or health. **Their philosophy** (like that of the NCRP) **is more a balancing of risks with the practicability of reduction.**" KEELER et al (1982) (Author's emphasis).

The TLV Committee has always struck some kind of balance between risk and the practicalities of control. That is why there are risks to health at TLV levels of exposure and these same levels are achievable. That a balance is struck is implicit in the OEL paradigm but never formally stated. It occasionally intruded in to the pronouncements of individuals. The whole process of the TLV Committee points in the direction of reasonable practicability. Once the altered status of TLVs is accepted the reasons for the careful wording of the TLV definition become clear. It represents a

"cloak of scientific objectivity" based on a professional paradigm. It has caused and it continues to cause a great deal of confusion.

9.5.2 What does reasonable practicability mean in practice?

If the proposition that TLVs are and always have been reasonably practicable OELs is accepted then the status of TLVs and the process by which they are set can be viewed in a new light and taken on different meanings.

Exposure measurement and the assessment of the practicability of controlling exposure to a certain level take on much bigger roles.

The slow rate of change in TLVs plus the relatively small reductions when changes do occur have been given special meaning in the past. Part of this meaning has already been touched on in Chapter 7 when the image of TLVs was discussed. The slow and low rate of change coupled with the relatively small reductions have all been presented as evidence that the Committee has more or less got it right – the TLVs are close to the threshold of effect. This reinforces the image that TLVs are, more or less, health based OELs. Viewed from the standpoint of reasonable practicability there is an additional reason for the low rate of change - the difficulties of controlling exposure down to a particular level for key processes. To understand why such processes are important it is necessary to understand what the working environment looks like in quantitative terms of personal exposure.

It is often the case that if the results of a series of personal TWA exposure measurements on people doing similar work are examined, it is found that the data are best described by a lognomal distribution. This is a skewed distribution with a long-tail to the right which indicates that a small proportion of results are considerably higher than average, Figure 9.2. A simpler and more accessible way of understanding lognomally distributed data is to plot them directly onto log-probability paper. A lognormal curve becomes transformed into a straight line the slope of which indicates the variability of the results, Figure 9.3. If the results are plotted by process using or releasing the same substance, a family of lines or distributions showing the average exposure for each process and the variability of those exposures is produced, Figure 9.4. The last figure represents the reality of the work environment facing any committee wishing to set reasonably practical OELs. The reason why certain processes become the focus of attention is clear. Process 1, in Figure 9.4, because it is the highest is the process which will determine where the OEL will lie. Exactly where the OEL is set will depend upon how the committee balances its perception of the seriousness of the health effects caused by the substance in question, the likelihood that the effect will occur at a certain level of exposure and also its perception of how difficult it will be to control exposures at the top end of Process 1's distribution. The uses and importance of this model of the work environment are discussed further in the last Part of the thesis. What needs emphasis here is that two factors will affect whether a TLV is reduced. The first factor is the one that is

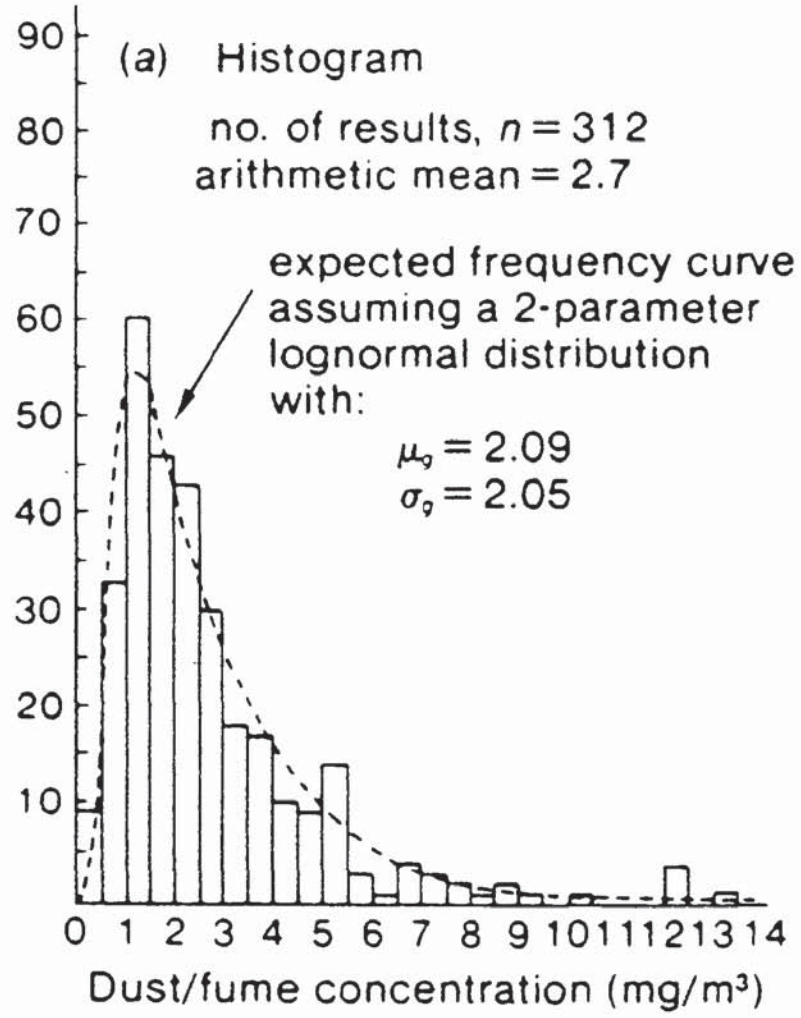


Figure 9.2 Frequency distribution of 8 hour TWA Dust/Fume measurements, (taken from CULLIS and FIRTH (1981)). See Figure 9.3 for a log-probability plot of the same data.

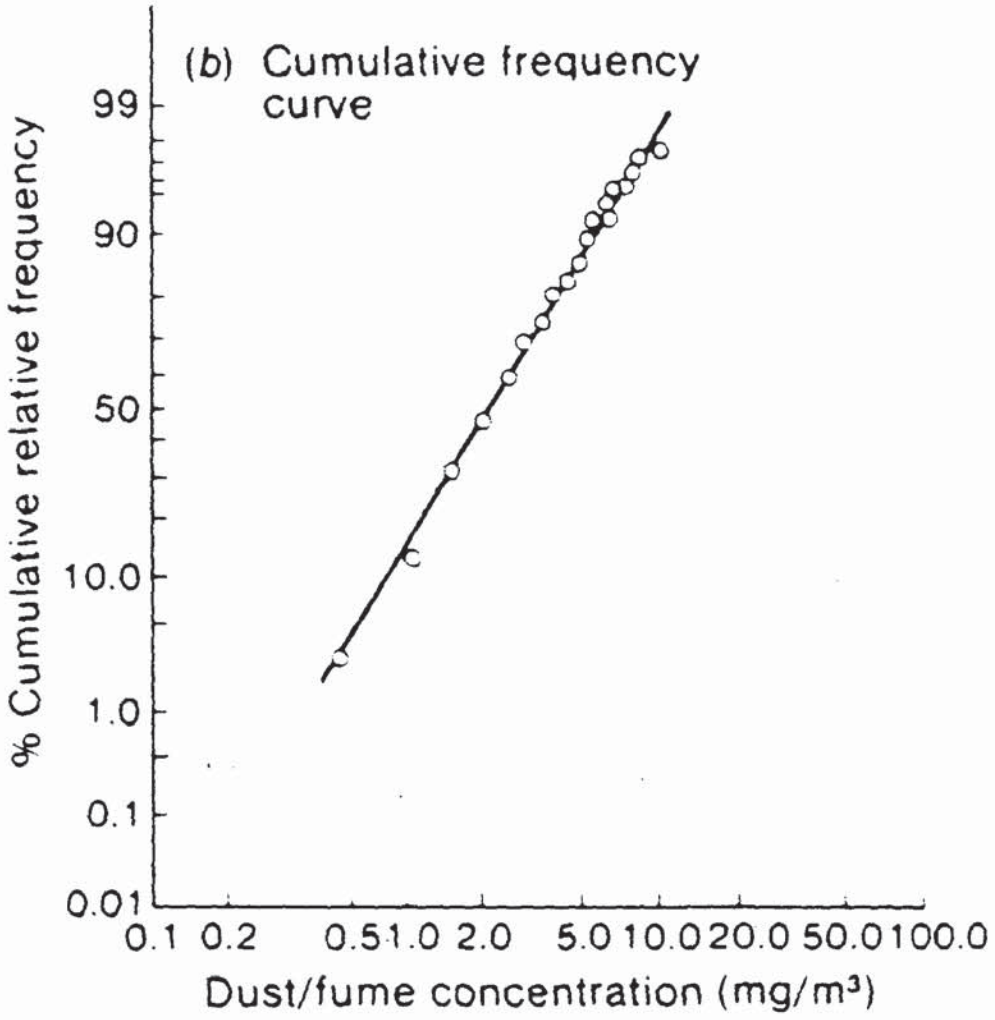


Figure 9.3 Frequency distribution of 8 hour TWA dust/fume measurements plotted on log probability paper, (taken from CULLIS and FIRTH (1981)). See Figure 9.2 for a different plot of the same data.

Figure 9.4. Distribution of sampling results for four processes using the same substance.

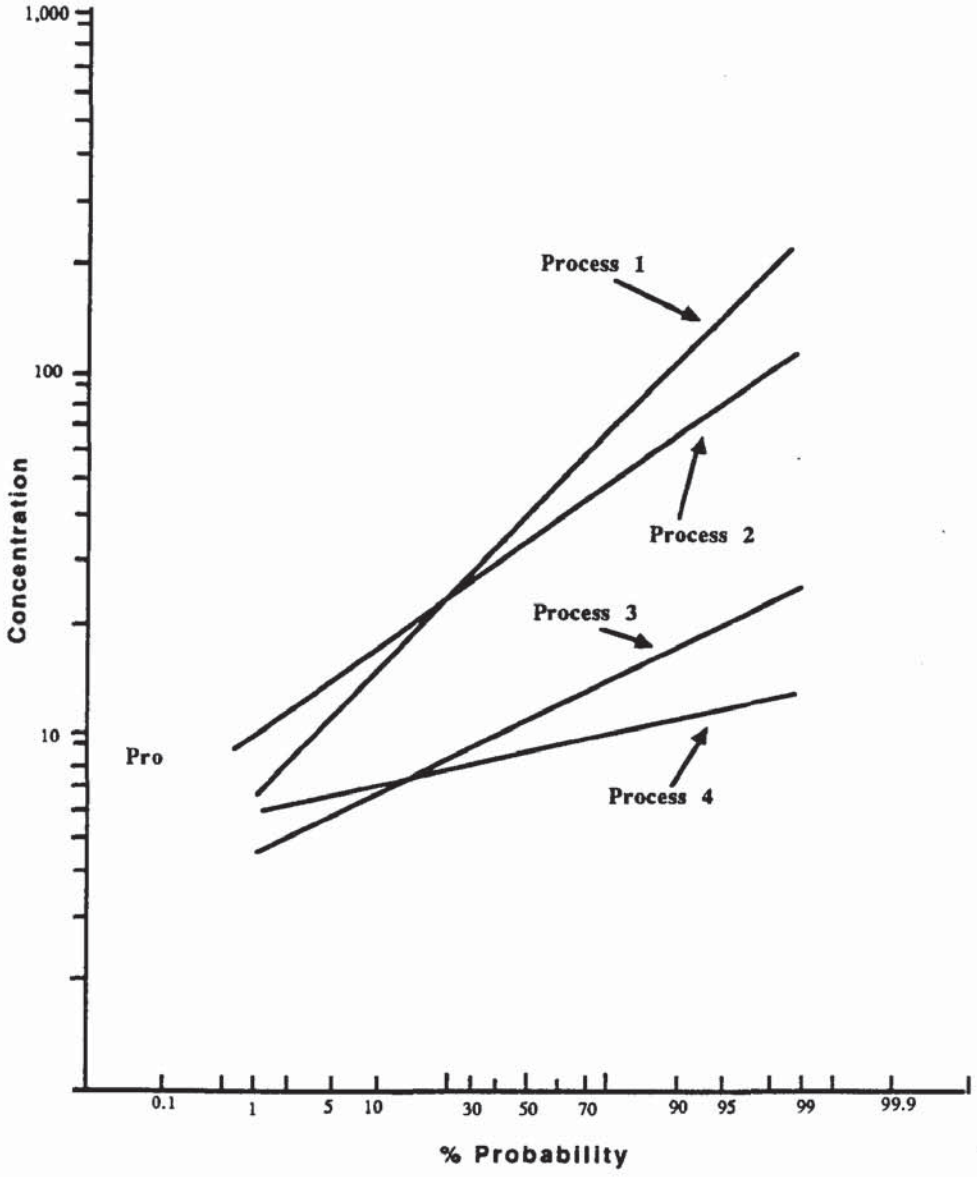


Figure 9.4 Log probability plot of four processes using or emitting the same substance

always emphasised. If there is evidence that a previously recognised health effect occurs at a level of exposure lower than the TLV, or, and probably more influential, if a new health effect is connected with exposure to the substance in question, there is pressure to reduce the TLV. The first factor is usually the initiator of change. How much the TLV can be reduced will be determined by how much conditions have improved in industries using Process 1 (Figure 9.4). If the process has been phased out or brought under greater control then the TLV can be reduced significantly perhaps by 90%. If there has been little change in Process 1 or the controls applied to it the TLV reduction will be circumscribed and probably far less than 90%.

The "anchoring effect" of other expert opinion has already been touched on, for example to the effects of the TLV Committee on the MAK Commission and vice-versa. This judgemental anchoring is important, but the anchoring effect of practicability is probably far more important.

In setting a reasonably practicable TLV the Committee takes into account the health effects of the substance in question. If a substance was regarded as a simple irritant but then new evidence convinces the Committee that it is a human carcinogen, the pressure to reduce the TLV will be greater than if the new evidence simply indicated that the substance was slightly more irritant than had been appreciated. And if Process 1 is a minor one and there are practicable measures which can reduce exposure considerably then the TLV can be reduced by a large amount. But, if Process 1 is a major one and large reductions in exposure are not judged to be practicable then the TLV cannot fall drastically and remain reasonably practicable.

The slow and low rate of change of TLV's coupled with the relatively small reductions when they do occur have more to do with judgements concerning the practicability of control of type "1" processes than any estimate of increased risk.

9.5.3 Reasonable practicability and the paradigm

"Scientists really do think that reasoned argument, and evidence are the crucial factors in the decisions they make in the cause of their research but time and again in the history of science we see in retrospect that other factors have had significant influence." HULL (1988).

TLVs are reasonably practicable OELs produced according to a professional and not a scientific paradigm. Yet, at the same time, there is the strongly held view of users of TLVs, the clients of the IH and IT profession, and the professions themselves, that TLVs are health based OELs and the process by which they are derived is scientific. Various partial explanations have been offered as to how the professions involved in particular could make such claims. Now is the point to recap and consolidate. The explanations of the behaviour of the ACGIH and the TLV Committee lie partly in history, partly in process and partly in image. But before attempting a synthesis a short digression into phenomenology is called for.

"Phenomenologists .... have argued that experienced reality in every day terms is a process of relationship between self and what is socially identified as the objective world. Any abstraction in regard to the world - and language is the most far-reaching abstraction confronting us - can no longer be identified as inherent in that world; it is, rather, a social object. Our descriptions of things are possible because we use an agreed upon system of symbols... Without prior social exchange to arrive at common meaning .... words ... can have no significance for anyone else. What this implies is that all meaningful action is **by definition** embedded in a social network.

What we mean, thus, in using the term **subjective** is that the action, verbalisation, or whatever performed by another has insufficient social basis to be meaningful. Equally, what we mean to be **objective** is not that which is independent of our own social action ie. directly accessible and meaningful to each individual - but rather that which **does** have a relatively stable, agreed upon significance." NOBLE (1978) Emphasis in the original.

The history of IH showed that it evolved from the engineering and chemical engineering professions; numerate, empirical professions which as a matter of course look to numerical, quantitative solutions to problems. That was their forte and their strength and that is what their clients employed them for. IH's like Patty, Elkins, Smyth and others believed in the efficacy and progressive nature of this approach. When combined with the OEL paradigm it also had the advantage of appearing to side step the questions of acceptability and degree of risk. But it was not only the ethos of the professions from which IH evolved which affected the process of TLV production. There was also the position, nature and internalised beliefs of the TLV Committee, which interacted to reinforce each other. And finally the OEL paradigm reinforced and supported the organisational position of the IH profession. IHs were, and still are, mainly technical advisers or middle mangers with little influence of the design of the production process or product design. They were not in a position to eliminate or substitute a process or substance and the OEL paradigm reinforced this position. It offered the promise that all potentially toxic substances could be controlled by IH control methods. It was simply a matter of applying the right methods and applying the right OEL. This belief amongst IH's and their clients has been systematically reinforced by the image TLVs project. And perhaps it is the image that the single figure OELs tabular presentation, coupled with absolute or carefully but vaguely worded qualifications present that most strongly sustains the OEL paradigm.

Having recapped it is time to consider the phenomenology of OEL setting, in particular how the TLV Committee could set reasonably practicable OEL's and yet claim they were scientific and, in effect, health based. And these beliefs are widely held outside the IH profession, amongst other professions and amongst client groups.

In the formative years of IH various OELs were propounded. With Winslow practicability dominated, although as we saw with the benzene permissible level this soon became transmuted into a health based "safe" OEL. With Drinker and Hatch in the 1930's they clearly believed that threshold doses must exist but they did not have the confidence to set OELs – indeed they warned against the idea at some length in the first edition of their book, DRINKER and HATCH (1936). The "... relatively stable agreed upon significance" of OELs by IHs did not occur until the profession gelled. The gelling of the profession and the crystallisation of the paradigm happened

together. IHs were not the first nor the only people to postulate a set of fundamentals like the OEL paradigm but they were the first group to apply it on a large scale and in a consistent fashion. And the particularly potent part of "agreed-upon" reality for IH's and others on the TLV Committee was that the process worked: TLV's did improve conditions and "experience" told them that TLVs were more-or-less "safe". They convinced themselves, new entrants into their profession and, most importantly their clients on an "agreed-upon system of symbols". This stability of meaning has given the producer and user of TLV's an overwhelming feeling that TLV's are in some way objective knowledge. Given the paradigmatic feel and community based project of IH it is but a short step to claiming TLV's are based on a scientific process. The conclusion of this part of the thesis is that it may be objective knowledge to the Committee and the many paradigm adherents but it is not scientific knowledge produced by an autonomous scientific community in the Kuhnian sense. They have a "group licensed" way of seeing the world but through a professional and not a scientific lens. It is too bound up with the professional project of the IH profession and the practical needs of its clients. And these needs would appear to effect even the scientific views imported into the standard setting process.

9.5.4 Implications

(i) Although TLVs offer some protection health effects are likely to occur at or below the TLV for many substances.

(ii) TLVs are a restriction on the uncontrollable use of a process or substance. However they do not stop their use and in fact, as Sachs pointed out in regard to ICRP ionizing radiation standards they may facilitate the use of a new process or use of a substance. TLVs have a strong legitimating function.

(iii) A large number of countries have adopted TLVs particularly those which had nascent IH professions or equivalent professional groupings. In most if not all of those countries TLVs or the national equivalent are imbued with the same qualities as the TLVs based on a similar, if adapted professional paradigm. This paradigm contains all the contradictions and confusions of the original.

(iv) The question of reasonable practicability always centres on specific processes (Process 1 in Figure 9.4). Because of this, many processes remain untouched by TLV's. This is a fundamental flaw and arises from the paradigm and the practice of OEL standard setters and their clients – the perceived need to set a single number, and yet a reasonably practicable OEL. To avoid this the restricted definition of reasonably practicable will require a change in the standard setting process.

(v) The production and use of health based OELs will require a different standard setting process and paradigm. Because of the hegemony of the OEL paradigm and TLVs, the widespread adoption of a new standard setting process is going to be difficult, a point that is amplified in the next sub-section.

9.5.5 Changing paradigms

The fact that the OEL paradigm is a professional paradigm has important implications as regards any attempt to change the paradigm. In a Kuhnian framework paradigm change occurs because of persistent anomalies thrown up by the workings of "normal science". Sciences are full of such anomalies but they are either regarded as curiosities or puzzles that can be solved. Occasionally they become so important to the scientific community that a paradigm shift occurs in order principally, to accommodate the anomaly. The Kuhnian view is that anomalies can be generated from within the community or can be introduced via the work of other related scientific communities. An important question is whether the same rules apply to the professional OEL paradigm?

In the sense that anomalies generated in related scientific fields have shaken the OEL paradigm and produced a spirited defence, the answer is yes. The developments touched on in Chapter 5 in biology and the USSR led to a paradigm crisis in the 1960's and 1970's, which most of those within the IH/IT community now regard as solved. But in a very important other sense the professional OEL paradigm does not work to the same rules as a Kuhnian scientific paradigm. For Kuhn, paradigm shifts result in a change of vision, a gestalt jump within the scientific community's world view. Crisis resolution is a process internal to the scientific community. With professional paradigms this may not be the case. Small shifts brought about by theory or empirical developments can occur in a Kuhnian fashion but large shifts cannot. This is because applied sciences such as IH and IT, working to professional paradigms, have clients. It is not only the professions who are convinced of the "agreed-upon significance" of their objective knowledge, it is also their clients view. For an OEL paradigm shift to occur will require not only a change of vision within the IH and IT professions but also a similar change amongst their various professional clients including: governments, employers, trade unions and insurance companies. Such clients add enormous inertia to the process of professional paradigm evolution. It is probably this inertial effect which goes a long way to explaining the longevity of the OEL paradigm. Although it was the IH and IT professions who forged and promulgated the OEL paradigm it now has enormous functional significance for a large range of powerful and yet disparate groups. The main difficulty in inducing a paradigm shift will probably not be in the realm of scientific debate but in the widespread commitment to the old OEL paradigm amongst the more powerful client groups.

9.6 Commentaries on ACGIH TLVs

9.6.1 Samuels

Sheldon Samuels has been a vociferous, if not on occasions a pompous and pugnacious critic of TLVs and the ACGIH.

In 1981 he described ACGIH's actions as:

"... praiseworthy as voluntary private acts by government officials, acting as individuals in alliance with management specialists, to control acute effects ... (though) little attention was given to chronic effects."

He continues a little later with a dramatic accusation:

".. the ACGIH TLV Committee exists almost solely for the purpose of exporting environmental values ... to other countries less stringent than those of the United States. This is being done with the knowledge and consent of government and management specialists in the recipient countries, but without the knowledge of most of their legislative or rule-making bodies."

He claims a little later that:

"... European countries recently have abandoned dependence on ACGIH." SAMUELS (1981).

Six years later he has more praise and, at the same time, more opprobrium for the Committee:

(In the early days) "membership (of the ACGIH) was an act of courage by those civil servants and management professionals who saw a clear need and a clear means of meeting that need. ... When the committee was initiated, the environmental movement did not exist... Responsible elements in management needed some tool to move the irresponsible."

At the meeting at which he said this he was given the William Steiger Memorial Award by the Conference. He thanked the Conference for acting, "... in the finest tradition of a democratic society." But he also said:

"... I will not slow my efforts to put your TLV Committee out of business. He ends by saying:

"End the TLV Committee. Force its true sponsors to find another front for their immoral mayhem." SAMUELS (1987).

Samuels appears to believe that the Committee has simply become a tool of industry. In the 1981 paper he recommends the OSHA standard setting procedure as more thorough and the final standards, with guidance on control, personal protective equipment (PPE) monitoring and

surveillance, as proper "standards". The thoroughness of the OSHA process is beyond dispute but it is a slow process as Mendeloff has pointed out and meanwhile ACGIH has moved on. Although Samuels is correct in asserting that industry does influence TLV setting, the control is by no means absolute. He offers little evidence of total industrial control apart from what could almost be described as name-calling. He does, in an early paper, take issue with the need to set one OEL for a substance and this point will be raised and examined again in the last part of the thesis.

A more measured critique of the ACGIH TLV Committee than Samuels's has be published recently.

9.6.2 Castleman and Ziem

The work of these research workers has already been referred to. By combing the TLV Documentation they identified instances where great reliance was placed on industry based research. They attempted to get copies of these unpublished research reports and evaluate them. Their approach yields a similar percentage figure to mine – 16% compared with 13% of TLVs directly based or strongly influenced by industry. They also identify certain industrial consultants as especially influential.

They accuse the Committee of turning, "... a blind eye to conflicts of interest," and then set out their own views about, "health and safety professionals".

"... from those who would resolve the benefit of doubt in assuring the fullest worker protection to those who are more sensitive to corporate financial priorities where health and safety is in practice regarded as an expenditure to be controlled as much as possible." CASTLEMAN and ZIEM (1988).

They go on to claim that the TLV Committee has never acknowledged this, "... reality or attempted to achieve a balance between corporate or union affiliated health professional." Ibid. Trade union representatives, when they were present, were "tokens" or "marginalised" by the sheer force of numbers of adversaries with vastly superior technical resources." Ibid.

In their conclusions Castleman and Ziem make further observations of, and accusations against the Committee:

(i) "The unavailability of unpublished corporate "documentation" precludes scientific scrutiny of the primary basis for nearly one-sixth of the "documented" TLVs."

(ii) "The documentation of TLV's for their own companies' products by industrial members of the TLV Committee constitutes a major conflict of interest."

(iii) "The history of dominant corporate TLV Committee members as "consultants" was deceptive.

(iv) "... documentation on many chemicals seems to have been prepared with minimal review of the literature"

(v) The TLVs are assumed by many to be first world, "first class" guidelines for worker protection. The consequences of such misplaced confidence in the TLVs are "profound and global".

They go on to say: "The credibility of the ACGIH limits as scientifically, independently, and verifiably determined persist as an obstacle to a better standard of worker protection."

They make several comments and proposals:

(i) Hygienists take a more critical attitude towards TLVs.

(ii) Concerning the proposal for OSHA to adapt the current TLV list, they acknowledge that, "... for some chemicals this may represent an improvement." But they go on to warn that, "... we cannot assume that the current TLV's are scientific or adequate," and that NIOSH levels should be adopted where they are stricter.

(iii) They argue that there are no "better trained and equipped groups... in North America, Europe and elsewhere," and that, "... it now seems appropriate for an international effort to be mounted to gradually replace the TLVs .....Corporations with their own internal lists ... can contribute to this process...".

(iv) They want to see an openness of process with "the exclusion of financially interested parties" and "conflicts of interest". They comment; "public access to minutes of meetings should be assured and provided for."

(v) They argue against the idea of "concept of "safe" exposure levels", and claim that this, "... is inherently unscientific". As with Smyth, some 40 years earlier they do not like the words "threshold limit"; "discarding the term "threshold limit" is a necessary first step in correcting this false ideology of the past."

(vi) They continue with the observation that the process of deciding on a figure "... selected as "acceptable" by one social group (scientists) for another social group (workers) is very much a political as well as scientific process." And they commend the Norwegian approach of separating the scientific process from the political process. Finally they argue that apart from freeing the OEL process from "... undue corporate influence", the decision process, "... must also include substantial participation by representatives of exposed persons."

In the same issue of the American Journal of Industrial Medicine in which the Castleman and Ziem paper is published Maltoni, a well respected Italian toxicologist, announced that the Collegium Ramazzini had set up a "Commission of International Standards" MALTONI (1988). There was also a very significant response to the Castleman and Ziem paper, in terms of letters to the Editor, in the same issue, (18 pages in all).

Compared with Samuels the authors certainly marshal an argument bolstered by an analysis of the TLVs Committee's use of industrial data but in a number of important ways their analysis and conclusions are shallow and confused.

9.7 Concluding comments on TLVs

If one accepts TLV's for what they are – reasonably practicable OEL's – then contact with industry makes sense. It especially makes sense if you have a positivistic view of science as objective knowledge and scientists, whether employed by industry or government, as dispassionate seekers after and purveyors of value-neutral knowledge. One can argue about the degree of contact and the probity of allowing industrial IHs and ITs into key positions when deciding on the TLVs for their company's products but the fact is that to set reasonably practicable OELs a standard setting body needs contact, in one shape or form, with industry. In the days before OSHA/NIOSH the Committee had little choice but to make use of its industrial contacts. And it is clear that it saw nothing wrong with this approach – it continued the long tradition of the PHS. It is also clear from the analysis in this part of the thesis that there was a price to pay. The cost was in terms of restricted room for manoeuvre over issues of practicality and a tendency to select from within what might be described as conservative schools of scientific thought.

It is worth pointing out that if the Committee sees its task as setting reasonably practicable OELs then there is not necessarily a "conflict of interest" in involving industrial consultants in a significant way. Their practical, tacit understanding of their companies' processes would be "invaluable", as Wagner put it. It may be regarded as unwise to allow industrial consultants too great an influence on judgements of where reasonably practicable OEL's lay, but it was not, and is not 'against the rules' in a committee setting such standards.

Castleman and Ziem are quite correct about the status and influence of TLV's and they show that industrial consultants have been influential. They regard some "TLVs as not scientific or adequate...". Their work and mine should help dispel some of the myths that surround TLV's. But they do not address a series of structural and related problems concerning applied science, professional paradigms and single figure OEL's in general. They offer little explanation of the consistency of the Committee's behaviour over a 40 year period. The explanatory framework developed in this thesis is that the Committee has been working to a professional paradigm, it has a certain view of science and it has been assessing risk to health and at the same time practicality of control. According to the paradigm this mix of concerns is permitted and legitimate. It is not, as Castleman and Ziem claim, that the Committee has not faced "reality". It is that the Committee has a different "reality" to them. The problem with their analysis is that it tends to focus on the role of individual actors and does not consider either the actor's scripts, the theatre in which they perform or the society which supports this artform. Their view thus boils down to what, at the risk of caricature, can be described as a 'goody' versus 'baddy' view of the world. All toxicologists and

other health professionals who work for industry are 'baddies' and all equivalent professionals who work for government, academia and unions are 'goodies'. If any of the latter work for industry they become 'baddies'. This view leads them into a somewhat paranoid, conspiracy based view of society. They conclude that if only an OEL committee could isolate itself from industry, was composed of only untainted 'good men and true' who, with representatives of exposed persons, set 'scientific' standards then all would be well with single figure OELs. This, it is clear from the foregoing, is palpably not the case and there are more fundamental structural problems to overcome. Having said this their attack on corporate influence has helped to undermine the view of TLVs as health based OELs and they do have many telling points to make.

Their point concerning the availability of unpublished company documentation and industry based consultants producing documentation for their own company's products are well made. Such documents should be open to critical scrutiny. And it is deceptive and disingenuous to list but not identify industrial consultants yet at the same time claim or infer they have limited influence because they cannot vote. The fact that the Committee started by identifying industrial consultants but soon stopped suggests a certain guilt. The Committee wanted the experience and knowledge of such consultants to set reasonably practicable OELs but it did not feel it could admit this. Publishing an evergrowing list of industrial consultants could have tarnished the scientific health based image of TLVs.

Castleman and Ziem's point about the lack of involvement of "union affiliated health professional" is unfair. It is not just that such people were few and far between in the early 1970's, and that they were virtually non-existent before this time, there is also the reluctance of such people to become involved with TLVs. The views of William Wagner have already been referred to – that union professionals were reluctant to become involved – and he goes into what he believed were the reasons. As it is clear that TLVs are reasonably practicable OELs one can understand this reluctance. As long as this reality remained unspoken union or other health professionals could not challenge the process – they did not agree with the 'objective reality' of the Committee, but at the same time could not put a finger on why they felt this way. The TLVs seemed compromised but they felt unable to mount an effective challenge to the professional paradigm the Committee worked to.

Castleman and Ziem's calls for hygienists to be more critical of TLVs and for a more open standard setting process which avoids conflict of interests, are fair comment. Their use of the term "scientific" and their belief that there are now "better trained and equipped groups in ... Europe" are questionable. The latter issue will be dealt with later, but their use of "scientific" must be questioned here. They use the word in a very similar manner to the Committee and its proponents, as if all the problems and challenges in setting OEL's would melt away if only OEL's were "scientific". It does not solve the problems of setting OEL's to cloak them with the word "scientific" using it as a shibboleth. In the light of the understanding gained in this thesis what

Castleman and Ziem probably mean to say by their use of the word scientific is that the TLV Committee has been setting reasonably practicable TLVs and we think it should be setting health-based TLVs but nowhere do they say this, instead they call for more scientific OELs. Also nowhere in their analysis do they show an historical understanding of the ACGIH and TLVs whereas at least Samuels does appreciate what the Committee did in the 1940's.

It is clear from the history of the IH and the development of the TLV Committee that the ACGIH was, and saw itself as "progressive" in the 1930's, 1940's and 1950's. The aim of the IH was to control the processes which caused ill health and predict, using toxicology, these materials that might cause, or might be causing harm. They also understood the problems faced by the rather less than "... Utopian cohabitation of Capital and Labour..." and for Elkins and many others the ACGIH was there to "protect the workers interests", (see Chapter 5). By setting reasonably practicable TLV's the ACGIH, together with the enforcing authorities in the States will have improved conditions in many workplaces. What they could not do then and still cannot do now, is set health-based TLVs which, because they are so low, cannot be enforced.

9.7.1 Fear of femptograms

Interestingly this fear of femptograms which Wagner referred to when commenting on the need for an "element of practicality" in setting TLVs surfaced in a slightly different form in a Japanese commentary on Castleman and Ziem. The author was concerned at what would happen if representatives of labor unions sat on OEL Committees. His fears have a similar ring to Wagner's:

"If I were a worker exposed to any toxic chemicals, I would insist on "zero value" of the toxic chemical ... because as a layman I would not have any scientific background in the toxicology of chemicals and would want to protect my health without any risk whatsoever". TSUCHIYA (1988).

Apart from the fact that the author is guessing at what workers may or may not feel, his defence of the TLVs and his fears are similar to Wagner's. TLVs must be reasonable and they cannot be simply health based, that way lies the precipice of femptograms. The TLV Committee could not and cannot set health based OELs. To do so in the early days would have marginalised the Committee and it would have gone against the whole participating and pragmatic ethos of IH and the PHS. To do so now would mean a break with 40 years of tradition and, what is probably far more important, a break with the OEL paradigm. Finally, the clients of IH's in general and ACGIH in particular wanted OELs they could live with so they wanted reasonably practicable OELs.

Given its history the TLV Committee was bound to set reasonably practical OELs, but for the variety of reasons explored in this part of the thesis, this fact has never been made explicit or understood by users of TLVs. Because TLVs have been portrayed and projected as, in effect, health based OELs this has inhibited the development of true health-based OELs. Perhaps even

more interestingly, they have inhibited the development of true reasonably practicable OELs. Some possible ways forward on both these fronts are addressed in the last Part of this thesis but it is worth listing, at this point, some of the structural changes referred to earlier which will be necessary in order that these two different types of limit can be produced.

(i) A different paradigm is required which supports the production of both health based and reasonably practicable OELs.

(ii) The process of setting health based OELs needs to be separated from the process of deciding upon reasonably practicable OELs.

(iii) The mechanisms by which both types of limit are set need to be discussed and debated and ultimately formalised as much as possible. This would facilitate the standard setting process.

(iv) The mechanism of how substances are selected needs to be reviewed, debated and formalised.

(v) The place of attitudes, beliefs and clients in the thought processes of professional applied scientists needs to be examined. All IHs/ITs working in industry do not believe in threshold for carcinogens and likewise, all IHs/ITs working for government do not believe in the lack of thresholds for carcinogens. The picture is not black and white and our understanding of professional behaviour needs to go deeper into issues of how professional, scientific and organisational culture affects peoples' views.

None of these structural changes will occur unless the "group licensed", "objective" world of the TLV setting process can be disturbed and changed. In the last year the proportion of "consultants" on the Committee has fallen from 39% to 18%. The Committee has taken Castleman and Ziem's 'goody' versus 'baddy' view of the world at face value and adapted to the criticism by reducing the proportion of 'baddies'. Even if they were reduced to 0% and the Committee only used published, peer reviewed research TLVs would still be reasonably practicable OELs. Far more fundamental changes are required.

9.8 The OEL paradigm revisited

For many health professionals worldwide, not simply in the West, Hatch's 1972 model of impairment – disability, dose response and TLV's is still the dominant exemplary model (see Figure 9.5). As was shown in Part 3.0 Hatch's model amounted to an effective defence of the OEL paradigm and it appeared to reconcile the two disparate approaches of the USA and the USSR. It is worth remembering the claims made for the USA standard setting approach and TLVs by Hatch:

"In the US, dose response studies have been continued down from the higher levels of demonstrable ill effects through the use of increasingly sensitive measures of preclinical, physiological, biochemical and other indices of functional disturbances. These new measures have been constantly subjected to critical tests of usefulness, however, in terms of their significance as predictors of ill health and the threshold limit values set to insure that the kind and degree of response which is produced is kept below the limit of such significance", HATCH (1972).

After presenting his model he goes on to identify where he thinks TLVs and USSR MACs are set. Point "B" (see Figure 9.5) represents levels set, "In accordance with criteria commonly employed in the USA...", whereas point "C" represents the approach of the USSR.

He recognises that the US approach requires:

"... a constant search for improvements in meaningful indices of disturbance to sharpen the point on the response axis that marks the upper limit of no health risk and continuing medical surveillance of exposed workers to make sure that the compensatory processes are not over-loaded and to validate the limits of exposure in current use. Such validation over many years is reported in support of this approach." Ibid.

The claim is then that the US approach allows standard setters to set TLVs at levels where, "the compensatory processes are not overloaded", that "increasingly sensitive measures of preclinical, physiological, biochemical and other indices of functional disturbance", are applied and that, "... validation over many years is reported in support of this approach."

Figure 9.5 Taken from HATCH (1972)

It was clear in Part 3 that Hatch's model was idealised. It is now clear that his model was more than idealised, it was idealistic, almost ideological. It was his conception of how he would like US OEL setting to have been. The analysis in this Part of the thesis shows that, in many cases, it was nowhere near this idealistic model. This was so because of a nagging problem that Hatch himself identified in the 1972 paper. Having presented his model he goes on to argue against excessive restrictions on industry:

"... beyond those actually needed to insure adequate health protection since **over-design of controls** can put serious if not impossible restraints on many operations and can add greatly to production costs. These practical considerations are not the direct concern of occupational health professionals but there is a responsibility, certainly, to support the regulations for occupational disease prevention with **hard facts** rather than theoretical criteria", Ibid (Author's emphasis).

The twin demands of "over-design of controls" and the need to operate with "hard facts" have always pushed the criteria defining harm, or ".. below the level of significance" as Hatch describes it, away from the "sub-clinical toxicology" region (see Figure 9.5) into the "medical region". And now that we have examined the TLV setting process in some detail we can see that the need to set reasonably practicable OELs has, on occasions, pushed the process even further into the region of "breakdowns" and "death", (see Figure 9.5).

Trying to reconcile the dose response relationships and dose response curves for the wide variety of toxic effects which can occur is difficult. Some may follow the simple threshold model, others like phosgene may cause cumulative damage, HENSCHLER (1984). For carcinogens some may have virtually no threshold of effect whereas others may have. Teratogens may fall into both these categories and allergens can induce a variety of effects after long or short exposure periods. For sensitized people the dose response relationship changes dramatically and may continue to change with continued exposure. Even some "simple" irritants may in fact induce more than simple irritation, STEENLAND et al (1988). In this context although Hatch's model is elegant and enormously seductive it is also enormously constraining. It does not allow for the large variety of dose response and effect relationships which are known to exist. It forces one into a conceptual straight-jacket. At the same time it has been and is a wonderful model for defenders of the OEL paradigm as a scientific paradigm. It helps shape the 'objective reality' of many people and groups and is still routinely referred to by defenders of TLVs as scientific.

Part 3 was concerned with how key individuals interpreted the OEL paradigm. We saw how practicability occasionally intruded into the idealised description of how TLVs were set. We can now see why such issues intruded. The individuals examined were working to a professional paradigm and they were concerned 'at the limit', as Hatch might have said, to set reasonably practicable OELs. Hatch's model, in particular, serves as a potent justificatory function for people who insist TLVs are "scientific". The realities implied by the model are, according to this thesis, a fiction. Hatch's model will need to be altered, or perhaps will need to be abandoned as too limiting a view, given this new perspective.

Finally, it is worth going back and considering the working definition of the OEL paradigm afresh.

It can now be rewritten as a professional paradigm. The tenets would read as follows:

Working Definition of the OEL Paradigm (Mark II) – a professional paradigm

(1) Some toxic substances can be used safely, but we are rarely sure which ones.

(2) IH control methods are adequate for some toxic substances.

(3) Threshold doses exist for some toxic substances.

(4) Standard setters can rarely identify threshold doses/exposures.

(5) Considerations in setting OELs include:

- (i) the health effects of a substance.
- (ii) the practicability and costs of control
- (iii) the seriousness with which the health effects are judged.
- (iv) the importance of the substance to industry.
- (v) the size of the population exposed.

The list in Tenet 5 will be extended and explained further in Part 5.0 and some of the issues touched on here will be aired and examined in greater length in the last part of this thesis. Before this though the workings of, what Congress might describe as a concensual OEL committee will be examined – the Advisory Committee on Toxic Substances (ACTS). As with the ACGIH TLV Committee it is necessary, to make some sense of why and how ACTS behaves the way it does to have an understanding of the history of occupational health and industrial hygiene in the UK.

CHAPTER TEN

INDUSTRIAL HYGIENE AND OELS IN THE UK

The earlier parts of this thesis showed the strong connection between the development of IH in the USA and the development and promulgation of OELs, particularly TLVs. A similar approach will be taken in examining the use and development of OELs in the UK.

In the USA the PHS was key in the development of IH and OELs and the UK Factory Inspectorate (FI) had a similar effect. This chapter thus starts with a section on the history of the FI and its use of IH as a discipline if not a profession.

10.1 The UK Factory Inspectorate

In Part 2.0 of this thesis a brief introduction to the history of the occupational health in the USA, in particular industrial hygiene was given. Although some interest was shown in individual states in the 19th century, principally those in the industrialised north east, the Federal government showed no great interest until the late 1930's. Although various attempts were made in the 1950's and 60's to promote Federal legislation it was not until 1970 that the wide reaching OSH Act was piloted successfully through Congress. The history of factory welfare legislation in the UK is very different and follows the pattern of other European countries. Table 10.1 lists some legal and organisational milestones on the path to the current Health and Safety at Work Act. Although the legislation can be traced back to 1802, full-time inspectors were not appointed to enforce it until 1833. In that same year the first health provision was included. Section 26 of the 1833 Act required occupiers to lime-wash the walls annually, HALE (1978).

In 1840 Horner, one of the first four factory inspectors observed that "the air, in many mills is in a very offensive state, and recommended that a clause in an amending Act, to compel a better ventilation, and the removal of nuisances which are injurious to health, would certainly be desirable..." DJANG (1942). Nothing was done and the 1844 Factory Act made no mention of ventilation. But a combination of pressure from the growing public health movement fuelled by reports such as Edwin Chadwick's "Report on the Sanitary Condition of the Labouring Population of Great Britain" in 1842 and "... a long series of committee discussions and reports that public health agitation began to gain fruitful results." Ibid. In 1848 the first Public Health Act was passed although factory ventilation had to wait another 16 years for a mention. Djang cites the work of a Dr John Simon, Medical Inspector of London, as being particularly influential. In 1862 he published a study in which he identified the cause of the "terrible shadow of suffering and death" as "want of ventilation". "The coercive hand of the law was needed, he urged, to

exterminate this great removable evil, for, he exhorted, "the canker of industrial diseases gnaws at the very root of our national strength." Ibid.

Table 10.1 Legal and organisational milestones in UK Factory Welfare legislation

1802	Health and Morals of Apprentices Act
1819, 1825, 1831	Factory Acts
1833	Factory Act - Inspectorate
1840	Railway Inspectorate - Public Safety
1844	Factory Act - Safety
1850	Mines Inspectorate - Professionals
1863	Alkali Inspectorate
1864	Factory Act - Ventilation
1867	Factory Act - Scientific Development
1878	Factory Act - Consolidation
1891	Factory Act
1893-99	Dangerous Trades Committee
1901, 1937	Factory Acts - Consolidation
1911	Mines Act
1961	Factory Act - Consolidation
1972	Robens Report
1974	Health and Safety at Work etc. Act
1989	Control of Substances Hazardous to Health Regulations

In 1864 general cleanliness provisions were applied for the first time and a requirement that factories should be ventilated "... in such a manner as to render harmless so far as is practicable gases, dust or other impurities generated in the process of manufacture which are injurious to health." Additional powers were given to inspectors in 1867 to approve means of ventilation.

In 1878 the Factory and Workshop Act "... empowered inspectors to require mechanical ventilation, whenever they saw fit in work rooms in which dust, gas, vapour or other impurities were generated." Ibid. The direct descendants of this provision are Section's 4 and 63 of the still operational 1961 Factories Act. Section 63 is of particular interest and states that where "dust or fume (is) likely to be injurious or offensive (then) all practicable measures shall be taken ... in particular ... exhaust appliances shall be provided ... as near as possible to the point of origin of the dust or fume ... so as to prevent it entering the air of any workroom". p41. GREAT BRITAIN (1961a).

In 1883 the first detailed provisions were made to control a process involving a toxic substance - white lead manufacture. Between 1891 and 1901 special rules were introduced covering a range of trades and processes. In 1896 Sir Arthur Whitelegge was appointed Chief Factory Inspector. Before his appointment he had been Medical Officer of Health in the West Riding of Yorkshire. A year earlier four industrial diseases became notifiable (lead, arsenic, phosphorous and mercury poisoning). Two years after Whitelegge's appointment the first Medical Inspector of Factories was appointed: Thomas Legge, due "...to difficulties encountered in the earthenware and china industries". Ibid. Although as Hale points out the range of responsibilities and knowledge that

Factory Inspectors had to encompass increased dramatically in the late 19th century and this led to a much greater emphasis on scientific and technical competence amongst people recruited to the Inspectorate. Ten years after Legge's appointment, a second medical inspector, E. L. Collis, was appointed. A parallel development was the appointment of E. H. Osborne as Engineering Advisor in 1899. His replacement in 1903 by Sir Hamilton Freer-Smith with the title of Inspector of Dangerous Trades marked, according to Hale, the start of the Inspectorate's Engineering Branch.

Hale summarises this period in terms of the development of the Factory Inspectorate:

"... the health hazards which (they) were charged with preventing were at first general ones. There was an awareness that dirt, effluvia, overcrowding, cold dust and fumes were in general undesirable. It was not until the end of the 19th century that inspectors had become interested in the specific effects of individual substances." HALE (1978).

The early factory legislation and the work and publications of Legge, Collis and other inspectors were very influential. They inspired Alice Hamilton and other members of the progressive movement in the USA. But it did not lead to the professional and applied scientific innovation which was soon to occur in the USA.

10.1.1 Control and measurement

Early concern with workplace environmental conditions focussed on general ventilation, overcrowding, heating and the emissions from gas and coal fires. In 1877 Redgrave decided to require a minimum of 250 cubic feet for each worker (taking into account only the first 12-14' of workspace height). This became incorporated in law in 1895, the volume rose to 400 cubic feet for those doing overtime. According to Dr Whitelegge the new provision "... has been of signal service in dealing with overcrowding, and indirectly with ventilation" DJANG (1942). In 1937 the legal minimum airspace was raised to 400 cubic feet with larger volumes for certain processes.

The Inspectorate published advice on how to improve general ventilation in its 1884 and 1886 reports including sketches, manufacturers name and addresses and prices GREAT BRITAIN (1884 and 1886). One sample calculation included a, "10% discount to persons recommended by factory inspectors." But it was not until the turn of the century that a Departmental committee on the ventilation of workshops was set up. It reported in 1902 and 1907 and considered the basic principles of general and local ventilation and the second report included many case histories of good and bad practice. HALDANE et al (1902 and 1907). Also Freer-Smith published his own guide to good practice which overlapped considerably with the second report of the Committee, FREER-SMITH (1906). In the second report the Committee recommended a "reasonable" standard of air purity as "10 volumes of carbonic acid in 10,000 volumes of air". Whether and where this standard was applied is uncertain although Harvey did refer to the use of regular carbon dioxide measuring equipment, HARVEY (1954a). It was designed principally to address the

problem of carbon dioxide and monoxide given off by gas and coal heating. The addendum to the Factories Act specifying that a reasonable temperature must be maintained "... but the measures so taken must not interfere with the purity of the air..." had only first been included after the Inspectorate lost a series of cases where although the temperature was reasonable it was kept so by "unwholesome" heating systems. DJANG (1942).

In the early 20th century a chemist called Duckering employed in the Engineering branch measured the lead exposure of workers in various trades known to cause lead poisoning and he investigated the mechanisms by which exposure occurred. Legge cites his work in his book "Lead Poisoning and Lead Absorption" in which he and Goadby use Duckering's work to derive a tentative lead-in-air standard, LEGGE and GOADBY (1912). His work was unique and Legge introduced it with a very pertinent observation:

"Administration of certain sections of the Factory and Workshop Act 1901, would be simplified were there a ready means available for determining the extent of contamination of the air" (especially Section 1 concerned with "rendering harmless ... all gases, vapours and dusts", Section 74 concerned with fans to "minimise inhalation" and Section 75 concerned with contamination in eating places). Unfortunately, owing to the difficulty hitherto of accurate collection, only a very few determinations of the actual amount of lead dust and fume present in the atmosphere breathed have been made." p202, Ibid.

Using Duckering's measurements and his records of lead poisoning Legge came to an important conclusion:

"... if the amount of lead present in the air breathed contains less than 5 milligrams per 10 cubic metres of air, cases of encephalopathy and paralysis would never, and cases of colic rarely, occur. And this figure is a quite practical one in any process amenable to locally applied exhaust ventilation. Somewhere about 2 milligrams, or 0.002 grammes of lead we regard as the lowest daily dose which, inhaled as fumes or dust in the air, may, in the course of years, set up chronic plumbism." p207, Ibid.

Duckering's work has a remarkably modern feel to it. He covered all facets of what became professional (non-medical) IH practice, examining the process, relating it to exposure, measuring exposure and quantifying the effectiveness of controls. And Legge, by combining the epidemiology of lead poisoning and symptoms relating to lead absorption with exposure measurement was able to estimate an OEL to prevent "chronic plumbism". If one assumes that an average a person inhales 10 cubic metres of air in an 8 hour shift then his dose of 2 milligrams daily dose becomes an 8 hour (TWA) OEL of 0.2 milligrams/m^3, close to the current UK standard of 0.15 mg/m^3.

Legge and Duckering's work prefigures the US multidiscipline medical/chemical/engineering team work, that was to develop in the US PHS, by a decade and it covers all aspects of what were to become the pattern of fairly regular "field surveys" conducted by the PHS. However, unlike the PHS Legge and Duckering did not actually work together on the same site at the same time. Legge used Duckering's measurements and related them to his own results. This separation of the work

of doctors, chemists and engineers was to become the pattern in the Factory Inspectorate. Also interesting is that although Legge could see the usefulness of regular air measurements and proposed an OEL for lead, no effort was made to develop sampling and analytical methods and routinely apply them in the UK until the 1960's.

Similar one-off field investigations were performed by Macklin and Middleton into grinding and polishing operations in the metal working industries in the early 1920's and by Merewether and Price into asbestos exposure and its effects in the late 1920's, MACKLIN and MIDDLETON (1923) and MEREWETHER and PRICE (1930). In the first case no attempt was made to relate exposure to effect and Macklin could only compare different methods of control. This is in stark contrast to what was done in the USA in the mid to late 1920's. Bloomfield had surveyed dust control in the granite cutting industry. However, in addition to being able to identify the better methods of control he was able to identify processes within the industry for which no effective method of control existed. He did this by using a tentative permissible level of dustiness derived from a field study involving himself and other members of the PHS in a team, RUSSELL et al (1929). Having identified problem processes research was commissioned by the industry from Hatch at Harvard School of Public Health and various possible solutions developed, HATCH et al (1930). Nothing like this happened in the UK and Macklin could only give general advice on what control methods appeared to work best. In contrast the later survey of asbestosis and dust suppression did eventually suggest a "dust datum". Merewether in his analysis of the occurrence of asbestosis by duration of employment dust exposure and type of job showed that "Group 3" type job carried less risk because they were less dusty. This was not to say such jobs were without risks as Table 10.2 shows. Merewether argued, "... that in order to prevent the full development of the disease amongst asbestos workers within the space of an average lifetime, it is necessary to reduce the concentration of dust in air ... to a figure **below** that pertaining to spinning..." MEREWETHER and PRICE (1930) (Authors emphasis).

Table 10.2
Incidence rates of Fibrosis (asbestosis) - based on Merewether and Price (1930).

YEARS EMPLOYED	*GROUP 3** *% incidence rate*	*GROUP 1,2,4*** *% incidence rate*
0-4	0	0
5-9	5	34
10-14	7	48
15-19	36	57
20+	67	85

* Spinning,. twisting, doubling, plaiting
** Crushing etc. carding, mattress making, weaving and associated processes

The Committee that considered ways of improving the suppression of dust in the asbestos textile industry quote Merewether's conclusion but then go on to make the following statement:

"For practical purposes, the conditions arising from flyer spinning ... may it seems to the Committee, be taken as the "dust datum"... If, therefore, a particular process appears to give rise to dust in excess of that associated with such flyer spinning, the Committee regard the need for preventative measures as established." GREAT BRITAIN (1931a).

The Committee believed that conditions could be improved to below the "dust datum" by the application of exhaust ventilation and were at pains to emphasise that their recommendations were, "... aimed at interfering as little as possible with existing work methods or lay out of premises." Ibid.

Merewether had clearly stated that elimination of risk would require lowering the exposure to below the level of flyer spinning, the "dust datum" level. The Committee understood this as they quoted the relevant part of his report. In the next sentence they behave as if Merewether had never spoken. Somehow the Committee decided that the risk was acceptable, and it certainly did not want to interfere with the production process unduly. Also, it knew that even applying the flyer spinning "dust datum" would require considerable exhaust ventilation to be installed. It clearly did not feel able to agree upon more stringent measures. What had the Committee done? The answer is that it had set the first "reasonably practicable" British OEL. In a sense the Committee had done what Winslow and his National Safety Council (NSC) benzene Committee had done a few years earlier. Though in his case he had not actually recommended an OEL he had simply stated that it was possible to get exposure at all processes using benzene to below 100ppm. This became transmuted into a "permissible level" of 100ppm.

The big difference between the "dust datum" and the Winslow/NSC benzene OEL was that the latter was applied and the former was not. Part of the reason was that there was concern about accuracy of the original flyer spinning dust measurement and the wording of the 1931 Asbestos Regulations requiring the elimination of dust did not help. But the primary reason was because there was only one person in the Factory Inspectorate in the early 1930's who could do the measurements - K. Goodall. The Factory Inspectorate had no intention of applying the "dust datum" on a widescale to determine compliance with the 1931 Asbestos Regulations. And indeed four years after the Regulations were passed the Home Office (in which the Factory Inspectorate (FI) were based) published a Memorandum which specifically down played the possibility that OELs could be set:

"It has not yet been possible to determine for all industries the limit within which a concentration of dust may be considered safe. Attempts have been made... Such standards may be of value in estimating the relative efficiency of the methods used for suppressing dust in a single industry, but it would be wrong to adopt any of these standards as the measure of safety under different conditions in other industries." p5, GREAT BRITAIN (1935).

It was this conclusion that Hatch and Drinker quoted so approvingly in the first edition of their seminal book before the OEL paradigm had crystalised in the minds of the IH's and IT's in the late 1930's in the USA.

The memorandum continues and mentions some of the problems that need to be overcome before OEL's can be set. The sub-section ends: "Much progress has been made in these directions as a result of co-ordinated research, and more is hoped for from work at present being carried on." Ibid.

Most of this work was "being carried on" in the USA and although some excellent work was to be done on coal, asbestos and cotton dusts in the UK very little was "carried on" by the FI.

10.1.2 Kenneth Goodall

Goodall was first employed by the FI in the early 1930's as an Engineering Inspector. He was a physicist and concentrated in the main, but not solely on dust measurement. Little survives of his reports except a selection taken from his work during the War years, a paper he published and a long memorandum on his suggestions for expansion and re-organisation of industrial health research.

His War year's reports are interesting and cover a variety of processes and problems:

1938	Attempt to define the asbestos "dust datum" more exactly.
1940/1941a,b&d	Detailed advice on exhaust ventilation design and maintenance.
1941a	Detailed advice on work methods and tool re-design to reduce skin contact and absorption of TNT.
1941c	Advice on process alteration to reduce lead exposure and absorption.

He was clearly a diligent well informed and imaginative person. He used the full range of industrial hygiene assessment and control techniques and in the USA would have been regarded as a hygienist. Revealingly although he practiced IH he always regarded himself as first and foremost a physicist. In his 1942 paper he clearly defers to US hygienists when it came to control and describes current practice: limited surveys of typical processes which result in guidance on, "... general application of protective measures to generally similar conditions in a whole industry." GOODALL (1942). At no point does he suggest that air sampling should be use done a wide scale to assess conditions or control methods. Also, he never mentions MAC's or other OEL's, dust sampling is done simply to assess the relative effectiveness of control methods. Goodall was isolated within the FI, there was no organised IH professions to pull together applied scientists from different disciplines into a new profession. Thus Goodall's first loyalty was to physics and

not to IH. The contradictions and debilitating effect this isolation had is thrown into greatest relief in his long memorandum on reorganisation. He starts with some preliminary points:

"1. The need for a reorganised and expanded body for the investigation and righting of conditions in industry, harmful to health, is urgent and claimant.

2. This organisation must be based on team work since the medical, chemical and physical aspects are indissolubly interlocked.... ram home that there are only 1 physicist and 2.5 chemists.

3. If our department does not move in the matter other outside bodies will..

4. Our own staff in the medical, chemical and physical branches are the obvious nucleus for the new organisation because:

(a) We have 100 years experience
(b) We have trained staffs....
(c) We ... have a wide knowledge of industrial conditions...
(d) We are an impartial body, respected by employers and employed alike ..." GOODALL (1943)

Items 2 and 4 more or less describe the US PHS and it is the US with which he unfavourably compares the UK:

"The present organisation of industrial health research in Great Britain is disgraceful and humiliating for a country which led the world in industrial health legislation. .... One has only to study the international health literature, as I do to see the lamentable lack of papers on research or field work in industrial health published by are own department or for that matter by other bodies in Great Britain. Every state in the USA, apart from the Federal Government in Washington, maintains its own public health organisation, and from these bodies flows a spate of publications, booklets, books and data on industrial health. Whatever may be thought of the value of individual papers... the state of affairs there is infinitely to be preferred to our own state of stagnation. One has only to think of what available information there is today on dust measurements of various kinds or on toxicities of various vapours, or an explosive limit, to realise at once that most of the information our department possesses comes from American sources." Ibid.

He identifies the causes for the present state of affairs as due to:

"(a) chronic understaffing of the technical branches.

(b) lack of research and laboratory facilities." Ibid.

And he continues with a plan which he believes will rectify the situation. Finally, he suggests that:

"We should carry out this field work (routine tests and advice) ourselves, leaving fundamental or long-term research to those outside bodies best qualified..." Ibid.

Then he lists "problems awaiting solution" including dust sampling, "fixing permissible dustiness standards" methods of testing and devising better ventilation systems, anticipation and control of dust explosion and other specific problems including noise - a problem "... which our department has given practically no attention at all."

His description of the facilities required has a very modern ring to it:

"... We shall need a central laboratory (it).. would best form part of a larger laboratory to be shared with the chemists and the medicals... as experience is gained, it may be necessary to set up a number of regional laboratories". Ibid.

This last quote is an almost exact description of the present Cricklewood laboratories and the area Field Consultancy Group (FCG).

He ends the main text on a visionary note:

"Realisation of the above scheme ... would bring nearer the day when no factory process dangerous to health would be started without previous notification to an organisation, so that every occupier could have the benefit of prior consultation with our expert staff, who would warn them of the possible dangers (and) suggest the best and safest methods of carrying out the processes..." Ibid.

Goodall clearly regarded the Departments role as central. He believed in state supervision of industry in quite an intimate way. He was, like many if not all in the Department at the time, not a 'market forces man'.

In the Appendix he reinforces some of his textual points and "... press(es) for a joint survey of industrial dust conditions by medical men and physicists and chemists, to obtain a complete picture which will relate dust conditions to medical disabilities. .... This type of work would ultimately lead to fixing dustiness standards for the various branches of industry." Ibid.

What can be concluded from the tone and content of Goodall's proposals?

He was clearly dismayed at the lack of interest and resources devoted to occupational health problems by the FI.

His scheme of action was apposite and in places visionary, and he understood the need for multi-disciplinary team work though he had rarely experienced it himself.

He saw the need for "permissible dustiness levels" and could see what needed to be done to develop them.

However, he undermined many of his good points by insisting on the central role of physicists. There was to be one in each region, one for x-ray work, one for ventilation and one for "less specialised problems" and Goodall was to be in charge of them all.

Almost none of Goodall's points were taken up at the time. The major reason being the FI's lack of interest but Goodall probably made it easy for his proposals to be dismissed as special pleading by a physicist who was obsessed with his own disciplines importance and who wanted to build an empire in the Department.

Finally, it is instructive to compare Goodall's visionary conclusions with Patty's written about the same time.

Patty was writing at a time when industrial toxicology had been developed in several universities, large companies, such as Dow, Dupont, Union Carbide and in the US PHS. Two organisations had been set up to develop and promulgate IH and the PHS had been doing multidisciplinary team research for 20 years. During the Second World War IH had expanded and proved its metal in Federal and local state government organisations.

Goodall, in comparison, was a dyed in the wool physicist who practiced IH, could see the need for a multidisciplinary approach, understood many of the problems which had to be tackled but did not see the way forward as a new profession with a new mission. He worked at a time in the UK when industrial toxicology hardly existed (the MRC unit was not founded until 1947) and people like himself worked by themselves maintaining their allegiance to their initial disciplines. He was isolated and ignored.

There was some movement in the FI through. A year after Goodall wrote his memorandum the Chief Inspector wrote to the C. W. Price the Chief Engineering Inspector and sanctioned the setting up of Chemical Laboratory which had been approved by Sub-Committee A of the Industrial Health Advisory Committee. Price was allowed £120 to set up the laboratory and £60 per annum to run it. In the letter there is no mention of additional staff over and above the $2\frac{1}{2}$ chemists Goodall mentioned.

Before continuing with the development of IH in the FI it is worth examining briefly the background and interests of the FI over the period in which Goodall and Harvey, who was very influential in the development of IH in the UK, both worked.

Hale surveyed the activity of the Inspectorate by noting how many prosecutions were taken under the various Acts and Regulations from 1840 to 1980. He divided prosecutions into six categories and table 10.3 is based on his analysis, HALE (1983). The author points out that prosecutions also reflect the ease with which proof can be obtained and that, "This is likely to underestimate the importance of the health area", Ibid. Nevertheless "... The picture created is of three periods which differed fairly sharply; in the first, from 1833 to 1890 the Inspectorate was predominantly an employment inspectorate with important, but minor involvement with health and safety and a waning interest in education the second from 1890 to the Second World War saw safety and employment change places in the priority; finally, after the War, concern with employment more or less faded out and health came much more to the fore". Ibid.

Table 10.3
Division of Inspectorate Prosecutions by type, 1840–1980, based on Table 6, HALE (1983)

PROSECUTIONS IN EVERY TENTH YEAR

Year	*Safety*	*Health & Welfare*	*Periods of Employment*	*Education*	*Payment*	*Admin. Provs.*
	%	%	%	%	%	%
1840	0	2	60	26	0	12
1850	4	1	73	8	0	14
1860	1	.3	77	5	0	9
1870	2	.4	83	6	0	6
1880	4	0	67	22	0	8
1890	1	.4	89	3	0	6
1900	5	5	73	.3	1	16
1910	8	4	74	.2	4	10
1920	16	5	61	0	0	16
1930	27	7	56	0	1	9
1939	38	1	54	0	1	5
1950	74	2	18	0	0	5
1960	65	16	6	0	1	12
1970	75	10	5	0	0	10
1980	75	5	1	6	0	13

Table 10.3 does give a good feel how the interests of the Inspectorate have changed since its inception. Three observations can be made, based on the data, in the present context. Firstly from the turn of the century until the 1950's the percentage of prosecutions for health and welfare infringements was always less than 10% and was on average 4%. Secondly, although the position improved in the 1950's and 1960's the percentage fell away again in the 1970's. And thirdly, as with health and welfare the percentage of safety related prosecutions increased from 1950 onwards, but unlike the former did not fall away in the 1970's but maintained the maximum figure of the decade before - 75% of all prosecutions.

Goodall worked through an era in which the FI was changing its emphasis from periods of employment to safety, where health was a low priority. It was this low priority which Goodall was reacting against in his memorandum. Harvey too worked during a period of change when the emphasis switched more to safety and somewhat more to health. Indeed some part of the switch of emphasis was due to Harvey's actions.

10.1.3 Bryan Harvey

Bryan Harvey joined the FI in 1938. After the War he was stationed in Manchester and from 1951 to 1959 he was the District Inspector in Oldham. After that he was rapidly promoted becoming Deputy Chief Inspector in charge of health with the Chief Medical Inspectorate. In 1972 he became Chief Inspector of Factories.

In the early 1950's Harvey, along with a small number of other people obtained Rockefeller Fellowships to study occupational health in the USA. Harvey chose to do the Masters in IH at Harvard School of Public Health in the 1952–53 academic year. He wrote up his impressions of health and safety in the USA as a report for the Chief Inspector and as a paper in the British Journal of Industrial Medicine HARVEY (1954a and 1954b).

His views on how far ahead the USA was were similar to Goodall's except that he had had direct experience during his attachment to Liberty Mutual Insurance Company in Boston.

Harvard "... was an enormous eye opener ... the Americans had all the knowledge and none of the means of enforcing it." HARVEY (1986). On the question of research "they were doing I think outstanding work there when I was over in the 50's. We could not have matched it in anyway at all in this country. Nobody was doing that work." Ibid.

Harvey in his report to the Chief Inspector has to spell out what IH was and dispel a common misconception in the UK:

"Industrial hygiene is not synonymous with industrial medicine and in this field represents a truly American approach to the problem of industrial disease." The Industrial Hygienist is part chemist and part engineer ... (and) carries some of the functions of the Industrial Toxicologist and also some of the functions of the ventilation engineer." HARVEY (1954a).

He continues a little later in the report to put the case for OEL's and attempts to persuade the Chief Inspector that air sampling might be a good idea:

"In this country we have not, so far, overtly accepted the principle of allowable concentrations, yet the justification put forward for it in America is not easily refuted, since they argue that if the air is to be cleaned by mechanical means ... the designer of the equipment must have a design limit to work to. This is a virtual rejection by the ventilation engineer of the possibility of 100% purity. If this is at first sight an anathema to the English way of life, it is in practice little more than a plain statement of fact".

Harvey views the US scene with British eyes:

"Some at least of the techniques of air analysis has been forced upon workers in this field since the development of industrial health – in the US has not yet produced anything comparable to Section 47 which would allow the word "offensive" to be an acceptable cause for the provision of exhaust ventilation. In almost every case where toxic risks are encountered investigations have to show... that the toxic agent is present in the atmosphere..." Ibid.

This insistence on measurement leads on occasions to some strange sights:

"... the refusal of industry to do anything by the way of removal until air analyses have been made, sometimes results in the odd spectacle of an industrial hygienist solemnly taking air samples in a room in which it is not possible to see across owing to the density of the dust cloud." Ibid.

Nevertheless Harvey is in favour of the Inspectorate doing more measurement. He points out that:

"... air sampling in this department, chiefly for carbon dioxide, was at one time widely practiced, many years before it became popular in America. There seems to be some case for reviewing this technique. ..... Accurate assessments of air contaminants, particularly where the presence of low concentrations of some of the newer industrial poisons can be demonstrated, would notably assist us in controlling health hazards. In this respect the experience with metallic poisons in the US, which has not so far been matched in this country, is a good example of the advantages of developing accurate air sampling techniques." Ibid.

He surveys and compares various aspects and differences in the UK and US approaches and is particularly impressed by US industrial toxicology and IH engineering. In particular he was impressed by the ACGIH's "Industrial Ventilation Manual".

"This handbook which is published for one dollar is one of the most useful publications I have ever seen and would be of invaluable use to inspectors in this country who are called upon to assess the technical problems in exhaust ventilation. .... One cannot but feel that the average ventilating contractor in the US is more competent than his counterpart in this country, and is far better versed in the theoretical considerations." Ibid.

He goes on to make a series of suggestions including:

(i) Sending a limited number of recruits to America for instruction in IH.

(ii) "... making use of our own universities for the training of a certain number of probationary inspectors..."

(iii) "... widening the scope of our own training to include work in statistics, in the theory of ventilation, and in practical work in air sampling technique."

(iv) He suggests that, "air sampling might be reviewed in the department...."

(v) "The possibility of obtaining copies of the Ventilation Book published by the Conference of Governmental Industrial Hygienists should be explored.". He also recommended distribution of Liberty Mutuals IH handbook. Or, alternatively, a similar book produced by the Canadian Government.

Harvey's views are pertinent and the reaction of senior individuals in the FI is very revealing.

Soon after returning to the UK and making his report a senior FI inspector remarked, "We'll have to knock all these silly American ideas out of your head won't we" HARVEY (1986).

And after requesting some smoke and carbon monoxide tubes his Superintending Inspector received a letter from the Chief Inspector which ended along the following lines:

"... Will you please instruct Mr Harvey that he should not interest himself in matters that are not his concern and that he should get on with the work for which he is paid". Ibid.

When asked why the Chief Inspector (Norman H. Jones) reacted this way Harvey replied:

"We were rubbing up against the feeling that this was not the business of the Factory Inspectorate. It might be the business of the Medical Inspectorate." Ibid.

10.1.3.4 IH develops in the Inspectorate

Despite the opposition, as Harvey's career progressed he was able to institute change in the FI. By the time he became responsible for the chemical inspectorate in the early 1960's their number had grown to 10. Stuart Luxon, who was to become the first Director of HSE's Cricklewood Laboratory gave an impression of how the Chemical Inspectorate (CI) has organised and what it involved at this time, LUXON (1986). He had been a CI since 1957, having joined the FI in 1949. In 1957 there were 8 CI's about half based in London and the other half in the regions. While the FI did publish "Red Book" method of air sampling and analysis though the late 1950's and 1960's, very little measurement was done and a large number of inspections were performed where the CI relied upon a subjective assessment, "A great deal was achieved by an ad-hoc basis without quantification." LUXON (1986). CI's were inspecting upwards of 1,000 workplaces a year and they "got a feel for it", what controls were sensible and economically possible. Sampling was done, but it was cumbersome and slow and most of the analyses were done by the Government Chemist. This arrangement was slow, and, according to Luxon sometimes the analysis did not square with experience. Also it made the chain of proof difficult to sustain in courts. By the mid 1960's it was clear to Harvey and CI's like Luxon that the FI needed its own laboratory facilities. Harvey was able to make use of a section in the 1960 Factories Act which enabled the Secretary of State to make special provisions. A small laboratory was started at Baynards House in 1967. It was expanded by unorthodox means a year or two later but by the early 1970's it was clear that a larger laboratory was required. Also, the Public Service Agency (PSA) in charge of the building, were concerned that strictly speaking it should not have contained a laboratory. A new premises at Cricklewood in North London was found by the PSA and it opened in late 1971 with a staff of 15 scientists and 15 CI's. Also at this time FI's were issued, on Harvey's instruction, with what were known as "Murder Bags". These consisted of a basic set of equipment including chemical indicator and smoke tubes and an anemometer. In Harvey's view this basic equipment "... greatly improved the impact of the Inspectorate..." With the advent of the Health and Safety at Work Act (HSW Act) in 1974 the Health and Safety Executive (HSE) was created out of an amalgamation of various inspectorates. One of the organisations brought into the fold was the Safety in Mines Research Establishment (SMRE). This, and the Cricklewood

laboratories, became the basis of the Research and Laboratory Sciences Division (RLSD). About the same time Field Consultancy Group (FCG's) were also set up in seven HSE areas, to do the routine analyses and to service most of the needs of the Specialist Inspectorates. Over 30 years after Goodall had written his perceptive memorandum the central and field laboratories he so earnestly wished for had been created. But, significantly, the research by multidisciplinary teams he advocated was not started. Although the picture has changed in recent years HSE still tends to concentrate what research it does in areas dominated by one profession. Indeed, until recently there were many within the organisation who would argue that it was not the place of HSE to do research unless it were directly related to some aspect of law enforcement. The large majority of the HSE research budget that goes on multidisciplinary research is dispersed to outside organisations.

Before turning to the development of IH in the UK in general it is worth describing what CI's found when they started doing measurements:

"In quite a number of instances we were surprised at the variation between the concentrations we actually found and what we would have thought was there. In many cases we were on the ball but in quite a number of cases we were either too high or too low. (Measurement) was quite revealing". LUXON (1986).

10.2 IH in the UK

From the developments described in section 10.1 it is clear that various individuals, mainly in the FI, have applied IH principles to recognise, evaluate and control health hazards at work since the turn of the century. But the profession and the multidisciplinary person with allegiance to the profession did not arrive on the scene until the mid-1950's. Outside of the FI little systematic research on industrial disease was done.

The Industrial Health Research Board (IHRB) was set up in 1918, and Schilling described its development in a review article, SCHILLING (1944). It developed out of the earlier work of the Health of Munition Workers Committee set up in 1915 to examine the effect of fatigue and hours of work on productivity. After the Committee was dissolved in 1917, the Medical Research Committee (later Council) and the Department of Scientific and Industrial Research set up the Industrial Fatigue Research Board with a similar remit, but applied to industry in general. The word "Fatigue" was replaced by "Health" in 1928. The Board was mainly concerned with hours of work, sickness, accident proneness, time and motion study and ventilation. In the main it did not concern itself with industrial disease although it did promote Brownings review of solvent toxicity and set up a Committee on Industrial Pulmonary Disease concerned with cotton and coal dust. Although the IHRB, did little on industrial disease comparable with the US PHS what work it did do was of good quality and various individuals who worked for it, such as Bedford, Warner and Schilling, became part of the back-bone of the British Occupational Hygiene Society (BOHS).

Apart from the IHRB individual research was conducted in some university departments, for instance, ANGUS and STEWART (1937). Interestingly, the authors of this piece of work into dust control in a ceramic plant cite Hatch's tentative permissible silica dust level and end with a plea; "It is felt strongly that only by such co-operative effort (between doctors and engineers) will preventative medicine in relation to occupation be of the greatest practical benefit..." Ibid. They published their research in the US Journal of IH and at the time, as with Goodall a few years later they were lone voices crying in the UK wilderness. Interestingly, Angus went on to become an IH, working in the Slough Industrial Medical service with some of the founder members of the BOHS.

Another lone voice whose views may have been influential is that of Merewether. In the War years in the early 1940's he spoke at the American Public Health Association (APHA) in 1942 and 1943 singing the praises of the 'British Way' and the organisation and effectiveness of the UK FI, MEREWETHER (1942 and 1943). However his national chauvinism did not prevent him from imbibing at least some of the 'American Way' though in a rather vague fashion. In 1945 he addressed the Institution of Chemical Engineers (IChemE). Apart from trying to convince them that "... (the) health of industry is synonymous with industrial health..." MEREWETHER (1945). He made a plea for "... the formation, if possible on an international scale, of a Society of Industrial Health and Safety, the establishment of a Group by one or more of the chemical societies, the holding of annual conferences and the publication and distribution of papers and literature..." Ibid. Three years later Merewether went on to describe the "British Tradition in Industrial Health", MEREWETHER (1948). He emphasises how, "Personal medicine is wholly wrapped up in the individual. Industrial medicine is predominantly wrapped up in the occupation and occupational environments". Ibid. To illustrate the difference he examined the correct approach to industrial health problems. While it was clear in his mind that, "... the problem is inherently medical...." he did argue that "... the chemist or the physicist, the management or the workers, the psychologist or some other experts have vital roles to play..." and he ended by stressing the need for ".... highly specialised team work". Ibid.

Merewether was very much a mainstream Medical Inspector, the role of the doctor was pivotal. However, he saw in the USA that team work in industrial health did bring results and there was a need for a "Society" like perhaps the US APHA to act as a focus for non-medical professionals. In his description of problem solving and team work in 1948 he was describing what he thought should be happening and not what was happening on a wide scale in the FI. His views are of relevance here because they give a flavour of the attitude of some Medical Inspectors in the 1940's. There were others though who took a far more traditional view, thus McLaughlin another Medical Inspector could end a review of the prevention of dust diseases; "Occupational or industrial medicine is simply clinical medicine applied to industry and there is no good reason why doctors

should not study their patients both in the hospital wards and in their working environment." McLaughlin (1953).

Other Medical Inspectors took a similar view. The focus was on the individual and prevention was based on medical examination:

"As a means of safeguarding the health of workmen exposed to dangerous processes, the statutory system of initial and periodic medical examinations by the Pneumoconioses Medical Panels is in operation." MEIKLEJOHN (1956).

The early exponents of IH in the UK in the early 1950's had to contend with this crippled and narrow vision of prevention. And it probably goes some way towards explaining the resistance and hostility Harvey met when he started expounding his "American ideas".

Merewether address to the IChemE is seen by some as an important stimulus to the eventual formation of the BOHS in 1953. This is clearly not the case. Merewether's proposal is vague, directed at health and safety and nowhere does it mention IH. And this view is reinforced three years later when he itemises the team which would assist the doctor; the traditional professions are listed. He was not thinking of or suggesting that a UK version of the US IH profession should be created. If Merewether was not a founding father of IH in the UK – who was? Goodall was using many IH techniques and could see the need for greater professional co-operation and teamwork but even he could not break free of the discipline bound straightjacket of physics. If the founding fathers of IH in the UK can be identified then Goodall and probably his few colleagues contributed but the leaders in the formation of a Society to promote and promulgate IH came from the IHRB and that small band who had been to the USA for IH training and had 'seen the light'. Bedford and Warner were not doctors but scientists who had worked for the IHRB in teams with doctors, psychologists and others on a variety of problems including thermal stress, dust measurement and pneumoconiosis. They joined forces with the 'Rockefeller Fellows', and in 1953 the BOHS held its inaugural meeting and Thomas Bedford became the first President of the Society. Harvey described the circumstances and feeling of those days as follows: "We became the core of the BOHS with support from Bedford and Warner. I think we saw ourselves as missionaries from another land." HARVEY (1986). These "missionaries" were few in number but many if not all became very influential in the development of IH in the UK. They included Bryan Harvey, David Hickish, Ray Higgins and Jerry Sherwood. Two became involved in an early industrial health service which offered IH consultancy and a feel for this work can be gained from early descriptions, see SHERWOOD (1953a and b and 1955) and NASH et al (1953). The debt owed to US IH is clear in these writings particularly SHERWOOD (1953a) who continually refers to the work of Dallavalle, Silverman and Elkins and reproduces the 1953 ACGIH TLV list.

Bedford's *inaugural* speech shows up the differences and similarities between US IH and UK IH. He traces the development of IH in the UK back to Thomas Legge and his definition of the aims of the Society owe a lot to his work in the IHRB:

"Occupational hygiene is not only concerned with the prevention of recognisable diseases, whether of specific occupational origin or otherwise. It's business is the maintenance of full bodily efficiency, well-being and safety." BEDFORD (1954).

He claims most of occupational psychology and a fair chunk of safety for occupational hygiene. And he goes on to describe teamwork, and this time unlike Goodall or Merewether he does not blow the trumpet for his particular discipline or profession. He draws on the views of his "... friend Professor Hatch...." whose Cummings Memorial Lecture he had attended two years earlier:

"Occupational Hygiene requires the employment of many skills, embracing those of the various branches of medical science, the physical sciences, and engineering..... as my friend Professor Hatch put it... one can think of (it) as a joint field of specialisation. The activities of the various specialist professions... are characterised by interdependence, mutual support, and team work. And, Professor Hatch added, in importance these transcend the separate skills and special techniques, valuable as they are.

In that admirable lecture Professor Hatch gave a brilliant exposition of the concept of teamwork in industrial hygiene, and I commend it for your earnest consideration." Ibid.

This quotation is interesting not only because of Bedford's praise and respect for Hatch but also because of what it says implicitly about "teamwork" in the UK. Bedford had had experience of working in teams in the IHRB but it was not until he went to the USA and met a man who really understood the meaning of the term, where individual professional or disciplinary affiliations were transcended, that he became a convert. There was something unique about US hygienists and their ability to work in mutually respecting and supporting teams which inspired the proto-hygienists from the UK who were touched and became converted. It is also clear that Hatch had learnt to inspire in the same fashion as his mentor, Professor Craig had done three decades earlier. Having said this Bedford and other early UK IH's did have a moral and progressive vision of the IH mission. He describes the position of many workers as he saw it:

"These can be little doubt that from the material standpoint the general standard of living has been raised since the Industrial Revolution but has that brought more happiness? Too often in this mechanical age men find themselves mere cogs in a massive and unfeeling machine." Ibid.

He continues a little later on the question of costs:

"I have said earlier that industrial diseases are preventable and should be prevented. They should be prevented even though the necessary measures may be costly. The worker should not be exposed to unnecessary risk in order that the product of his labours may be sold more cheaply. Life is not cheap." Ibid.

He went on to argue that managers should "... treat the worker as a person and not a production unit.... " and this would bring contentment and increase production. And as for scientists:

"... (they) should not offer industry new processes or materials, nor should industry accept them, without first ensuring that they are unlikely to cause injury to health." Ibid.

Bedford struck a strong moral tone and his views that new hazards were predictable and therefore were preventable are very similar to Patty's written some seven years earlier. However Bedford has none of Patty's or Hatch's optimism and looking at the state of hygiene and industrial health in the UK at the time one can understand why. When the ACGIH and AIHA had been set up some 15 years earlier there was several hundred hygienists practicing in the private and public sector. The teamwork that Hatch spoke of had been practiced in the PHS for two decades. Industrial toxicology, another new profession, had grown up linked to and in parallel with IH. Perhaps most importantly US IH's had confidence in their new professions ability to carve a niche and survive and a vision of where they were going and what made them unique. In the UK when the BOHS was set up there were very few practicing hygienists. The teamwork that Bedford praised had rarely been practiced. The FI did not see the need for IH and assiduously divided the field into what could be regarded as its component parts. And what perhaps most of all was lacking was a coherent collective vision of where the new and nascent hygiene profession might go. Nevertheless IH (or occupational hygiene (OH) as it became known in the UK) did develop and expand slowly. There were set backs, for instance the closure of the Slough Industrial Health Service, but the BOHS expanded. In 1958 it started publishing the Annals of Occupational Hygiene (AOH) and three years earlier it held the first of its international quinquennial "Inhaled Particles" conferences. Both the AOH and the Conference have grown in international standing and the Societies annual conference attracts a significant international audience. However the emphasis in the Society has always been on the word "learned". It never did and still does not see its aim as the support of the development of the profession of OH, more the development of the subject of OH. Development of the profession was slow and it was not until 1967 that the British Examining Board in Occupational Hygiene (BERBOH) was formed as a body set up to create OH qualifications. And in about 1962 the London School of Hygiene and Tropical Medicine started a full-time postgraduate course in OH. The BERBOH syllabus and examinations were clearly modelled on the American Board of IH set up in the late 1950's. But it was not until 1975 that the Institute of Occupational Hygiene (IOH) was founded to specifically support the professional needs of hygienists. In that year Atherley and Hale reviewed the status of the safety and hygiene professions and concluded that:

(i) They had not gained state, union or employer recognition.

(ii) The subject area was not coherent.

(iii) There was considerable rivalry between the newer professional groups and between them and the other professions.

(iv) They were small in number and isolated.
ATHERLEY and HALE (1975).

Ten years later the picture had changed for the better, "....notably in the recognition by influential bodies of the need for expertise and in the coherence of the knowledge base which underpins it." HALE et al (1986).

However, the UK OH still did not compare well with US IH. Even on a generous estimate of the number of hygienists in the UK the ratio of hygienists to workforce was 1:10,000 in the USA compared to 1:30,000 in the UK. In terms of numbers of hygienists the USA leads the UK by at least 3 to 1 and it must be remembered that even within the USA the hygiene profession by no means feels secure. And the UK hygienists did not seem to be aware of any problem. Whereas OSHA estimated the needs of US industry as 750 new hygienists per annum by 1985, UK hygienists considered that their numbers were more or less adequate.

Hale and co-workers summarise the UK scene as follows:

"The inescapable conclusion ... is that occupational hygienists as a separate professional body with an important say in the field are in danger of being swamped. Their numbers may be too small to provide an effective voice and they do not speak for the majority of those who practice hygiene in one form or another." Ibid.

10.2.1 Discussion – IH in the UK

Some of the factors which go towards explaining why IH did not develop in the UK before it did in the USA have already been touched on. Of central importance is the development of IH in the FI or one should say lack of development in the FI.

One productive way of examining IH development in the UK is to compare it with the development of IH in the USA. In Part 2.0 of this thesis a range of factors which lead to the successful development of non-medical IH in the USA were identified. Many of these factors were either not present in the UK or did not have as much or the same type of influence.

Ten separate factors were identified as influential in the development of IH in the USA:

(i) The structure of the US PHS

(ii) The high status of the engineering profession including sanitary engineering.

(iii) The low status of industrial medicine.

(iv) The autonomy and enforcement role of local state Boards of Health and Bureaus of Labor.

(v) The strong pro-market anti-Federal Government orientation of US business with its emphasis on self-regulation. Coupled with the lack of any centralised Federal state legislation and enforcement organisation.

(vi) The influence of scientific management on the importance of quantification, work organisation and environment planning.

(vii) The openness of some occupational physicians.

(viii) The innovations of certain US universities prepared to experiment with small multidisciplinary groups.

(ix) The concurrent and supportive development of industrial toxicology.

(x) The important preventative role of major insurance companies.

These factors combined to support and sustain the small beginnings of non-medical IH in the USA. The position in the UK was very different.

The UK was governed by a strong unitary state and the FI was part of the state apparatus. The FI had existed together with an ever increasing amount of detailed factory legislation which it drafted for over one hundred years. The existence of the FI inhibited not only the development of IH in private industry but also restricted the role of insurance companies. Few, if any took on the preventative and policing role of the large companies such as Liberty Mutual that Harvey was attached to briefly during his stay in the USA. Indeed, in his report to Chief Inspector he pointed out how similar in structure and function were such insurance companies to the FI.

Within the FI, unlike the PHS, there was little room for professional innovation. Engineers did engineering, chemists did chemistry, physicists did physics and doctors did health. In particular "the need to maintain medical supremacy" HARVEY (1986) inhibited the movement of other professions such as engineering or chemistry towards IH. Even when IH started to develop in the FI there was medical resistance, "I think they (the doctors) would like to have seen IH as part of the medical set up." Ibid. An almost exact parallel with the approach made to Smyth and others in the late 1930's to become fellows of the APHA and not to set up the separate, non-medical AIHA.

One must also not forget the general antipathy that professional UK civil servants had towards professionals or specialists of any hue. They were tolerated and marginalised certainly in the 1930's and 1940's and the "tradition" continues in a diluted form through to the present day. Certainly the higher reaches of the civil service in the 1930's, 40's and 50's had little interest in the professions, apart from law and medicine which were too powerful to ignore, and no interest in fostering the development of a new specialism.

Apart from the rigid compartmentalisation of professions in the FI and the dominance of the medical and engineering professional species, "There was a very strong case for saying that our system worked". Ibid. By the 1940's reported lead poisonings had fallen from well over 1,000 at the turn of the century to several tens. And a similar fall in silicosis rates seemed to be occurring. The FI seemed, through its national poisoning and industrial disease reporting systems ".... to have a much wider picture" LUXON (1986) than was the case in the USA. And the picture painted by these statistics was one of success. The British way of using inspectors to do subjective assessments, the division of labour between the traditional professions coupled with often detailed specification standards seemed to work. There was no need, at least in the 1940's and 1950's for measurement and OEL's and there was certainly no need for a new profession. Medical Inspectors such as McLaughlin and Mieklejohn were confident of their patient oriented approach and would resist any encroachment of a new upstart profession.

It is clear from Harvey's description of the introduction of IH techniques into widespread use in the FI that the he met much resistance. The founding fathers of IH in the UK were few in number and had no support from a proud and entrenched profession such as the sanitary and chemical engineers had in the US PHS. Harvey and the other missionaries were isolated. It took a long-time to subvert and alter the vision of the FI. The FI supertanker of received wisdom and custom and practice took a long-time to turn round.

Looking down the list of special factors which promoted IH in the USA one can make the following comments:

(i) The age, structure and success of the FI inhibited and militated against the development of IH (and OEL's).

(ii) Engineering was a low status profession in the UK compared to the USA.

(iii) Industrial medicine, certainly within the FI, was a high status profession quite capable of challenging the potential threat of any new professional species.

(iv) The UK had no local federal organisations in a position to produce innovative legislation or professions.

(v) Has already been dealt with as has (x).

(vi) The larger companies in the UK especially US subsidiaries adopted Taylorist methods. But generally management was on paternalistic lines and when it came to health and safety would follow the FI's lead. The emphasis was on specification standards and inspection not on performance standards such as OELs and measurement.

(vii) Although there have been exceptions medical training and attitudes have been more traditional. Very few industrial doctors saw the need for an IH profession or were prepared to foster or work with the few IHs that existed.

(viii) The same conservative, traditional approach to multidisciplinary subjects applied to universities in the UK until the 1960's. There was no room for innovative small units outside the mainstream subject divisions. Goodall's division of IH into physics, chemistry and engineering with no place for biology or toxicology was in line with the FI view and the view from the dominant ivory towers of higher education.

(ix) Industrial toxicology (IT) did not start as a recognised discipline until 1947 with the formation of an MRC unit. And once started there were no special links between it and IH. IT was the province of biologists and biochemists – there was no fusion of subjects and blurring of boundaries as happened in the USA.

As a result of all these factors IH in the UK started much later than in the USA. It did not grow organically out of old professions while learning from newly developing sciences such as industrial toxicology and meld them into something new. IH was grafted onto the UK scene from the USA and it has never, for the reasons examined, taken with the same vigour or confidence as in the country which created it. In the USA IH had grown to a position of size and power in the 1940's such that Patty could say that it had arrived and was not going to be displaced. By the 1960's and 1970's, toxicologists, chemists, physicists and others working in the occupational health field showed allegiance to IH by publishing in IH journals and giving papers at IH conferences. This occurs to a limited extent in the UK but not to the same degree. There is still a tendency to deconstruct IH into what the older professions or subjects regarded as its component parts thereby destroying the unique vision that is IH.

It is clear that IH in the UK, even in the late 1980's lags behind its progenitor in the USA.

The specific importance and relevance of IH to the development and use of OELs in the UK will be discussed in the next chapter. First a summary of the importance and development of OELs in the UK is needed.

10.3 OEL's in the UK

Although Legge and Goadby proposed a daily dose for inhaled lead in 1912 and the "dust datum" for asbestos was suggested in 1931 there is little evidence that their or other limits proposed outside the UK were ever used. It is clear from the section on IH in the UK that little measurement of airborne contaminants was done either within or without the FI right up to and through the Second World War. The UK approach of applying exhaust ventilation to remove "offensive or injurious" dust or fume and on occasions substituting for certain recognised dangerous substances, (see for instance the story of flint substitution in the potteries MARTLEW (1983)) did not require measurement. If measurement is not done there is no need for OELs of any kind whether as a measure of practicability ("dust datum") or health risk (lead dose). And anyway, the statistics of poisoning and prescribed diseases appeared to indicate that, in the main, the UK approach worked. The few people doing measurements like Goodall either referred to Section 47 of the 1937 Factories Act or like Angus compared their counts to US Maximum Permissible Concentrations (MPCs).

In 1937 Browning, working for the IHRB published her review of the published work on the toxicity of industrial organic solvents, BROWNING (1937). She quotes OELs from the USA in the text quoting, for instance, the National Safety Councils practicable level of 100ppm for benzene as "The maximum allowable concentrations above which toxic symptoms may be expected to occur ...." Ibid.

In contrast, one year later in 1938, Lehman and Flury published their book on the toxicology and hygiene of solvents. Apart from the fact that much of the work they quote has been done in their own or other German laboratories they derive their own list of "Higher Permissible Concentrations", LEHMAN and FLURY (1938). Beside this work Brownings review though competent is derivative and shows up the lack of basic research done in the UK that Goodall so bitterly complained about six years later.

In 1953 Browning produced a reworked version of her early review but now she quotes both ICIs, ".... tentative values of concentrations ..... greater than which indicate unsatisfactory conditions", and US PHS "Maximum Allowable Concentrations" BROWNING (1953). Here she confuses ACGIHs MAC/TLVs as being the official limits of the US PHS (Just the kind of confusion that Henry Smyth (Jnr) warned about and challenged in 1949). The qualifications she adds to her section on MACs are remarkably similar in content to the first preface to the TLV list published in

the same year. In the much extended second edition of her book published in 1965 the ICI "tentative values" have disappeared and both ACGIH TLV's and USSR MAC's are quoted, BROWNING (1965). Also the author acknowledges a debt of gratitude to Patty. The early influence of US OELs is clear and this continues in the shape of ACGIH TLVs in the early 1950's. But by this time ICI had published its list and so the two were quoted side by side. As was shown earlier, in many instances, the values were similar. By the 1960's the ICI values had gone, to be replaced by the USSR MACs. These three books published over almost two decades give some feel for where OELs, cited at least in the UK publications, came from. The strong influence of the ACGIH is clear.

10.3.1 The Factory Inspectorate and TLVs

According to Luxon, once the Chemical Inspectorate started doing airborne sampling in earnest in the later 1950's, early 1960's, "We felt we should give industry some sort of guidance on what sort of standards they should be maintaining" LUXON (1986). In 1959 Alan Coleman the chairman of the TLV Committee received a ".... request from the British Embassy to publish the (TLV) list in a table to be included in a new publication of the British Department of Safety, Health and Welfare", ACGIH (1959) to which he acceded. In 1960 the FI published Safety, Health and Welfare booklet Nº 8 "Toxic Substances in Factory Atmospheres" MoL (1960). "The series (was) designed to give information and advice about the best practices in the field of safety, health and welfare". And the preface went on to emphasise that there was no intention, "... to interpret the legal requirements of the Factories Acts..." Ibid.

Apart from general advice on how air contaminants could be controlled the introduction to the booklet contained a section entitled "Permissible Concentrations":

"While systems of control should be as effective as it is practicable to make them, it is desirable to have some guide to which the efficiency of the control measures can be related. In the list at the end of this booklet there are set out figures of maximum permissible concentrations of certain substances used in industry. For each substance a figure of concentration in atmosphere is given. If this concentration is exceeded, further action is necessary to achieve satisfactory working conditions. The List also serves as a general indication of the relative degrees of toxicity of these substances.

The concentrations given are based on these formulated by the Committee on Threshold Limits of the (ACGIH). The figures relate to average concentrations for a normal working day. They are based on the best available information at the present time, and are subject to annual review in the light of existing scientific knowledge." Ibid.

The name given to the TLVs and the wording of the introduction to the list are of particular interest given the earlier analysis of the TLVs.

The FI booklet is confused and confusing on a number of points. The name MPC and the description on the first paragraph that, "If this concentration is exceeded, further action is

necessary..." both suggest that TLVs are maximum or peak exposure levels not to be exceeded at any time. The FI definition causes exactly the same confusion as the ACGIHs first name for its OELs (MAC's) and its first definition published in 1953, already discussed in Chapter 6. In the second paragraph the FI go on to say that MPCs are in effect Time Weighted Averages. By changing the name and giving two contradictory definitions of what TLVs were in the space of two paragraph the confusion in the mind of the reader must have been just about complete. The definition also directly contradicts the ACGIH by claiming that TLVs can be used as measures of "relative toxicity". The ACGIH preface at the time specifically warned that, "the (TLV) should not be used as a common denominator of toxicity..." ACGIH (1960). And finally apart from repeating ACGIHs assertion that all TLVs are reviewed annually the FI inject the magic words "scientific knowledge". Thus, whereas the ACGIH preface states that TLVs "... are based on the best available information from industrial experience (and) from experimental studies...." Ibid, the FI claim that TLVs are set "... in the light of existing scientific knowledge." A far grander and potent formulation of words and very significant given the earlier discussion of how the words "science" and "scientific" are used to bestow unquestionable status on authoritative statements.

These confusions and contradictions are, to use a well worn but nevertheless appropriate phrase, not accidental. They are a sign of the status and understanding of IH in the FI. Some of the confusion results from ignorance but some is due to the fact that the Chemical Inspectors were bending the definition of TLV's to their particular purpose, to fit in with the FI's approach to controlling toxic substances. Both the quote from Luxon and the first sentence of the MPC definition make clear that the FI wanted limits by which employers could judge the effectiveness of their control measures.

The FIs early view of OELs was as benchmarks for engineering control, indeed Harvey's report to the Chief Inspector was quite explicit on this point:

".... the justification put forward for (MACs) in America is not easily refuted, since they argue that if the air is to be cleaned by mechanical means.... the designer of the equipment must have a design limit to work to." HARVEY (1954a). The FI wanted TLVs to be engineering "design limits" but in reality the ACGIH regarded them as exposure limits to protect workers health.

On the question of the confusion about whether MPCs were peak or TWA levels this probably relates to the question of measurement. Most if not all the "Red Book" methods available in the late 1950's measured atmospheric concentrations of substances over short periods of time (1–20 minutes). Methods for measuring full-shift exposure did not develop until the mid-late 1960's. And yet the TLV was ostensibly set to protect against long-term chronic health effects and was therefore supposed to be related to the average exposure over a shift. The gap between the sampling duration of the then current measurement techniques and the definitions of the TLV

caused a lot of argument in the early days of the ACGIH TLV Committee. In Annual Conference after Annual Conference it had to correct people and draw their attention to the fact that the TLV was an eight-hour TWA and it was a person's or population's eight hour average exposure which should be measured or estimated. The FI could not contemplate insisting either for their own Chemical Inspectors or for occupiers that the eight-hour TWA should be measured or estimated and so the issue was fudged and confused. One can understand the pragmatic, practical reasons for doing so but the contradictory definition must have caused a great deal of confusion.

How were TLVs regarded by the FI in the late 1950's and early 1960's and what did Chemical Inspectors find when they started applying them?

"We had a great regard for their (ACGIHs) work and we saw that as better than doing anything ourselves.... I think it (the TLV) was seen by Inspectors who were knowledgeable **as being a standard which would protect health with a fair built in safety margin."** LUXON (1986). (Author's emphasis).

For Harvey TLVs were used as one part of the assessment they: "Supported the action we had previously taken. Where the engineering control was good the TLV was not exceeded". HARVEY (1986).

And the TLVs were received "very well" according to Luxon and, "The big employers were quite happy because it (the TLV) did give them a way of underwriting the conditions in their plants... it put them in a very strong position if they subsequently became liable for civil claims." LUXON (1986).

These views are interesting in that they confirm the earlier analysis. TLVs were different things to different men. For some they were health based, for others they were also practicable and indicative of a good standard of control. And as I have already arrived at the conclusion that TLVs were and are reasonably practicable OELs it is not surprising that when Chemical Inspectors in the FI started to apply them they found that they were achievable in the well controlled factories.

In later editions of Safety, Health and Welfare booklet Nº 8 the FI introduction was dropped and the ACGIH preface reproduced in its entirety. This was forced on the FI by the ACGIH when it decided in the mid 1960's that the TLV list and preface could only be reproduced in its entirety or not at all. The introduction to the booklet did draw attention to the fact that some UK measurement techniques were different from the USA and in 1968 a warning was given that a UK asbestos limit would be introduced in a new set of regulations, DoLP (1968). Booklet Nº 8 the next year listed the first UK OELs to differ from the TLV list – the chrysotile asbestos standard became 2 fibres/cc of air and the crocidolite standard was set separately at 0.2 fibres/cc, gravimetric standards were also listed. In 1971, the TLV list and preface was published in a new FI series of Technical Data Notes which listed the TLVs of the previous year – plus any UK/US differences.

After the creation of the HSE in 1974 a new series of Guidance Notes was started and the TLV list of the previous year was reproduced in Guidance Note EH15 each year up to 1980 (by this date the TLVs reproduced was current ie., 1980 TLVs). By this date the UK introduction listed 11 substances where UK recommendations differed from the ACGIH. Of those 11 differences, four were on the recommendation of the HSC Advisory Committee on Toxic Substances (ACTS) and one was due to a European Directive (Vinyl Chloride), HSE (1980). The 1980 edition of EH15 was the last one printed. After that there was a four year hiatus and a new list of Control and Recommended Limits was produced in 1984 in Guidance Note EH40. It is worth at this point having a brief look at the definitions of Control and Recommended Limits.

Control Limits are judged, ".... to be reasonably practicable" for the whole spectrum of work activities in Great Britain ... and should not normally be exceeded" HSE (1984).

These OELs have greater force in law than the TLVs which were always guidance levels and this is made clear in the Guidance Note:

"The HSE will use control limits in determining whether, in their opinion, the requirements of the relevant legislation are being observed. Failure to comply with control limits, or to reduce exposure still further, where this is reasonably practicable, may result in enforcement action" Ibid.

Recommended limits, ".... are considered to represent good practice and realistic criteria for the control of exposure, plant design, engineering controls.... HSE inspectors will use these exposure limits as part of their criteria for assessing compliance with the HSW Act and other relevant statutory provisions." Ibid.

The definition of Recommended Limits is the same as Luxon and Harvey used to describe the use of TLVs in the early 1960's and these limits probably have as much force in law. In reality the first list of Recommended Limits was a slightly amended and shortened version of the 1980 TLV list. From the definition, Control Limits might appear to be different from Recommended Limits, but as the latter were in effect TLVs the difference is more apparent than real. The only real difference between the two types of Limit is that Control Limits have greater force in law and are easier and therefore more likely to be used in enforcement actions. By changing the name and producing two types of OEL HSE appeared to be breaking free of the ACGIH TLV list. In fact they were and still are strongly influenced by the ACGIH and when they decided to erect a two tier system of OELs they were in fact heading off down a cul-de-sac. The work of ACTS will be explored in more detail in the the next chapter. Before that though it is worth considering the setting of OELs in the UK before the ACTS.

CHAPTER ELEVEN

UK OCCUPATIONAL EXPOSURE LIMITS

Introduction - The Early years

In this chapter the development of UK OEL's will be examined and then discussed in the context of the environment described in the last chapter, the analytical framework of this thesis and the earlier analysis of TLV's and the TLV Committee.

Legge's proposed lead standard of 1912 and the asbestos "dust datum" of 1931 were UK OEL's. But they were exceptions, they were not used and little other work to produce OEL's was done in the UK until the late 1940's. From the description of the development of IH in the UK and the organisation of the FI's specialist and medical inspectors one can see why. There was no demand for OEL's there were few if any multi-discipline teams which could produce OEL's and there was no IH profession to lobby for and promulgate the idea of OELs. Goodall could see the need from his practical hygiene work but his pleas for more resources and better organisation fell on deaf ears. The FI did nothing to produce or encourage the production of OELs in the 1950's, indeed Medical Inspectors such as McLaughlin were not impressed with OELs.

"Individuals.... vary greatly in their capacity to deal with dusts, and of two men who have been working at the same job for the same length of time one may get a disease of the lungs and the other may be unaffected. This is one reason why I am not greatly impressed by the validity of what are known as maximum allowable concentrations of dusts (MAC), of which lists have been drawn up in various countries. The MACs seem to be based on the assumption that man is a standardised machine, which he is not". McLAUGHLIN (1953).

Even the people who were later to become the backbone of the BOHS were tentative and shy of mentioning or proposing OEL's in the early 1950's. Bloor who went on to work on methods of improving dust control in the pottery industry and became president of the Society had the following to say about the silica dust TLV:

"Standards of dustiness for foundries have been proposed by various overseas investigation.... however, the present state of medical knowledge on this subject does not warrant a statement on the maximum amount of dust which can be inhaled safely." BLOOR (1951).

Having stressed the need for combined studies of silicosis and dust exposure he continues:

"In the meantime, it might be useful to adopt some measure of what is considered good practice in the industry as a tentative standard." Ibid.

Similarly Roach who was to become so influential in the OEL setting activities of the BOHS in the late 1960's made no mention of TLV's or any other OELs in his paper on dust measurement to the inaugural conference of the Society, ROACH (1954). Even more surprising there is no mention of OEL's by a toxicologist who presented a paper on chemical toxicity at the same meeting, HARVEY (1954). And perhaps, in the present context, the most interesting observation on the papers given at this meeting is that the only person to mention OEL's was the chief design engineer for ICI Limited, BRAHAM (1954). Having described the stages in the design of a new plant he stresses that the engineer also requires "hygiene requirements" and that the specification "... must, for example, inform him of the permissible concentration of a toxic gas .... so that he can determine what type of gland he must employ for process fans in the plant or whether the fans must be placed outside the building or perhaps in a cubicle." Ibid

For Braham, like Harvey and Winslow several decades before them OELs were needed by engineers as measures of good practice, as "engineering design limits."

In fact, in the early 1950's there were only two organisations setting OEL's in the UK, ICI Limited and the Medical Research Council's Pneumoconiosis Research Unit (MRC-PRU). ICI Industrial Products and Health Research Committee published its list of internally generated OEL's in the early 1950's and methods for air sampling in 1956. They were reasonably practicable limits set by the largest chemical company in the UK and they overlap considerably with the contemporary ACGIH TLV list. Interestingly Goldblatt the company's chief medical officer from his writings clearly understood toxicology and used what was understood of toxic mechanisms as the starting point in his papers GOLDBLATT (1943 and 1955). He had an idealistic view of OELs which explains why the company shied away from using the then current acronyms TLV or MAC:

"The use of the words 'maximum allowable concentration' has been avoided because at ICI we hold that no concentration is allowable. Some are worse than others but all are bad." GOLDBLATT (1955).

Why did ICI set its own OELs? For various reasons including some or all of the following:

(i) Because it operated in international markets where its competitors used or worked to TLVs and it wanted its own OEL's to put up against them.

(ii) Because it was a responsible company and its health and safety personnel became converted to the OEL paradigm as promulgated by the ACGIH.

(iii) Because company design engineers wanted "design limits" to work to.

(iv) Because the company wanted to protect itself from civil claims.

Whatever the exact combination of reasons IHs and others working in the UK in the 1950's would usually quote ACGIH and ICI OELs, SHERWOOD (1984).

The FI, by publishing TLV's from 1960 onwards were not blazing a new trail they were following an old one blazed by ICI roughly a decade earlier. The FIs contribution was to signal to the whole of industry that OELs, especially TLV's, were a legitimate part of an assessment.

Apart from ICI, work at the MRC's PRU in Cardiff started in the late 1940's eventually led to a coal dust OEL and Roach learnt and developed many of the ideas and techniques he was going to use in the 1960's during this time. He drew strongly on the earlier work of surveys conducted by the US PHS and his own work on dust measurement and sampling strategies with Oldham, OLDHAM and ROACH (1952). In a paper published a year later Roach built on the work of Finney a statistician who worked at Rothampsted Research Station to propose a method of modelling cumulating dust exposure and the distribution of the risk of contracting pneumoconiosis ROACH (1953). (It was Finney who was seconded to the FDA in the USA before the Second World War to help them develop their biostatistics department). Roach's attitude to risk and the other factors which affect the setting of OELs are clearly laid out and a strong echo of these views are to be found in the work he did on cotton dust with Schilling in the late 1950's and 15 years later on the BOHS asbestos dust standards.

Having derived a dose-response relationship Roach makes the following observations:

"An exposure response curve of this kind provides an estimate of the risk of developing pneumoconiosis in terms of the total dust exposure. Although not sufficient in itself, it is an essential piece of information for the establishment of standards of air cleanliness." Ibid.

He goes on:

"For all practical purposes as long as there is dust there will be some pneumoconiosis, and it is first necessary to consider just how much or how little pneumoconiosis can be tolerated in the colliery." Ibid.

In the summary he describes his two stage view of the standard setting process even more starkly:

Firstly an exposure-response relationship is determined and this is "... needed for the establishment of standards ....". However, "**A decision must first be taken on the maximum risk that can be tolerated** and the dust exposure is reduced below the level corresponding to this risk" Ibid (authors emphasis).

It would seem that Roach's ideas or beliefs about the inevitability of a certain amount of risk and the need for standard setters to choose a level of risk "... that can be tolerated" stem from this time. But does he mean "... as long as there is dust there will be some pneumoconiosis..." ? Or does he really mean as long as coal mining is carried out then even with the application of all foreseeable dust control techniques the dust levels, while lower, will still cause pneumoconiosis in a small number of miners?

The first statement has a certain fatalistic inevitability about it. The second is more pragmatic and probably more accurate especially in the light of observations made a little later where Roach considers practical factors:

"Other factors which may have to be taken into consideration (in determining "standards of air cleanliness") are the cost of compensation, dust suppression, dust measurement and radiological examination, and, in a colliery short of man power, the effect of pneumoconiosis on recruitment as well as on loss of men" Ibid.

The more pragmatic formulation also receives support from the fact that, seven years later when Roach is concerned with cotton dust and byssinosis, he argues that because the mineral dust concentrations is 100 times lower in a cotton mill than in a coalmine the risk can be ignored - "with very low levels of dust there will be no pneumoconiosis", ROACH and SCHILLING (1960).

Apart from ICI's list and the coal dust limit eventually derived from the work of the MRC-PRU and other research centres such as the Institute of Occupational Medicine (IOM) in Edinburgh no other UK OELs were set and published in the 1950's. Luxon had a recollection that a government committee had been set up in the early 1950's to investigate the possibility of setting and documenting OEL's but after a few meetings the project was abandoned "...because of the effort involved" LUXON (1986). (No mention of such a committee was found in the PRO records of the FI but it may be that the committee Luxon refers to was part of another government department). The next phase in the UK OEL setting story took place in the 1960's and early 1970's. This period was the heyday of the UK IH community's involvement in setting OEL's and interestingly it was not the number of standards that were derived which made this an important era, the number was small, but it was the quality, timeliness and originality of approach which had the impact.

11.1 BOHS OEL's

The next section examines four of the five hygiene standards the BOHS set and then goes on to discuss their common features, their impact, their relationship to ACGIH TLV's and their reception by the FI. The last hygiene standard that was set was for cadmium, BOHS (1978), and there was

a five year gap between this standard and the other four. In many ways it differs from them, in particular a tolerable risk estimate is given, it was heavily criticised (see for instance KJELLSTROM (1979)) and it had nothing like the status or impact of the first four standards. For these reasons no detailed consideration of the cadmium OEL will be undertaken.

In the mid 1960's the FI Senior Medical Inspector set up a panel of experts to consider the asbestos problem. Dr Newhouse had published evidence linking asbestos exposure to mesothelioma, a cancer of the lung pleura, "... and public interest was particularly stimulated by the publication...." GREAT BRITAIN (1967).

The expert panel considered the Chief FI's remit and by means of an FI survey found 300 factories registered under the 1931 Asbestos Regulations. Another 228 factories were found in which asbestos was used but which were not covered by the Regulations, groups such as laggers and insulations workers, who were known to be highly exposed, were not covered. The diagnosis of asbestosis was increasing though the rate might not have been and the panel quoted and commented on a prophetic statement by Merewether in his 1930 report:

"... in the space of a decade, or there about, the effects of energetic application of preventative measures should be apparent in a great reduction in the incidence of asbestosis. This prophesy unfortunately has not been fulfilled." Ibid.

They quoted evidence showing that of 247 new cases of asbestosis diagnosed by Medical panels between 1955 and 1963, "... no fewer than 164 or 67% had entered the industry in 1933..." Ibid.

The expert Advisory Panel "... accepted that there is an excess incidence of lung cancer in those dying with asbestosis". However although American investigators accepted there was a lung cancer risk in the absence of demonstrable asbestosis the Advisory Panel did not But apart from the lung cancer question, with or without association with asbestosis there was another cancer which concerned the Advisory Panel:

"This growing evidence linking many mesothelial tumours ... with exposure to asbestos, apparently of slight degree, or remote in time, constitutes, in our opinion one of the most serious aspects, particularly from a public health point of view, of the asbestos problem." Ibid. But the Panel do not conclude that only crocidolite causes mesotheliomas and quote evidence implicating other fibre types. However they do conclude that "... in the light of the existing evidence incriminating crocidolite..." that other asbestos types should be substituted wherever possible and control measures should ".... be even more rigidly applied to crocidolite" than with the other asbestos types.

The Panel did not believe, from the evidence, that "conventional periodic medical examinations" were useful. By the time mesothelioma or lung cancer was diagnosed it was too late and even with asbestosis there was evidence that it progressed even after a person was taken out of dust exposed jobs. They did suggest the development of more sensitive techniques, X-ray and other, of detecting the very earliest signs of asbestosis.

The Panel were not impressed with the ACGIH asbestos TLV. "Although issued as a biological standard this value .... lacks recent confirmation and includes.... several practical defects...." Ibid. However they approved of the $0.1mg/m^3$ standard which the FI had added to their publication of the TLV list. "Although this level is an arbitrary one, it seems from practice to require for its attainment, a correspondingly high standard of dust control by conventional engineering techniques." And they go on to quote the fibre counts obtained in a textile mill where "... every endeavour has been made to reduce airborne asbestos...." (Turner and Newall in Rochdale). The processes and fibre counts are reproduced in Table 11.1

PROCESS	FIBRE COUNTS PER CC
Carding	7.7
Roving frames	5.5
Cheese winding	5.0
Beaming	4.5
Pin winding	3.0
Bag slitting	4.3
Mechanical bagging	3.8
Doubling	2.4
Weaving	3.0
Webbing (narrow widths) weaving	1.9
Plaiting	3.8

Table 11.1 (taken from GREAT BRITAIN (1967)).

They suggest that these are the figures that all similar factories should aim at but they add an important rider; "We would not wish to regard the above levels as standards but would prefer to regard them as immediate goals. As knowledge develops, the goal should be subject to progressive reduction" And they looked to the BOHS Standards Committee "... to be of use in setting an agreed national goal." Ibid.

The Advisory Panel's findings and opinions set the background behind the work of the BOHS Standards Committee. The diagnosis of asbestosis was increasing and it was clear that asbestos also caused lung cancer and mesotheliomas. A significant part of industry where asbestos exposure occurred was not covered by the 1931 Regulations and the controls instituted after 1931 had not presented asbestosis or the cancers linked to asbestos exposure. The picture painted by the Advisory Panel was bleak.

11.1.1 Asbestos Hygiene Standards

The BOHS Committee on Hygiene standards (which will be referred to as the Standards Committee) published its first standard in June 1968 for chrysotile asbestos, BOHS (1968). Roach wrote the report and it bears his stamp. Using data supplied by the asbestos industry the Committee drew upon Roach's earlier work on coal-dust and related cumulative asbestosis dust exposure to the incidence of asbestos and other signs of effect. The Standards Committee recommended a hygiene standard of 2 fibres/cubic centimetre of air (averaged over 3 months). The Committee or Roach himself, modified his earlier stated view on dust by introducing the idea of tolerable risk:

"As long as there is any airborne chrysotile dust in the work environment there may be some overall risk to health. Nevertheless, it should be realised that exposure up to certain limits can be tolerated for a lifetime without incurring undue risks.

The Committee believes that a proper and reasonable objective would be to reduce the risk of contracting asbestosis to 1 per cent of those who have a lifetimes exposure to the dust." Ibid.

And later a caveat is added which could be straight out of Roach's earlier work on coal dust or his collaborative work with Schilling on cotton dust:

"Knowledge of the relationship between airborne dust exposure and the risk of asbestosis is not in itself sufficient to establish a hygiene standard. Another important problem, and one which is very difficult to resolve, is that of balancing the risks to health against the consequences of demanding excessive dust reduction." Ibid.

The Committee resolved the problem by deciding the 1% risk was tolerable, it openly defined the "nearly all" that the ACGIH referred to but never quantified. This was the major innovation that the BOHS introduced into the field of OEL setting and it did the same with the other hygiene standards it published. In the case of chrysotile asbestos it also acknowledged the cancer hazard but it argued that, "The primary danger of inhaling asbestos dust is asbestosis ...." and anyway "... the quantitative relationship between asbestos and cancer risk is not known. .... Consequently it is not possible, at this time to specify an air concentration which is known will be risk free in this respect." Ibid.

The BOHS Standards Committee deliberations overlapped with those of the FI's Medical Inspectors Panel and they knew of each others work. The possibility that asbestos induced lung cancer had been strongly suspected since the late 1930's and some estimate of the size of the risk was available after Doll's research was published, DOLL (1955). The link between mesothelioma and asbestos exposure was made in the 1950's and confirmed by the early 1960's. The Panel regarded both these hazards as serious and hoped that dust control applied to reduce the incidence of asbestosis would also reduce, if not eliminate the cancer hazard. The Standards Committee did not address the problem head on but avoided it by arguing that asbestosis was the "primary

danger" and anyway, there was no dose exposure data by which to assess the cancer hazard risk at different levels of exposure and determine a level which was "risk free". The first reason given is not in accord with the FI's view published a year earlier and the second, while technically correct, dose response data for asbestos and cancer were not available until the mid 1970's, is strange given the Committee's approach to asbestosis. It had not chosen a "risk free" level in this case so why was it necessary to do so in the case of lung cancer and mesothelioma. The primary reason is probably because the Committee could not bring itself publicly to state that a certain risk of contracting cancer was tolerable. But perhaps another, and no less influential reason was that there was a way of relating cumulative exposure to asbestos to the risk of asbestosis and reminds one of a joke: An epidemiologist has lost his keys and is looking on the ground around the base of a streetlamp. When asked where he thought he had lost his keys he replied that they were probably 20 yards further down the pavement but the light was better under the lamp so that is where he was looking. The Standards Committee wanted to set an OEL for chrysotile asbestos and it was given data by industry which could be used to define the exposure response relationship for asbestosis. It therefore set an OEL based on this data because that was what was available and it is in the nature of OEL Committees to set OEL's. The light was better under the asbestosis lamp but the key to the asbestos hazard was in the street marked cancer. The Standards Committee was doing no more or less than similar committees before or after it. It was using the evidence it had available to set a reasonably practicable OEL. What made the BOHS chrysotile OEL so important was that the process was more explicit than that of the ACGIH. And like the ACGIH the Standards Committee knew that its OEL for chrysotile was, at the time, at the more stringent end of the spectrum of reasonably practicability. The data in Table 10.2 show what was being achieved by a large company applying what was regarded at the time as good control technology. Only one process gave an average exposure level below 2 fibres/cc. The Standards Committee knew, as did the ACGIH TLV Committee, that by applying its OEL, conditions across large parts of the asbestos industry would improve. And in this case the BOHS standard was considerably more stringent than the ACGIH TLV which, in 1968, stood at 12 fibres/cc. The BOHS 2 fibre standard was used in setting agreed "national goal" and was incorporated in the Technical Data Note which accompanied the 1969 Asbestos Regulations, GREAT BRITAIN (1969). As had been done before in 1960, when the TLV's where first published by the FI, the sampling durations were changed to suit the enforcing Inspectors needs. The BOHS 2 fibre standard referred to a 3 monthly average. The FI took the numerical limit and applied it to a four-hour sampling period and it went further and allowed the inspector to take a 10 minute sample. If the average concentration over this 10 minute period was > 12 fibres/cc then regulations 7 or 8 of the 1969 Act applied. By shortening the sampling period the FI made the BOHS 2 fibre standard more stringent and reducing the initial sampling time to 10 minutes was done purely as a matter of convenience and in terms of hygiene sampling theory was indefensible. However, there was also a certain disingenuousness to the BOHS approach and a refusal to consider the application of its standard. Quite where the 3 month averaging period came from is not clear although a long averaging time is

in order for substances which are not cleared from the body and where the effect is cumulative and chronic. However even though the BOHS Standard Committee felt that 3 months was the appropriate averaging period it should have addressed the problem of application by relating the 3 monthly average to an appropriate 8 hour average. By not addressing this, the Standards Committee left the door wide open to the FI to reinterpret its hygiene standard, and to a degree, some individuals at least, on the Committee must have known this would happen.

The BOHS did not set a standard for crocidolite although the FI Technical Data Note listed an OEL of 0.2 fibres/cc. Harvey sheds some light on how this figure was arrived at, HARVEY (1986). Having decided to use the BOHS 2 fibre standard for chrysotile he asked his medical advisers how much more dangerous is crocidolite and was met with the answer that it was difficult to say. He decided to try and asked "Well, is it one hundred times more dangerous? "No, not as much as that", was the reply. "Is it twice as dangerous?" No, more than that was the second reply. "Is it ten times more dangerous?" "Yes, perhaps ten times" - was the final reply. And so the 2 fibres standard was divided by 10 - the crocidolite standard was set by comparison with chrysotile based on a feeling for how much more dangerous it might be. This procedure was not unique and was clearly used by the TLV Committee where it based TLV's on analogy with the TLV's of similar substances. And as with the TLV Committee the final number arrived at owes a lot to the feelings of the committee and has a certain arithmetic neatness.

Five years after the Standards Committee set the 2 fibre chrysotile standard it set the same standard for amosite which, with hindsight is a little strange. It claims that life long exposure will result in a 1% risk of asbestosis as with chrysotile but, in the document outlining the standard, the Committee states that amosite appears to be more biologically active than chrysotile and eliminated more slowly from the lungs BOHS (1973). And the Committee even went as far as to say: (the evidence) "... indicates that different standards may eventually be established in amosite operations." Ibid. But it did not feel able to make the jump and set a lower OEL. The amosite standard was also adopted by the HSE but eventually in 1983 differential standards were set for chrysotile, amosite and crocidolite asbestos.

11.1.2 Noise

The amosite OEL was the penultimate of the five hygiene standards set by the BOHS. The second published in 1971 was on "wide-band noise" BOHS (1971). Again the Committee used data recently released, this time by a joint MRC-NPL piece of epidemiological research, (see BURNS and ROBINSON (1970) – for a summary of the research and its results). And again the Committee decided that a 1% risk of occupational deafness was tolerable.

"... a reasonable objective would be to restrict occupational exposure to noise so that handicap should not occur in more than 1 per cent of persons exposed for a working lifetime." BOHS (1971).

The Committee arrived at this risk figure by choosing a loss of hearing acuity of 40 decibels as the threshold of hearing handicap and using a scale of hearing handicap based upon symptoms devised by Atherley and Noble, ATHERLEY and NOBLE (1971). The Committee did not use the more conventional approach adopted by Burns and Robinson and the BSI a few years later of relating noise emission levels to hearing loss as measured by pure tone audiometry. When the BSI eventually published its assessment of the Burns and Robinson data some five years later it arrived at a risk estimate of 15% for a 30 year exposure at 90dB(A) (ie. a noise emission level of 105dB), BSI (1976). The BSI judged that hearing impairment started at 30dB loss, they used fewer audiometric frequencies than the BOHS and hearing loss was judged on the basis of pure-tone audiometry alone. The BOHS Standards Committee chose a considerably higher fence for defining the beginning of handicap but the principal reason for the large difference in risk estimates is the use of the interesting but unconventional test of hearing handicap based upon symptoms. There are good arguments for using a more realistic measure of handicap than pure tone audiometry and this area of research was the particular interest of one person on the Standards Committee (Atherley). However the whole implication of the BOHS document is that the Burns and Robinson data (based on pure-tone audiometry) is the basis of the hygiene standard. It is only at the end of the document in Appendix 2 that it is made clear that the committee has done something different. It is hard not to conclude that the Standards Committee was attracted to this relatively unorthodox method of analysis because the resulting risk estimate came out at 1%, in line with its previous pronouncement on chrysotile exposure and asbestosis. For a few years 1% became the tolerable almost the "acceptable" risk level for the Standards Committee. As with chrysotile the Committee knew that its reasonably practicable limit would if enforced, bring considerable improvements across UK industry. There were no nationwide figures available at the time but a decade later an HSE estimate put the number of employees exposed above 90dB(A) (8-hour Leq) at $\frac{3}{4}$ million, HSC (1981).

A year after the wide-band noise standard was published the Department of Employment, of which the FI was a part, published a "Code of Practice for reducing the exposure of employed persons to Noise" GREAT BRITAIN (1972c). The DoE was advised by an Industrial Health Advisory Committee, sub-committee on Noise which recommended a limit of 90dB(A) for an 8-hour working day, the equivalent of the BOHS lifetime noise emission standard. It seems highly likely that the BOHS standard strongly influenced the DoE sub-committee or at the very least strengthened its resolve to set a 90dB(A) limit.

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* Although noise is not a chemical hazard a discussion of the BOHS standard is included here because it helps to demostrate Society's approach to hygiene standard setting in the late 1960's early 1970's.

11.1.3 Cotton Dust

The last hygiene standard to be considered is the third published by the Standards Committee – for cotton dust, BOHS (1972). Before considering this standard it is worth examining briefly the history of knowledge concerning cotton dust and the disease it can cause, byssinosis.

Two reports were presented to the FI in 1930 on the health effects of dust at work. One on asbestos and another on dust in cardrooms in the cotton industry, GREAT BRITAIN (1931b). The asbestos report led to the 1931 Asbestos Regulations, the cotton dust report led to further research. A German research worker was commissioned to investigate cotton dust diseases and came to the conclusion that the effect was caused by the proteinaceous component of fine dust GREAT BRITAIN (1936). In 1937 a new Factory Act was passed which contained an explicit ruling in Section 47 that:

"In every factory in which .... there is given off any dust or fume or other impurity of such a character and to such an extent as to be likely to be injurious or offensive to the persons employed - measures shall be taken to protect the persons against inhalation...." GREAT BRITAIN (1937).

This extended and amplified similar provisions in earlier Acts.

In 1944 a tripartite "Joint Advisory Committee (JAC) on conditions of work in the cotton trade" was set up to deliberate on a variety of topics, one of which included the problem of the control of dust. The first Interim report was published in 1946 and concluded "after extensive enquiry" that it ".... has not been able to find any mill so equipped as to provide the complete answer to the problem so far as process dust is concerned, nor has its attention been drawn to any apparatus which will ensure compliance with Section 47 of the Factories Act, 1937." GREAT BRITAIN (1946).

Between 1946 and 1961 another three Interim reports were published on work which addressed this problem, GREAT BRITAIN (1952, 1957, and 1958) and in 1961 the final report was published GREAT BRITAIN (1961b). This reported on the successful application of two systems of exhaust ventilation to carding machines and other control methods. Thirty years after the first FI committee report on cotton dust disease, 17 years after the JAC on dust control was set up and four Interim reports later the FI and the industry came up with an agreed solution to the problem of dust control in cotton mills.

The approach taken to control dust in the cotton industry, and in particular to meet the provisions of Section 47 of the 1937 Factories Act, is an archetypal example of the "British Way", the way that Harvey and the other "missionaries" tried and partially succeeded in changing in the 1950's. Nowhere in the four Interim and the final JAC reports on dust control are OEL's mentioned as

measures of dust control success or dust exposure. The whole approach is focussed on the specification of controls, usually exhaust ventilation, for certain types of machine or process. The question of control appears to have been completely divorced from the question of medical evidence of harm or what an appropriate hygiene standard (OEL) might be. Towards the end of the 17 years of deliberations of the JAC two research scientists, an IH and a doctor made careful measurements of dust exposure and byssinotic symptoms. They produced a dose response relationship from their work, ROACH and SCHILLING (1960), but did not feel justified in applying the earlier regression model Roach used for coal dust. This was because selection was thought likely to have occurred amongst the more highly exposed workers; they were dealing with a survivor population. [An important difference between mice and men, when applying animal dose response models, is that men can, depending upon their circumstances, leave their job whereas mice cannot leave their cages]. From their work the authors found "none of those workers exposed to a concentration of less than 100mg per 100m3 had byssinosis grade 1 or 2". Ibid. But they also judged this level impossible to attain throughout the cotton mill and they thus conclude:

"At the present time we do not think it would be wise to recommend a target level of dustiness which could not be achieved in practice for this would be likely to result in little or no action. Our aim would be initially to adopt a realistic target that could be achieved in the immediate future by reasonably practicable measures, namely that dust concentrations in cotton mills should be less than 250mg per 100m3". Ibid.

This statement could equally have been written by the ACGIH TLV Committee or indeed any other OEL setting committee bent on setting a single figure enforcible OEL.

The ACGIH did in fact set a TLV for cotton dust in 1970 drawing on the work of Roach and Schilling and other UK investigators and it was 12 years before the BOHS sub-committee on vegetable dusts (Chair - Schilling, Secretary - Roach) published an OEL, BOHS (1972). In the section of the standard document entitled "derivation of a hygiene standard" the Committee lays out its approach to setting OEL's:

"The achievement of an air quality standard does not, by itself, guarantee completely safe conditions of work ... The application of engineering control procedures will limit and control the dustiness, but it is unrealistic to suppose that the dustiness can be reduced to zero. A recommended standard of air cleanliness should, to be accepted, be both technically feasible and within the means of industry". Ibid

In the first paragraph of the summary and recommendations at the start of the document the Standards Committee makes exactly the same points about any level of dust equalling some level of risk and certain dust levels being "tolerated" as it did in the chrysotile asbestos standard. And the second paragraph has the same form as in the asbestos standard though the contents are different:

"... a proper and reasonable objective would be to reduce the prevalence of grade II byssinosis due to airborne dust to less than 4 in 100 (4 percent) among those who work in the dustiest

conditions." And in line with this views outlined in the body of the standard the Committee continues a little later "... it is necessary for the industry to have realistic dust levels at which to aim in order to reduce the risk of contracting the least demonstrable permanent effect to as low a level as possible." Ibid.

The OEL (8 hour) which corresponded with a 4% risk of grade II byssinosis was set at 0.5mg/m^3 (less fly) which corresponds roughly with Roach and Schillings "reasonably practicable" and "realistic target" level of 250mg/100m^3 (including fly). Thus in the intervening 12 years despite the introduction of exhaust ventilation on carding machines the level which is judged to be "realistic" has not fallen. One explanation which Harvey referred to in his interview is based on his experience in the Oldham FI district. He encouraged the introduction of the Shirley Pressure Point system into cotton mills but he found that the system had limited effect because the process technology had changed. Speeds had increased and this tended to overwhelm and reduce the effectiveness of the Shirley system.

As in Roach and Schillings earlier paper the Committee recommended regular medical surveillance every three years and annually if the dust levels were "excessive" (> 1.0mg/m^3 less fly).

11.1.4 BOHS OEL's discussion

The BOHS Standard Committee's five hygiene standards (four if you discount the wide-band noise standard) were the first and the only OEL's produced by the UK IH community as a formal entity.

As has been explored earlier although the IH community in the UK took its inspiration from IH in the USA it grew and developed in a very different environment. Also, significantly, there was no division of hygienists into governmental and academic and those working in private industry, as took place in the USA. Why this did not happen is almost certainly due, in the broadest sense, to differing political perceptions in the UK. Especially in the post-war period there was a widely held belief in the social democratic, unitary view of government. A consensus had apparently been forged around a stable mixed economy. The stark divisions, that some at least in the PHS in the mid-1930's saw, between "labor" and "capital" did not exist for many in post-war Britain. Another reason why no public/private division occurred must also be put down to sheer practicalities. When the BOHS was created there were very few people who regarded themselves as IH's. To divide the community's strength into two groups at this time was not a practicable option. In any case the UK equivalent of the PHS had next to no IHP within it to form the nucleus of a UK 'ACGIH'. The unitary view, that there are no fundamentally irreconcilable differences between hygienists working in the public and private sectors, has had an impact on BOHS OEL setting. But before its relevance is considered it is worth examining how closely the BOHS followed the OEL paradigm.

The Standards Committee was in basic agreement with the paradigm as one might expect, they being followers of the US IH profession. However the Committee reinterpreted the paradigm and did not follow the ACGIH approach slavishly. In particular the Standards Committee:

(i) Defined and quantified the phase "nearly all".

(ii) Made questions of practicability explicit.

(iii) Argued that the level of risk it had calculated was "tolerable".

(iv) Implied, in the hygiene standards documents, that the percentage level of risk was arrived at first and then the corresponding exposure level was selected.

(v) Was quite explicit that it regarded its OELs as reasonably practicable.

BOHS Hygiene Standards were in every sense set on the same basis as ACGIH TLV's. By concentrating a greater amount of effort on two substances the reasoning behind the standards was made clearer. And by facing some of the more contentious issues buried in the paradigm and the ACGIH method head on the Committee laid bare the reasonable practicability of its standards and put a numerical value to the risk at what it regarded as a fair balance point between risk and cost.

Why did the BOHS feel the need to set reasonably practicable OEL's and why did it not break with the paradigm?

Firstly because the BOHS and UK IH in general was borne and raised on the OEL paradigm imported from the USA.

Secondly because the UK FI had always worked in a co-operative manner with UK industry as did the US PHS. The strategy for improving working conditions in the worst places was to try and bring them up to the conditions in the best. Standards of whatever kind, including OEL's had, according to this scheme, to be 'realistic' else the strategy fell apart.

Thirdly because of the unitary nature of the BOHS and its committees. These always had a nominally tripartite structure as did the Joint Advisory Committees of the FI and the later committees of the HSC. Such committees, as has already been discussed in the context of the ACGIH TLV Committee and industry, rely upon the willing co-operation of industry. In the case of the chrysotile OEL the original data came from the asbestos industry. Such committees do not select unrealisable target OELs, they choose realistic, achievable OELs.

Finally, as with the ACGIH, given the practical project of IH and the hygiene profession in general it would be very surprising if the BOHS did anything else but set reasonably practicable OEL's.

One final comment needs to be made before the relevance of the BOHS Standards Committee is assessed, and it concerns a point already touched on. The way the hygiene standard documents are worded suggests that the Committee decided on a level of risk that was tolerable and then chose an exposure level which corresponded to this risk level. In reality it is much more likely that the Committee's thought process was the exact opposite of this, and that the level of exposure which was judged to be practicable ultimately determined the level of risk which was judged to be tolerable.

The evidence for this comes from the earlier analysis of the TLV setting processes where the importance of practicability was clear. And it also comes from the BOHS Standard Committee's own work. The tolerable level of risk was set at 1% in the chrysotile OEL set in 1968 and 1% in the wide-based noise standard set in 1971 but in 1972 the tolerable risk level for byssinosis and cotton dust became 4% - Why? The most straight-forward and simple answer is that 2 fibres/cc, 90dB(A) and 0.5mg/m3 (less fly) were judged to be the practicable, realistic levels which industry could meet in time and indeed would be prepared to meet in time. In the case of cotton dust the realistic dust level resulted in a 4% level of grade II byssinosis and so 4% because the tolerable risk level. It is clear from the earlier work of Roach and Schilling that they knew that 100mg/100m^3 was closer to a low risk if not, no-risk level, but they felt impelled "... to adopt a realistic target that could be achieved..." and plumped for 250mg/100m^3. The BOHS Standards Committee felt similarly impelled.

In the case of the wide-based noise standard it would appear that the Standards Committee felt impelled to choose a method of analysis which produced a 1% tolerable risk level. By choosing a relatively unorthodox method of analysis, out of the main stream, this could be done. To have chosen the method of analysis used by Robinson and adopted by the BSI (and later ISO) would have produced risk estimates of around 15%. It would have been difficult for the Committee to argue that this level of risk was "tolerable". The Committee had to set a reasonably practical limit which it could argue protected the hearing of the majority of people exposed and they did so by judicious selection of analytical method. The wide-band noise standard is one of the few examples of standard setting where one can see the effect of the pressure to produce a reasonably practicable standard on the choice of analytical model.

Why are the first four "Hygiene Standards" of the BOHS important?

Firstly, because they were the first UK produced OEL's to be enforced in the UK and to gain international recognition. The 2 fibre standard had a great impact on the ACGIH TLV Committee

and later the OSHA rule making on the Federal asbestos PEL. The 2 fibre OEL was adopted by many countries.

Secondly, because the BOHS made explicit the risk to health of prolonged exposure at the limit. This had been done before in the setting of ionising radiation limits but this was the first time in the field of toxic substances. It was an honest, and in many ways a bold move, it drew a lot of critical fire and it helped start if not prime a prolonged debate on "acceptable risk". It also put the BOHS on the map and demonstrated to the world, and perhaps more importantly to UK practitioners and other professions that IH in the UK had come of age.

Thirdly, in the context of this thesis, because BOHS OEL-setting throws a very revealing light on the underlying process of the ACGIH TLV Committee and vice versa. The BOHS followed in the footsteps of the ACGIH and it felt constrained to set single figure reasonably practicable OEL's. But it choose only to do this for a few substances for which there were at least some dose response data and these data were available to the Committee. It did not attempt, as the West German DFG Commission did, to mimic the TLV Committee. The result is that the BOHS Standards Committee shows clearly, by its explicit hygiene standard documents and method, what the TLV Committee is and has been doing when it sets reasonably practicable OEL's . The earlier analysis including the work of Roach indicated that health effects will and do occur at TLV levels of exposure, and since 1958 the TLV Committee has tacitly admitted this by its use of the phrase "nearly all". The standard setting process of the BOHS meant in the late 1960's and early 1970's that it was possible to put a figure to the phrase which had always been up until this time, nebulous and vague. This quantitative approach was innovative and is the first example of risk assessment applied in the field of toxic substances. The approach was taken up in the USA, especially in the 1980's and has found less favour in the UK and Europe. It started though with the BOHS in the UK.

The Standards Committee stated in its documents that it would review the OEL's within 3-5 years. But, as it turned out this was not to happen. The BOHS did not review the four standards and only set one other OEL. The Society was overtaken by events in the shape of a complete review of health and safety in the UK, a new enabling Act and the formation of the Health and Safety Commission and Executive.

The BOHS OEL-setting episode was important for the IH profession in the UK but more importantly it revealed, by its openness, the compromises inherent in setting reasonably practicable OEL's. The next phase of UK OEL-setting has been neither as open or as coherent .

11.2 The Advisory Committee on Toxic Substances (ACTS)

11.2.1 Early beginnings

In the 1960's in the UK dissatisfaction with the health and safety performance of workplaces, the structure of legislation and the enforcing authorities, led to proposals for new health, safety and welfare legislation.

The Department of Employment produced its first consultative document in December 1967. However it became clear that a more wide ranging and in depth review was needed. In 1970 a "Committee of Inquiry on Safety and Health at Work" was set up under the chairmanship of Lord Robens. It reported in 1972 and its recommendations ultimately gave rise to the Health and Safety at Work Act of 1974 and the creation of a unified inspectorate, the Health and Safety Executive (HSE) answerable to a tripartite body, the Health and Safety Commission (HSC).

The Robens Committee addressed many fundamental questions and received evidence from a variety of individuals and organisations including the BOHS. The society extolled the virtues of TLVs and continued:

"There is a need for a central independent organisation to collect together the existing data ... which would enable standards specifically applicable to the UK to be defined. Organisations such as this Society can help in this work...." GREAT BRITAIN (1972b).

The offer was never taken up, but the Committee did consider not just simply the need for more TLV's but also the general problem of the control of toxic substances. They agreed with the Chief FI remarks in his 1970 report that:

"The proliferation of more subtle hazards ... must (also) be the subject of continuous vigilance" ... (and) ... "Any failure at the present time to bring these risks under control can only therefore be reaped as a bitter harvest, not by us but by the next generation." GREAT BRITAIN (1971).

The Robens Committee then went on to extol the virtues of IH and the ".... rapid development of the FI's Industrial Hygiene Unit since 1966, GREAT BRITAIN (1972b) and their use of TLVs. Having mentioned the need for team work the Committee went on to mention that "... doubts have been expressed about the adequacy of the underpining statutory controls over toxic substances in industry." Ibid. In summary the problem was that, "many toxic substances used in many industrial circumstances are not directly regulated by statutory provision. Further, there is no adequate mechanism for co-ordinating relevant information from industry, the universities and bodies such as the MRC, and for linking this with the regulatory work of government departments." Ibid.

The Committee knew and clearly approved of the actions of the newly created USA OSHA. It mentioned in particular that the Administration must publish an annual list of all known toxic substances and the first such list, compiled by NIOSH, contained 8,000 substances. Also that a Toxic Substances Control Bill was being considered. The Committee concluded that, "There is

wide agreement on the need (for better precautions) ... particularly on the importance of arrangements to ensure early warning of new hazards." Ibid. As to how this could be done the Committee mentions two methods which had been suggested. The MRC wanted a scheme of compulsory testing similar to that introduced in 1968 in the Medicines Act. Whereas the CBI argued that current "testing arrangements" were adequate. The Committee decided "... that the practicalities of the matter point to an approach lying somewhere between these two viewpoints" And having decided that a comprehensive licensing system was not workable they thought "... that there is everything to be said for a comprehensive system of notification to an authoritative body as new substances are brought into use..." Ibid. But where was this "authoritative body"?

After the DoE's first consultative document in 1967 the CBI and the TUC had got together to discuss a range of issues including toxic substances. The CBI had proposed to the Department that "... a Standing Advisory Committee consisting of representatives from both sides of industry" GREAT BRITAIN (1972c) should exist which would produce an "... an authoritative list of dangerous substances" Ibid. According to the Robens Committee Report the TUC and the CBI eventually agreed that "... there should be an independent and expert standing Advisory Committee on Toxic Substances" GREAT BRITAIN (1972b). The Report continues with a description of the Committee and its remit:

"We believe that the creation of an authoritative body of this nature, responsible for giving expert advice on specific problems, on the establishment of authoritative threshold limit values, and on methods of measuring and control, would fill a vacuum in the present arrangements and would help to ensure that new toxic hazards are more likely to be picked up and intensively examined at an early stage before they have been able to do harm. It is our view that the relatively slow progress in the establishment of agreed threshold limit values in this country has been largely due to the lack of an authoritative expert body of this kind with a wide and comprehensive remit." GREAT BRITAIN (1972b).

The idea behind the ACTS had its origins in the late 1960's as part of the discussions between the TUC and the CBI. It is clearly described in the Robens Report and its remit is wide. It is to:

(i) establish "authoritative TLV's"

(ii) establish "authoritative methods of measuring and control".

(iii) to ensure that new toxic hazards are picked up (before they) do harm.

And the ACTS itself is to be authoritative and expert. The Robens Committee expected a lot from ACTS and the new Health and Safety Authority. It is instructive to consider what happened in practice especially in the setting of "authoritative TLVs".

11.2.2 Formation and workings of the ACTS

With the creation of the HSC and HSE various bodies were set up to advise HSC, for instance the Medical Advisory Committee (MAC) was created in 1976. Two additional committees were set up in the following year: the Advisory Committee on Dangerous Substances (ACDS) and the committee the Roben's Report wanted the Advisory Committee on Toxic Substances (ACTS). The first chairperson of the Committee was Audrey Pittom, Director of HSE's Hazardous Substances Division (HSD). The Committee consisted of 14 people, four nominated by the TUC, four by the CBI, two by Local Authorities and four independents selected by HSE. The HSE's HSD serviced and ran the Committee.

The expert, authoritative committee that the Robens Committee had seen the need for was not to be. The consensual, tripartite structure of the HSC had to be reflected in its advisory committees. ACTS is chaired by the Director of HSD and various other people attend meetings as observers or when substances or matters of concern to their government department arise. Regular attendees include representatives from the DHSS, DoI, MoD, DoE, MAFF and usually the laboratory of the Government Chemist.

The official remit of the Committee was:

"To consider and advise the Commission on:

(a) Methods of controlling the health hazards to persons at work and related hazards to the public which may arise from toxic substances..."

In an internal discussion paper one of the tasks of the Committee was to "... advise (HSC) on means of undertaking a more systematic surveillance of toxic and carcinogenic substances, and of instituting a notification and screening system of substances prior to coming into general use."

The official remit is extremely general whereas the internal paper spells out more or less what the Robens Committee had in mind.

The ACTS was not an expert committee and it was not large. It was decided to set up a series of expert Working Groups. The first considered carcinogenic substances, lead, man-made mineral fibres (MMMF), fumigation and vinyl chloride.

Up to 1980 the HSE published the ACGIH TLV list in Guidance Note EH15 and, from 1977, limits set by the ACTS were added to the introduction to the Guidance Note. Four were set between 1977 and 1980 - acrylonitrile (UK = 2ppm, US TLV = A1b), benzene (UK = 10ppm, US TLV = 10pp), 4,4-Methylene-bis-(2 chloroaniline) (UK = 50mg/m^3, USA TLV = 200 mg/m^3 (A2)) and trichloroethylene (UK = 100ppm, USA TLV = 50ppm). The UK OEL's were called "control limits" and were not defined apart from their being limits "... above which personal exposure is considered to be unacceptable and if continued might merit enforcement action" HSE

(1980), and that they were agreed by the HSC on advice from ACTS. After 1980 there was a four year period when no further EH15's were published while ACTS and HSD decided how to set UK OEL's. In 1984 Guidance Note EH40 was published containing 10 control limits and 400+ Recommended Limits. Each year after this a new edition of EH40 was published with additions and alterations to the two types of OEL. By 1988 there was "29" control limits and 500+ Recommended Limits. However even before the EH40 series was started it was clear to the main Committee that there was a need for regular help in the setting of OEL's particularly Recommended Limits which were regarded as less contentious. HSC approached HSE in late 1982 which proposed the setting up of a permanent OEL advisory sub-committee to be called WATCH, the Working Group on the Assessment of Toxic Chemicals, ACTS (1982 and 1983). WATCH itself has advisory satellite working groups and all comprise of people put forward by HSE, TUC and CBI.

11.2.3 Control Limits and Recommended Limits – their history

The definitions of these two limits, taken from EH40, have already been given (see Chapter 10). They are, as definitions strictly functional and give no hint as to the reasoning behind them or why HSE/HSC decided upon a two tier system of OELs, or indeed, where the Recommended Limits came from.

In fact the name and the ideas behind Control Limits came out of the workings of the Advisory Committee on Asbestos (ACA) set up by the HSC the year before ACTS was created. And the ACA itself came out of the need to respond to growing public concern over asbestos and the critical ombudsman's report into the FI's enforcement of the Factories Act and Asbestos Regulations at Acre Mill, GREAT BRITAIN (1976).

The ACA's analysis of OELs and its definition of control limits is important because it throws some light on OEL standard setting in general and corroborates the earlier description and analysis of the working environment and the difficulties this imposes on the setting of single figure OELs. It also tells us something at least about the early thinking of HSE/HSC on OELs.

The ACA argued for a particular name for their OEL as follows:

"As there is no apparent threshold below which exposure to asbestos dust entails no risk to human health, we believe that it is inappropriate to continue to control exposure levels in terms of a 'hygiene standard'. Such a concept is misleading in the case of asbestos as it implies a level of exposure below which exposure is safe. We have therefore sought an alternative term ... (and)... As what we are concerned to achieve is the controlled and progressive limitation of the levels of exposure, we propose that in future the terms 'hygiene standard' should be replaced by 'control limit'." ACA (1979).

The ACA considered three ways of setting a control limit and chose a method where the objective was "... to identify the concentration of asbestos dust in the workplace at which further expenditure of effort to lower the level is out of all proportion to the reductions thereby achieved...." And they continued with the logic of this argument:

"If this approach is applied to the processes where reductions are most difficult, the selected level will be by definition achievable there, and all other parts of industry will be able to do better." Ibid.

However, there is a problem with this approach in that, "... the control limit selected in this way might still be unduly high." The solution was to compare the risk ".... which occurred in industry and commerce ...." and if the risk is ".... so high as to be unjustifiable ... we would have to recommend that these processes be banned." Ibid. And they go on to emphasise an important point: ".... with this approach, the control limit represents the upper limit of permitted exposure, not a 'safe' level such that, once it is attained, no further improvement in dust control is necessary." Ibid. They conclude that their approach, "... although not ideal, represents the best available solution in a difficult situation."

There are other implications in this approach which the ACA flesh out and reconcile to their satisfaction. "It is obvious from the evidence given to us that any control level based on this approach could vary markedly according to the process considered. For example, dust emissions in the manufacture of asbestos cement sheet are much easier to control than in the manufacture of asbestos textiles. It would have been possible to suggest a different control level for each of the many processes. But there are several objections to this approach. First of all it would mean categorising every process. Secondly, **it would suggest that a certain level of risk is more acceptable in some processes than others: an impression which we would not wish to create.**" Ibid. (Author's emphasis).

The ACA recognised the existence of the work environment as described in the last chapter and followed through the logic of their standard setting method. But in a sense they fell at the last post and could not or would not face the reality of their final conclusion. They did not wish to create the impression that a certain level of risk "is more acceptable in some processes than others" but the reality of the workplace is that different levels of exposure from different processes has always been the case. What the ACA could not do was publicly admit the facts of workplace life. They fell back on the old tried and tested professional or governmental ideology of a single figure OEL plus exhortation to get as far below the limit as was reasonably practicable.

The ACA's work overlapped with the formation of the ACTS and its early deliberations. After Audrey Pittom, John Dunster became chairperson of ACTS and he also chaired the committee on Legal and Administrative Controls for the ACA. (Dunster was ideally suited to the task of

considering how to set an OEL for a carcinogen in that he had been Director of the NRPB concerned with standards for ionising radiation. The ACA discussion of risk and how to set an overriding limit bear all the hallmarks of the nuclear industry's approach to limit setting). The thinking of the ACA on the problem of setting an OEL for a substance for which there appeared to be no threshold of effect clearly influenced ACTS. The ACTS used the same name for its first tier OEL and the definition is clearly based on the ACA analysis. However, unlike the ACA, ACTS has never spelt out the logic behind and the implications of its approach to setting control limits. It has though, in the main reserved these limits for substances like asbestos which have no or very low limits of effect.

As to where the initial list of Recommended Limits came from, the HSE never officially revealed their source. But an examination of the list and the draft version of EH40 gives a clue. The draft identifies Recommended Limits, which were described as "Other Exposure Limits" as ".... derived from values in national lists which have received a large measure of acceptance. It is believed in the light of current scientific and medical evidence, that control of exposure within these limits should give adequate protection to the health of most people." HSE (1981). The first Recommended Limits were a shortened and slightly amended version of the TLV list. The last sentence is interesting in that it is a good example of the way governments cannot resist the temptation to increase the status and security of TLVs. They become scientific, medically based OEL's and they protect "most" people and not just "nearly all". In the next draft of EH40 the description and origin of Recommended Limits is deleted and they are described as representing, "good practice and realistic criteria for the control of exposure". Someone in HSE-HSD had a more realistic understanding of the basis and limitations of TLV's. However by omitting the description and origin of Recommended Limits it looked, with the first publication of EH40 in 1984, as if HSE had developed its own UK list of OELs.

But why did HSE divide OEL's into two types; Control Limits, small in number but with significant legal status and Recommended Limits, far greater in number but with less legal status? Again HSE and HSC did not publicly explain why they felt it was important to, nominally at least, cut free of the ACGIH TLV list. Indeed the DoE in its evidence to the Robens Committee saw the TLV list as the starting point for the "advisory body" to which it would add "... new dangerous substances as they come into use." GREAT BRITAIN (1972c). The original idea was that the "advisory body" added to the list, not to trawl backwards and forwards over the old list. The explanation of the two tier, two status list is partly to do with politics, partly to do with the internal workings of HSE and partly to do with the UK legal system.

The political part of the explanation runs as follows: The European Commission Directives on health and safety matters have had a growing impact on the UK. When it came to a discussion of OELs the only country in the EEC which could point to its own list of ostensibly home grown

OELs in the late 1970's was West German. Various countries including the UK started to feel the need to break free of the ACGIH TLV list, (others included Holland and in the mid-1980's France). In negotiations in Brussels each country that wanted a bargaining counter had to be able to point to its own national list. Up to 1980 the UK could not , the West Germans and by that time the Dutch could argue that the UK more or less followed the ACGIH. The UK, in the shape of HSE, wanted its own British OEL list to put on the negotiating table. It could argue that Control Limits certainly came under this category and, if the origins were made obscure enough, perhaps Recommended Limits too.

Apart from the EEC there were internal UK developments to take account of. By the late 1970's there was an uneasy consensus within HSC and HSE for some form of regulation applied specifically to toxic substances. After prolonged drafting and re-drafting the Control of Substances Hazardous to Health (COSHH) regulations are about to become operational (October 1989) but it was clear in the early 1980's that OELs would become incorporated in such regulations and such OELs would be enforceable. If the TLVs were simply incorporated in the new regulations it would mean that the ACGIH officially determined the OEL's set and enforced in the UK. While the UK had in fact followed the ACGIH for two decades the TLVs had only been guidance levels. Incorporation of US OELs as enforceable standards in UK regulations was quite another thing. Publicly at least UK OELs had to be "made in Britain". This offers a good explanation of why the HSE felt a need to, officially at least, break free of the ACGIH. But it goes only part way to explaining the two tier list. This has more to do with the consensus politics which has been practiced by the HSE and FI before it, and the internal workings of HSE and the courts. The tripartism set up as part of the HSW Act is a formalisation of an approach the FI has always encouraged. Improvements had to go forward with the approval of at least some of the more powerful employers in a particular industry. And if the industry was strongly unionised, the union's approval was also sought. From a enforcement point of view the FI had never been satisfied with TLVs. They were guidance levels, as was emphasises in booklet Nº 8 from 1960 onwards and prosecutions were rarely if ever brought simply on the basis of non-compliance with a TLV. They were clearly performance standards and yet they had no teeth.

This was compounded by the way UK courts viewed evidence of harm. Generally, even though an OEL had been exceeded the courts still wanted a doctor to say that peoples' health was likely to be harmed at the OEL. The FI had found that very few Medical Inspectors were prepared to do this. Various explanations have been offered for this behaviour including:

(i) Doctors do not like their judgement being questioned.

(ii) Fear that their general knowledge on a topic would be undermined by a medical specialist called by the plaintiff.

(iii) Related to (ii) Deference and even fear of their medical peers.

(iv) A professional aloofness which results in a lack of commitment to the enforcement needs of the organisation.

(v) An uncertainty and even distaste for OEL's (clearly evident in the 1950's and 1960's).

(vii) The difficulty of presenting hea;th issues in court in black and white terms.

Whatever the reasons, their reluctance to appear in court meant that few prosecutions could be brought on the basis of the non-compliance with TLVs. By the 1970's the FI and the Specialist Inspectors within HSE wanted standards which they could enforce. Tripartite agreed control limits were a way round this problem. These OELs were agreed in a negotiation process involving the CBI, TUC and HSE and were more or less mandatory. They had greater force in law than the TLVs. Also, HSD had got an agreement from the Employment Medical Advisory Service (EMAS) that they would give evidence in court that a health hazard did exist if Control Limits were exceeded. Neither the CBI or probably EMAS would agree to the wholesale conversion of TLVs into Control Limits. And so HSE got some of what it wanted, in the shape of enforceable OELs but it was at a price, perhaps in retrospect, too heavy a price. Because the fact is that there is no fundamental difference between control limits and recommended limits, they are both sets of reasonably practicable OELs. Control limits are in the main concerned with carcinogens or substances with effects at low exposure levels, but Recommended Limits contain a fair sprinkling of such substances. Control limits are decided upon by a lengthy review and tripartite negotiation procedure. Recommended limits, being basically TLVs are arrived at via a somewhat different process, an expert committee of a professional organisation, but the result is similar if not the same. As was shown in the last chapter there is a tendency for the ACGIH to pitch its definition of reasonably practicable at the lower end of the spectrum compared to the ACTS, but they overlap and are not poles apart.

The conclusion is that the two tier UK system did not evolve out of any fundamental analysis of OELs or toxicology - it is the product of a series of pragmatic policy moves designed to ameliorate short term problems that HSE faced in the late 1970's and 1980's. However the problem did not stop with the creation of a two tier system of OELs. It became compounded as HSE sought to justify the system by claiming there were important, and in some ways fundamental, differences between Control and Recommended Limits But in reality no such differences exist, however unfortunately HSE decided to build on their two tier system and worse, as will be shown was to come.

Quite how such pragmatism could come to rule the policy roost is examined later. What is of more immediate interest in the present context is the working of the ACTS and the process by which Control Limits are set.

11.2.4 The standard setting process of the ACTS

No attempt will be made to examine comprehensively all the OELs that the ACTS has set or sanctioned. The general process of the Committee will be examined by considering the development of the Control Limit for Styrene and, where appropriate, adding the insight gained from an analysis of the internal dynamics of the Committee (see Appendix 6).

HSE published the first edition of Guidance Note EH40 in 1984. Under the heading "Control Limits" 10 substances or families of substances were listed together with 12 OEL's (asbestos had 3), HSE (1984). Various changes were made to the Control Limits and other OEL's listed in the preface to the last TLV list published, HSE (1980): one control limit was downgraded to a Recommended Limit (Benzene) and three other OEL's ratified by ACTS were also converted into Recommended Limits (chromium and compounds, MbOCA and perchloroethylene). In the period between 1980 and 1984 the ACTS produced 4 additional Control Limits (carbon disulphide, ethyleneoxide, isocyanates and styrene). Table 11.2 charts the number of Control Limits set or notified by ACTS from 1980 to 1988. If one takes the starting date for Control Limit production as 1980, the average number produced per year has been ~3, but if the start date is 1984 the rate rises to ~ 5 per year. Whatever date is chosen one conclusion that is immediately obvious is that the rate of production of Control Limits is low.

Table 11.2 Number of Control Limits*

Date	*Number*	*Increase*
1980	8**	–
1984	12	4
1985	20	8
1986	25	5
1987	32	7
1988	34	2
1989	(34)***	0

* Control Limits set, the number of substances is less than this number

** Includes benzene which was downgraded to a Recommended Limit in 1984. Excludes, chromium, MbOCA and perchloroethylene which were agreed by the ACTS but never called Control Limits.

*** Control Limits are renamed Maximum Exposure Limits (MEL).

The setting of the Control Limit for styrene gives a good illustration of the Committee's method. But before this is considered the TLV background should be described:

Table 11.3 Development of ACGIH Styrene TLV

1946*	400ppm
1947	200ppm
1957	100ppm
1964	100 ppm made a Ceiling value.
1967	50ppm (NIC)
1968	50ppm (NIC)
1969	100ppm
1971	100ppm Ceiling designation deleted
1979	50ppm listing under Notice of Intended Change (NIC)
1981	50ppm adopted with a STEL of 100ppm

* Although Cook, the author of the paper from which the ACGIH took many TLV's warned that: "Experience has already suggested that the concentration causes sufficient irritation under actual working conditions to revise this standard downwards. Until broader experience is available, it is suggested that concentrations be not allowed to exceed half the tentative value of 400ppm", COOK (1945). A year later the ACGIH dropped the TLV to 200ppm.

The 1967-1969 episode requires some comment. Baier proposed a reduction to 50ppm in April 1967 and in November 1968 a decision was deferred but a note from Stokinger in the Minutes suggested: "Amplify neurologic impairment statement" ACGIH (November 1968). By 1969 the TLV Committee had swung back to 100ppm due to Caplan's evidence: ".... based on Californian IH experiences, recommended that level be established at 100ppm." ACGIH (April 1969), It is not clear whether Caplan's "experience" consisted of lack of reported problems from exposed workers or knowledge of how difficult it was to attain 50ppm. Given the earlier discussion of the meaning of a hygienists experience, and the evidence from the ACTS process which follows, it seems likely that it was the latter.

Styrene is a liquid at room temperature with an aromatic smell. It is used to make polystyrene plastics, protective coatings, styrenated polyesters, copolymer resins and as a chemical intermediate. The NIOSH criteria Document summarises the human health effects as follows:

"The most frequently reported effects of exposure at about 100ppm are subjective symptoms such as fatigue, dizziness, headache, nausea, poor memory, and drowsiness... chromosome changes occurred with greater frequency in the lymphocytes of workers exposed to styrene at about 100ppm. Although the evidence is not strong, exposure to styrene has also been implicated with other adverse health effects such as peripheral neuropathy, abnormal pulmonary function, liver toxicity, teratogenicity, and carcinogenicity." NIOSH (1983).

Apart from the ACGIH, West Germany had a MAK of 100ppm, Sweden lowered its to TLV from 50ppm to 25ppm in 1981 and NIOSH recommended a REL of 50ppm in 1983.

The chronological development of the UK styrene Control Limit was as follows:

Styrene was raised as an issue on the ACTS early in its life. The ACGIH (NIC) in 1979 and the drawn out NIOSH review process which started in the mid 1970's must have been a stimulus to action.

1981: A Toxicity Review (the first in a new series) on styrene was submitted to the ACTS and published a while later, HSE (1981b). The paper was introduced by an HSE doctor. He pointed out that apart from the well known irritant effects there was evidence of possible behavioural effects and brought the Committee's attention to ACGIH's TLV reduction to 50ppm ".... which members may consider to be an appropriate level for a Control Limit." ACTS (1981).

1982: At the next meeting considerable discussion took place. A TUC speaker said: "he considered that the existing exposure limit did not allow a sufficient margin of safety" ACTS (1982) and later his colleague pointed out that Sweden was considering a reduction to 25ppm. A CBI speaker, ".... said that a reduced limit of 50ppm could be achieved in most of industry but explained that in certain industries, eg. glass fibre boat building, where work was performed within closed hulls, the existing limit of 100ppm was already very difficult to achieve ... it would not be practical to reduce the limit" Ibid. The TUC speaker, "... was reluctant to accept that an improved standard should be set aside because of problems in one small sector of a specialist industry". The HSE chairman replied "... that HSE would not be willing to reduce the limit to 50ppm and at the same time allow exemption for the glass fibre boat building industry as it was not their policy to permit exemptions from general control limits." Ibid. It was agreed that HSE would collate information on exposure in boat building and the CBI was asked to supply any data its members had.

At the next meeting the HSE reported that the CBI "... indicated that the problem was wider than boat building" and exposure data should be ready for the next meeting, ACTS (1982b).

At the next meeting the exposure data were still not available. The TUC reiterated that the Swedish TLV had been reduced to 25ppm and one of the independents countered that the WHO had just agreed on 100ppm.

1983: An HSE speaker introduced a Supplementary paper on exposure levels in the glass reinforced plastic (GRP) industry. The paper concluded that, "(excluding the GRP industry) there is no evidence that the current TLV and STEL are exceeded other than in very exceptional circumstances. Typical exposure levels are well below the current TLV." ACTS (1983b). In the section on control of exposure the report concluded that "... the correct application of conventional occupational hygiene techniques to the GRP process can reduce exposure levels to well below the present TLV in most circumstances. Even where GRP is fabricated in comparatively confined spaces, the application of properly designed ventilation equipment can reduce styrene levels

significantly." Ibid. However there were some processes in inaccessible confined spaces where ventilation was judged to be impossible and respiratory protection would be needed. The industry estimated it would cost £1,000 for employers in a medium sized firm to reduce exposure to 50ppm (8 hour TWA). HSE, "... suggested that the control limit should be set at 100ppm, 8 hour TWA, for a period of two years and then reduced to a level of 80ppm, 8 hour TWA. The deferred introduction of the 80ppm limit would enable users .... to introduce new work practices and modified control measures so as to achieve the reduced limit." ibid.

One independent was not in favour of the reduction to 80ppm after 2 years and another wanted 50ppm as a Recommended Limit after three years. After a discussion, the contents of which are not recorded, the ACTS agreed on a Control Limit of 100ppm, 8 hour TWA; "... but it should be made clear that experience showed it to be reasonably practicable in most sectors of industry to reduce exposures to 50ppm, 8 hour TWA and that the latter should be accepted as a target to be achieved." Ibid. They also resolved ".... to review the Control Limit when further epidemiological evidence became available from current MRC and EMAS studies in two or three years time." ibid.

At the next meeting the ACTS considered setting a STEL for styrene. The Supplementary Report had outlined the position: "Even in circumstances where the 8 hour TWA exposure levels are well controlled, peak exposures in excess of the current STEL (125ppm, 15 minutes TWA) can be found. It may not be practicable to introduce engineering control measures or revised working procedures so as to reduce short term exposures to below the current STEL." ACTS (1983b). At the meeting HSE tabled a proposal that the STEL should be 250ppm (10 minute TWA). The independent and the CBI agreed. A TUC speaker had some reservations. "It was agreed to recommend to the Commission a two-part control limit comprising a 100ppm 8 hour TWA and 250ppm 10 minute TWA". ACTS (1983c). At the next meeting it was reported that the HSC would consider the styrene Control Limit in October, ACTS (1983d). Styrene appeared in the first edition of EH40 in 1984, HSE (1984).

After three years of deliberation the ACTS set an OEL of the same value as the TLV which the ACGIH Committee selected in 1957, twenty-seven years earlier. What factors pinned these two OEL standard setting committees to the same number?

From the earlier analysis of the process of the ACGIH together with the model of the work environment certain constraints on the TLV Committee were inferred – TLVs had to be reasonably practicable for "Type 1" processes (see Figure 9.4). The BOHS Hygiene Standards Committee mentions costs and practicability explicitly though no processes are identified. In the setting of the styrene Control Limit by the ACTS the constraints of a certain process are crystal clear. Work in confined or enclosed spaces in GRP boat building determined that the limit would be between 50-

100ppm. For styrene, boat building was the "Type 1" process and within this process enclosed space work was the job which caused greatest exposure and was the hardest to control by process design or engineering means. And the evidence that the health effects of styrene should be treated more seriously, though it had increased in amount since the 1960's when Stokinger felt that "neurologic impairment" should be emphasised, was still not clear cut or overwhelming. None of the parties on the ACTS made great play of the health effects though the TUC did call for more research into its long-term effects. The HSE Toxicity Review came to similar conclusions as the NIOSH Criteria document though it tended to play down the evidence on carcinogenicity and teratogenicity. With no strong evidence of health effects the Committee was almost cut loose from considering risk and focussed instead on practicability, and the practicability issues had not changed in a major way since the ACGIH had set its TLV of 100ppm almost three decades earlier. The question hinged on how much effort and money industry was prepared to allocate to controlling exposure in the boat building industry. Evidence published by research workers in the US corroborate the HSE's analysis: 50ppm was possible but difficult to achieve in enclosed spaces and almost all the industry were working below 100ppm, LEMASTERS et al (1985) and TODD (1985). The Swedish analysis differed on this point and argued that 25ppm was possible except for enclosed space work for which respiratory protection would be necessary, SWEF (1984). The spectrum of practicability ranged from 25ppm at the stringent end to 100ppm at the more lenient end. The UK and West Germany chose the latter end while NIOSH chose somewhere close to the middle of the spectrum and Sweden chose the stringent end. Whichever level was chosen they were all practicable with greater or lesser effort and cost. In the analysis of the TLV Committee's process the question of the anchoring effect of other OEL committees was considered, but it was concluded that practicability was a far stronger anchoring force. While ACGIH's decision to lower the styrene TLV to 50ppm put pressure on the ACTS, of greater significance ultimately has been the judgement on practicability and cost. The ACTS process would appear to confirm the earlier conclusion. If an OEL setting committee sets a limit in the face of uncertainty over the health effects of a substance the issue of practicability rules the process. If the evidence on health effects is more certain than the anchor of practicability is still the most weighty factor but the argument over where it lies in terms of the spectrum of possible reasonably practicable OEL's becomes fiercer.

The setting of the styrene Control Limit says quite a lot about the process of the ACTS and adds a bit of detail to the more general analysis in Appendix 6. The analysis shows that overall the TUC ask most of the questions, the HSE answers almost all the queries raised by any of the parties on the Committee and also makes most of the formal proposals and by far and away the majority of proposals for OELs. In the case of styrene it was HSE that provided the analysis of what was practicable, the assessment of health effects, the first proposal for 50ppm, the second proposal for 100ppm with a reduction to 80ppm later and ruled that it was not possible to exempt parts of

industry from a limit. The CBI provided an estimate of costs and argued with the TUC over where the practicable limit should be set.

The HSE are constrained as to where they judge the spectrum of practicability to lie. They cannot choose an arbitrary region of possible OELs and in fact tend to do more than just simply present an analysis of exposure data and control possibilities. Before the value is recommended to the Committee the industries using or involving a particular substance are usually canvassed. The value recommended is usually pitched at a level which is being attained in a significant part of the well controlled end of these industry or industries. In this sense assessment and manoeuvre take place off and on the Committee.

As much as any one Control Limit can be described as typical styrene illustrates the general pattern of the ACTS behaviour well. Finally the decision to set a limit of 100ppm while emphasising that 50ppm was attainable in much of industry was almost straight out of the ACA's recommended approach to setting control limits.

11.2.5 Recommended Limits

Apart from Control Limits the ACTS as has already been mentioned sets or agrees Recommended Limits. The first list published in 1984 was based explicitly on the TLV list. Since then the WATCH has been set up and with its various working groups makes the vast majority of suggestions for Recommended Limits. These, in the main, do not differ significantly from ACGIH TLV's in value. Occasionally the ACTS will decide out of its own deliberations to set a Recommended Limit, thus for instance it set an OEL for wood dust of 5mg/m3 but the value for hard wood was made a Control Limit and the value for soft wood was made a Recommended Limit. Presumably, by this decision, the Committee meant to signal that it did not regard soft wood dust as posing as much of a carcinogenic hazard as hard wood dust and therefore enforcement of the OEL would not be so strict.

Before embarking on the discussion of IH and OEL's in the UK one recent development in the current two tier OEL system needs to be described.

11.3 The transmutation of Control and Recommended Limits

"Dissatisfaction has been expressed over definitions and associated concepts for both Control and Recommended Limits as stated in Guidance Note EH40. A joint attempt is now being made by HSC, the Trades Unions and industry to clarify and simplify these concepts and definitions." WINTERBOTTOM (1987).

"A two tier system of MEL's and OES's .... is both confusing to persons without experience, impracticable to apply - even by trained and experienced persons - and open to differing interpretation by management and union representatives." Joint Occupational Hygiene Standards Committee, BOHS and IOH (1987).

In October 1988 the Control of Substance Hazardous to Health (COSHH) Regulations were laid before Parliament HSE (1988). They contained in Regulation 7 references to two new types of OEL, Maximum Exposure Limits (MEL's) and Occupational Exposure Standards (OES's). In Guidance Note EH40 published the following year these limits were defined:

MELs – "A MEL is the maximum concentration of an airborne substance, averaged over a reference period, to which employees may be exposed by inhalation under any circumstances...." HSE (1989).

Later this absolute duty is relaxed for under Regulation 10 of COSHH the employer only has to show "... that the MEL is not normally exceeded".

The degree to which MEL's protect health is not stated but there is a duty as with Control Limits to reduce exposure below the MEL as far as is reasonably practicable. In the case of short-term MEL's however there is no doubt ".... the purpose of limits of this kind is to render insignificant the risks to health." Ibid.

OESs – "An OES is the concentration of an airborne substance, averaged over a reference period, at which, according to current knowledge, there is no evidence that it is likely to be injurious to employees..."

For a substance which has been assigned an OES, exposure by inhalation should be reduced to that standard. However, if exposure ... exceeded the OES, control will still be deemed to be adequate provided that the employer has identified why the OES has been exceeded **and** is taking appropriate steps to comply with the OES as soon as is reasonably practicable." Ibid.

The OES definition is a break with HSE's and FI's, and indeed the ACGIH's, tradition of requiring further effort, as far as is reasonably practicable, to apply more control even though the OEL level has been reached. Does the HSE believe it can and has identified 'safe' OELs? It would seem from the definition that it does but in the next paragraph OESs are qualified, ".... it is not intended that the statutory requirements under Regulation 7(5) should discourage the further application of good occupational hygiene principles in order to reduce exposure below the OES." Ibid.

The HSE does not seem to be able to make up its mind. One minute it can identify 'safe' levels of exposure and the next it is recommending that the employer should try to get exposure below these

no risk levels. Taking the first definition at face value there is no need to do so. How did these OEL's come about and what relation do they have to Control and Recommended Limits (CLs and RLs)?

Discussion on the ACTS and within HSE on the need for a change in the definition and status of Control and Recommended Limits started in 1985, stimulated by the publication of the draft COSHH Regulations in 1984. The OELs it became clear, were going to become central to the interpretation of COSHH and for various reasons certain parties, in particular the CBI and HSE were not satisfied with the current system. Even with access to a certain proportion of the discussion documents it is not at all clear how CLs and RLs became transmuted into MELs and OESs, though the evidence does show how persistent early ideas and misconceptions can be.

In late 1985 a draft document on OELs was circulated for comment, it contained some insights and several significant misconceptions. It starts with the "aims of a policy for the control of workplace exposure." It should be:

"Clearly understandable and logically defensible. Consistent, both within itself, ie limits are comparable with each other; and with other international standards, particularly those in Europe. Capable of practicable implementation and result in enforcible limits." HSE (1985b).

The document was a draft and contains some contradictory statements however it does reveal some of the early thinking behind MEL's and OES's. Points made in the document, relevant to this research are listed below:

1. CLs and RLs ".... have no direct legal status or distinction from each other..." Ibid.

2. "The reference to 'detailed considerations of the available scientific and medical evidence, (part of the CL definition) has been construed by many, particularly the CBI .... to imply that there must be clearly demonstrable ill-health effects before a control limit can be set." Ibid.

3. On the question of RLs the author was confused: ".... most (RL's) are set having regard to the effects of exposure to the substance, not in terms of what can be achieved. Most are adopted from the ACGIH and have no direct relationship to what is reasonably practicable in British Industry .... Most of these RL's have been set following consideration of toxicological and medical effects only. The levels are usually well below those which have been associated with harmful effects, either directly in volunteers or by extrapolation from tests on animals." Ibid.

But, a little further on the following statement is made: "A fundamental point about both types of limit is that they are based on a consideration of what is reasonably practicable." Ibid.

4. As this is the case it presents the author with a problem: "The concept of reasonable practicability is one which is not used explicitly by other countries in setting their limits..." Ibid.

5. The author summarises the problem:

Firstly: "The UK control limit attempts with a single figure to meet at least two distinct and possibly conflicting criteria, viz: a level above which nobody should be exposed .... and a level judged to be reasonably practicable....

Secondly: "Because of their relationship to process control within industry the limit numbers are not directly comparable with each other, creating very serious problems for any policy on the control of mixtures."

Thirdly: "The concept of reasonable practicability is not used explicitly in setting maximum exposure limits elsewhere." Ibid.

6. Various proposals are made:

(i) "The acknowledgement that the requirement of 'reasonably practicability' applies to separate individual uses, processes etc., and not overall to a substance, is the first step in any new policy for the control of hazardous substances." Later this point is amplified: "For many substances we have a lot of information on UK use. We understand the process and can say something about what is good practice. Therefore we can say what action should be taken and can issue guidance - on a process by process, or industry by industry basis - of what is reasonably practicable." Ibid.

(ii) On the question of what industry wanted: "... many industrialists would like to see a single number limit: above the number is an offence and below it there is no requirement to reduce exposure. This concept is based on the fallacy that there is a clear demarcation between risk and no risk. This is not the case. For the great majority of substances there is inadequate knowledge for us to be very precise about the level at which any effect on health occurs and, when it does, what is its significance. Thus, there can be no single "go - no go" number." Ibid.

(iii) To produce OEL's for mixtures ".... we must be able to compare and argue the merits of the numbers internationally. To achieve this the numbers can not be based on current UK control practices: they must be based on internationally accepted criteria which are similar for each substance: this means they must be based on health effects. The likely onset of adverse health effects provides the only bench mark for true compatibility." Ibid. This level is not seen as a no-risk level ".... but the risks appear to merge with, and are comparable to, the general risks to which all workers are routinely exposed." Ibid. A dubious form of comparison discussed in the last part of the thesis.

(iv) He proposes that, "all substance in EH40, including those currently with Control Limits, should be considered by the WATCH panel and a health based action level set for each ..... scientifically based criteria for health protection, rather than technical or economic feasibility, should be employed." Ibid.

Again, as with the BOHS Hygiene Standards and the discussion of "Control Limits" by the ACA the draft OEL document threw light on TLVs which in turn reflects back on the ideas and confusions in the document.

The first statement on the aims of policy threw into sharp relief the contradictions. The writer wants the best of all worlds:

- An understandable and logical policy
- Internationally comparable limits.
- Enforceable limits

He understands some of the problems, for instance, that clearly demonstrable health effects cannot be the only acceptable evidence of harm and precise "go - no go" numbers are a chimera. The point about reasonable practicability and the fact that other countries do not admit explicitly that their OELs are set in a similar way (ie., balancing risks and costs) is well made. However he is clearly confused over the status of TLVs: At one point they are set solely on the basis of scientific and medical evidence with no consideration of practicability and the next they are reasonably practicable. Finally, in the first proposal (5 (i)) the author comes close to breaking with the idea of a single figure OEL. A point that the ACA approached but then shied away from.

Apart from these specific comments there is one general observation which is pertinent and that concerns the content and context of the draft document: It is written in an a historical vacuum. Nowhere is the work or views of other authors in the UK or other countries referred to. This leads the author to make one original observation concerning the setting of OELs for mixtures based on individual reasonably practicable OELs. But in the main the author's naivety leads to some

dangerous confusions especially as regards the status of the TLV. Although it has not been stated badly in the literature that TLVs are reasonably practicable OEL's it was certainly recognised by IHs and others in the UK, that they were set on more than simply the "scientific and medical evidence". The author being insulated from this 'community knowledge'" while confused clearly believed that many, if not all, TLVs were set at "... levels .... well below those which have been associated with harmful effects." And although he chides industry for wanting "go - no go" number his utopian view of TLVs became married with what "many industrialists would like to see."

Although the draft was discussed by the ACTS Working Group on OELs and many objections were made very few changes were in fact made to the original draft and much of it appears in a paper presented at a BOHS Annual conference in 1987, ROLT (1987). There are some differences between the draft document and the paper. The main ones of interest in this context concern a diagram (see Figure 11.1), which originally appeared in one of the CBI responses to the earlier draft and the conclusions.

"The most notable aspects of the proposals I have outlined are the establishment of an "effect level" below which no further legal requirements for the general reduction of exposure would be applied, to separate the concept of "reasonable practicability" from overall standards setting and to apply it to particular processes/uses only; and to set a maximum exposure level for some substances." Ibid.

Having argued in the draft that "go - no go" levels were a fallacy Rolt appears to have come round to the idea and earlier appears to agree with a proposal discussed in the last part of this thesis and circulated in September 1986 by the author, PINEY (1986).

"In an ideal world there would be a top and bottom limit for every substance and extensive guidance for particular sections of industry." Ibid.

However after his three part proposal Rolt shows his confusion over the status of TLVs and MAKs:

"This proposal may at first sight seem extremely complicated, but in practice our present limits, which are set at the level of, and based on the same data as, the ACGIH or MAK levels, are in fact the "effect level". I have been referring to. Similarly, the concept of the level that should not be exceeded is the popular understanding of what a Control Limit is". Ibid.

Figure 11.1 (Taken from ROLT (1987))

The confusion is more or less total: Control Limits are to become renamed as MELs and their definition remains approximately the same as that first defined by the ACA. And Recommended Limits become renamed as OESs and this definition becomes transformed. They are no longer TLVs; reasonably practicable OELs but have become transmuted into OELs, based on health effects. It is almost as if HSE attempted to solve the difficult problem of setting Health Based Limits (HBLs) by waiving a magic wand over TLVs and redefine them, having changed their name first to Recommended Limits, as HBLs. At one swish of the policy wand the problem is solved.

What actually happened to Rolt's proposals was in some ways even worse. His first and third proposals were actioned and the second was lost from sight. But first to compound the problem the transmutation of RLs was only partially done. The majority were simply given a new name, OES, but the number, in most cases the TLV number, remained the same. With others, 65 in all, it was decided, on what basis has never been made public, that the numerical limit needed to be reviewed. Not all RLs (TLVs) were set on the "scientific and medical evidence" at the "effect level" it would seem. This is the first time in the history of factory legislation in the UK, since the duty to control dusty or offensive processes was first incorporated in 1864, that employers have been given standards which if met, absolve them of any further responsibility or duty to improve conditions. Indeed some within HSE were clearly unhappy at the OES definition and this probably explains the rather incongruous plea to apply ".... good occupational hygiene principles" at the end of the definition.

What were the immediate reasons which led to MELs and the OESs? As with the selection of RLs for review, no explanation has been given, it simply 'happened'. However, educated guesses are possible.

From the late 1970's HSE hankered after enforcible statutory OELs. Control Limits were their first attempt and the price was the two tier division into CLs and RLs. But CLs still had an ambivalent status in law and although EMAS had promised to support prosecutions if the CL was exceeded the agreement had not been put to the test. MELs were the solution to the enforcement officers prayer - there is almost an absolute duty to abide by them. In theory at least there should be no discussion in court on the merits of the standard and no doubt that an offence has been committed - only time and case law will tell. But there was a price to pay for these statutory limits and that was the "go - no go" OESs. Quite who was pushing for them is not clear. The CBI was certainly split, one group proposed the system depicted in Figure 11.1 while another wanted "walk away" OELs and at least one of the CBI appointees on the ACTS supported the HSW Act approach:

"Industry believes that the already established "reasonably practicable" approach, for which a body of case law already exists, is .... suitable." WINTERBOTTOM (1987). As the CBI did not apparently speak with one voice this perhaps left the HSE room to pick the options it liked. And quite what the TUC's attitude was is not known - no argued case for or against MELs and OESs was ever circulated. It is tempting, but wrong, to conclude that OESs owe their origins to misconceptions in the minds of a few individuals in HSE's Policy Division, who somehow convinced themselves that TLVs were health based and that defining them as such was a price worth paying for MELs.

HSE Policy Division hoped to transmute the leaden reasonably practicable OELs that were RLs (TLVs) into the gold of health-based OELs simply by use of the pen and the printed word. Transmutation was the holy grail of the alchemists which was never realised, it was a forlorn hope in our more scientific, paradigmatic times. Base metal can only be transmuted by cosmological forces which are outside the remit of HSEs Policy Division. The results of the name and definition changes is an unworkable, paralysed and indefensible two tier,three list set of standards. It is not at all clear how HSE and the ACTS will extricate themselves from the mess, which in large part is of their own making. What is clear though that the decision and policy making process is not simply the responsibility of a few individuals.

11.4 IH and OEL's in the UK – Discussion

Compared with the USA IH developed late in the UK. Once it started it was slow to develop and is still not as embedded in industry and government as is IH in the US. This is not to say that

some original and influential work has not been done in the field of IH in the UK. The early work of the FI Ventilation Committee or the observations and analysis of Legge and Duckering on lead exposure and absorption were firsts in the field. And later the work of research associations such as the BCRA and BCIRA on dust control was thorough and elegant and the work of the latter, on pedestal grinders was referred to approvingly by the ACGIH. However, the pattern of IH research tended to be a glorious saga of one-off's - work done by one or two individuals based in a variety of institutions and organisations. In the context of this thesis the BOHS OEL setting episode fits firmly into this mould. However, despite the small number of OELs they had a significant impact in the UK and in many other countries and raised issues which were not overtly addressed by other committees.

A variety of reasons for the late, slow and halting development of IH in the UK have been identified. The most significant was the existence of the FI, the very traditional division of labour within the organisation and its resistance to IH concepts and practice and in particular its resistance to the need for a IH profession. The very name that the early IH inspectors were given, a name that continued until very recently gives an indication of how narrowly IH was viewed and still is viewed by many: IH inspectors were called "Chemical Inspectors". They might make recommendations on control but strictly this was the province of the Engineering Inspector. And again, although many did make assessments and become involved in recognition of potential health hazards this was, and is, strictly the territory of the doctors. The traditional and powerful professions have always restricted the definition of IH and tended to reduce it to the assessment and measurement of chemical exposure. The broader definition encompassing physical hazards and stresses and spreading into ergonomics, for reasons already discussed, has never existed in the FI or HSE. Thus, as noise is a physical effect caused by physical processes it is the province of physics. Most if not all Noise and Vibration Specialist Inspectors are physicists and not a part of IH - the ghost of Kenneth Goodall still haunts the HSE.

IH in the HSE is not practised or defined in the same way as in the USA or indeed as in the private sector in the UK. It has always been small and of relatively low status. It is no accident that the major innovations in OELs came from a professional body outside of the HSE - the BOHS. This is in stark contrast to the ACGIH which developed directly out of the experience and needs of the US PHS, a Federal governmental body.

With no significant and powerful professional to define and defend the OEL paradigm and promulgate the use of OELs they have become distorted. After all there were good reasons for believing the "British Way" worked. Control of air contaminants emitted by processes or machines, through enforcement of Section 47 or Section 63 of the 1937 or 1961 Factories Acts, and similar provisions before them did appear to have been relatively successful, at least as far as the FI reportable disease and the DHSS compensation statistics went. This process based view

became incorporated in the development of ICI OEL's and the early FI use of TLV's – both ICI and the FI saw TLVs as "design limits" for processes, as design benchmarks. Even so the introduction of TLVs into the FI in 1960 in an official publication was done by IHs or people with a basic allegiance to the profession. Although the definition was tinkered with and confused, as described earlier, IHs in the FI remained relatively faithful to the OEL paradigm. And as the FI began to use them they found that TLVs were in fact reasonably practicable. Where processes had been controlled by use of Section 47 or 63 and the Chemical Inspector judged the process well controlled, the TLV was not exceeded. The PHS as much as the FI wanted enforcible limits, not a target that meant that a significant proportion of industry could not meet the standard. As argued in Part 4 TLVs were produced for and used by a variety of groups but the principal customers were the state IH organisations. It is not surprising, given this context, that reasonably practicable OELs were also to the liking of another enforcement organisation – the UK FI.

11.4.1 The OEL paradigm in the UK

For a science to be universal, including an applied science such as IH, the groups that practice that science and interpret and extend it also have to be universal. This point was argued in Part Three and, in the context of the acceptance and use of the OEL paradigm and TLVs was mentioned in Part Four when the international influence of TLV's was discussed. In this discussion we saw that whether TLV's were accepted depended very much on whether there was an IH group, particularly in government, or an equivalent professional grouping. Where there was none, such as in France, TLVs in particular and OELs in general did not find favour. And now that the development of IH in both the USA and the UK have been considered a little additional detail can be added to the question first raised in Part 2.0 and discussed in Chapter 9.0: Is the OEL paradigm used in other countries the same as that promulgated by the ACGIH? In the UK the answer, up until recently, would have been - yes; though, as has been discussed, the FI bent the definition of TLVs towards its needs and the BOHS extended it. Overall the IH profession in the UK has remained faithful to the paradigm. However, more recently with the advent of HSE, HSC and the ACTS OEL standard setting in the UK has been steadily decoupled from the original IH OEL paradigm. This decoupling is closely correlated with the erosion of IH's influence on OEL setting. As long as the UK followed the ACGIH TLVs the lack of IH power within HSE did not matter greatly. As soon as HSE decided, for the reasons already discussed, to develop its own OELs this IH power vacuum became important and obvious. With no coherent profession to supervise the interpretation of the OEL paradigm HSE's policy makers together with the tripartite parties were cut adrift from a coherent or at least self-consistent philosophy and quickly drifted into strange waters. The discussion of the development of the MELs and OESs showed that the break with the paradigm was not complete and the de novo thinking did reveal some original insights -but in the main the decoupling has been a disaster. The formation of the ACTS accelerated the decoupling process and led to what can best be described as a pick 'n' mix approach to policy concept

formation; very few if any of the concepts found in the draft EH40 (named EH00) or the current EH40 were or are original - most have been plagiarised from other groups or countries. For instance:

(i) Control Limits - the name and concept came from the ACA.
(ii) Skin notation - came directly from the ACGIH.
(iii) Mixed exposure - came directly from the ACGIH.
(iv) Short-Term Exposure Limits - came directly from the ACGIH.
(v) Allergy notation - in the original EH00 draft, is an idea first used by the West German DFG and introduced in the early 1970's.
(vi) Reasonably practicable time to comply - the idea of a time limit to comply with an OEL is referred to by Rolt and appears to be a direct lift from the EC Lead Directive of 1982.

How did this happen? Some of the reasons have already been mentioned. The tripartite process has little or no allegiance to, and indeed some antipathy towards professional paradigms. And this was coupled to an HSE Division with almost no allegiance to the OEL paradigm, staffed in the main by people from a variety of professions many of whom were passing through on a 2-3 year cycle and did not in practice either have to live with or defend their decisions. This is also the point at which to reconsider the remit that the IH profession has written itself. It has tended to have and to project a technocratic self-image and fought shy of involving itself in more general social issues and policy formation. It has painted itself into a professional - technical corner in which "real" policy makers in the civil service and the HSE are only too happy to leave it. Even so one is forced back to the point that the debacle described would not have occurred, or would have been strongly mitigated if IH had had significant power and status within and without HSE. And here we return to the factors described in Chapter 10.0, the late, slow and weak development of IH, particularly in the FI. From this perspective the current debacle has its roots in a long historical process stretching back to the very earliest days of the introduction of applied scientific professionalism into the FI at the turn of the century, and certainly as far back as the creation of an FI analytical laboratory in the 1940's.

As to the question of why HSE got itself into the position now the answer lies partly in short-term policy and political needs as perceived by HSE, but also because of wider political changes which have occurred within the lifetime of the ACTS. The Committee was born during a period of consensual politics where the belief was that the best decisions were taken and implemented by negotiation and compromise between the social partners usually in the shape of employer and trade union organisations. The prevailing political ideology in the 1980's has been either directly hostile or indifferent to such ideas. And this, coupled with an attack on the power of the trade unions has, in a very fundamental way, undermined the raison d'etre of the process of the ACTS. The tripartite structure of the HSC and the ACTS a 1970's anachronism in the light of contemporary

dominant political thought. That it survives says something about the staying power of institutional arrangements and perhaps it also serves a continuing political function as a projected image. However it has not survived unchanged. This is evident in the discussion of MELs and OESs where the TUC had very little input to the debate and from the textual analysis in Appendix 6, the activity of the TUC clearly fell away in late 1982 and it appears that the independents on the Committee stepped into the vacuum. The social partners have never been equal in the sense of being able to call upon equivalent resources and expertise but in recent years the evidence suggests the inequality of power and influence has increased. One could make a strong case for saying that the tripartite process in the ACTS at least has become lop-sided due to a fundamental change in the political climate. HSE policy makers clearly felt that they had to make their peace with this political reality.

But perhaps these very real political changes and policy manoeuvres mask an even more fundamental change, and that concerns the OEL paradigm. It is my contention that what HSE, in a faltering and confused way, has been trying to cope with is a paradigm crisis. Before that is discussed it is worth considering what can be learned from the UK OEL setting experience.

11.4.1 UK OEL setting as a window on TLV's

In the chapters on TLVs and the OEL paradigm a case was developed that these OELs were and always had been what, in the UK, are called reasonably practicable standards. The case is convincing but is even more so when one considers OEL setting in the UK.

Although the BOHS never saw its role as setting OEL's en masse the explicitness with which it addressed issues which were left implicit by the ACGIH reflects back onto the process of the TLV Committee. The BOHS Hygiene Standards Committee put a number to the degree of risk at the limit and made it clear that technical difficulties and cost were the principle reasons why a certain level was chosen. The ACGIH used the phrase "nearly all" to cover a spectrum of risks and effects and it is implicit in their process that they balance cost and risk.

The definition of control limits developed by the ACA and adopted by the ACTS extends our understanding of TLVs and enforcible OELs in general. And the workings of the ACTS adds to our knowledge. It is clear that in setting a reasonably practicable OEL, the ACGIH, the BOHS, the ACTS and indeed any other OEL standards committee setting enforcible OELs, are not simply concerned with technical feasibility and cost across the board but with the exposure pattern, feasibility of control and cost for one industry and often one process within that industry. The styrene example is very clear on this point.

Thus, although UK OELs do not in general have the same status and influence as ACGIH TLVs the process by which they are set does act as a window on the process in the USA.

What is perhaps surprising at first sight is that a nominally expert committee should arrive at decisions which are similar to a tripartite consensual committee. However, it is clear from the analysis and discussion in this chapter and the earlier analysis and discussion in this chapter and the earlier analysis of the ACGIH TLV Committee that although ostensibly working according to different principles and remits, in reality this is not the case. It would seem the case that when setting reasonably practicable OELs the composition of the committee is more or less irrelevant except that consensual committees tend to choose from the more lenient end of the spectrum of reasonable practicability.

11.4.2 The OEL paradigm - a continuing crisis

The conclusion of Chapter 5 was that the IH profession and other groups that worked to and exposed the OEL paradigm rode out the crisis in the 1960's and early 1970's. Hatch's model in particular appeared to resolve the contradictions. However the crisis has rumbled on because the ideas and forces which challenged the paradigm in the 1960's have not only continued their attack they have grown in strength, in particular the environmental lobby.

From the analysis of the TLV setting process in this thesis there are very real contradictions in the OEL paradigm and the rewritten version at the end of this chapter is a far more prosaic affair than the idealistic earlier Working Definition. Castleman and Ziem's paper has called into question the impartial status of TLVs. The UK OEL setting experience has cast a new light on TLVs. And this understanding of TLV Committee practice probably extends to all those countries with their own OEL setting committees - they will understand the methods the ACGIH uses in setting reasonably practicable OELs. The claims made for TLVs and the OEL paradigm in general and the reality of OEL standard setting have moved further and further apart. Why then has there been no clear response - no alternative paradigm?

Well, in the UK context there has been a response of sorts. HSE policy makers had little loyalty to the IH profession and likewise although willing to use the OEL paradigm were not of a mind to defend and espouse it. However, the alternative two-tier, three list approach is according to the analysis in the thesis confused and confusing. Kuhn's evolutionary view of the growth of scientific knowledge is very apposite at this point. He argues that looking back at the growth of scientific knowledge is like looking at an evolutionary tree - looking backwards in time it is clear that many lines and species have died out. In this light although HSE has perceived and responded to a paradigm crisis its alternative is, in the Kuhnian evolutionary sense – a dodo: it may live a few years, but in the author's judgement, it is bound to die out and leave no progeny.

The IH profession itself has no answer except to reiterate what are for it self-evident truths based on the current paradigm. The IH profession itself is caught in a tautology.

The only current alternative appears to be that proposed and practiced in Holland, but for reasons to be examined and discussed in the next Part of the thesis, the Dutch approach does not represent a true paradigm shift.

How can it be that a paradigm remains in chronic crisis and yet no alternative is developed which would resolve the crisis? Why is it that the crisis remains hidden?

These questions are addressed in the final part of the thesis but a general point can be made at this point and that involves the non-Kuhnian nature of applied sciences. This point has clearly been a potential issue throughout the thesis and the implications were made plain in chapter x where the inertial role of clients was considered. A shift in the OEL paradigm will not occur simply due to internal discussion within the IH profession and other professions that are wedded to it, such as IT. The clients of applied sciences and professions in some way anchor paradigms and make it difficult for shifts to occur The implication is that the crisis will have to be large and sustained before both the professions which espouse the paradigm and the clients that rely upon it will be prepared to contemplate an alternative.

Much ground has been covered in the four parts of this thesis. They were necessary to clarify the "Development of chemical exposure limits in the workplace". We are now in a position to consider an alternative paradigm with alternative standard setting practices; more general issues only touched on so far, and finally, what additional research avenues need exploring.