UNOFFICIAL ASPECTS OF A LIFE IN POLICY RESEARCH

Leslie T Wilkins
Research Professor, State University New York

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ABOUT THE AUTHOR

The author has experience in both the `hard' and `soft' sciences. He has spent about half of his research career in England and half in the United States. He undertook sponsored research for two President's Commissions in the U.S. and for the Royal Commission on Taxation in the UK He has carried out research in the military, civil and academic settings. Quite early in his career(s) he had an ethical disagreement with an Air Chief Marshal about flying safety research. His R.A.F. commission ended forthwith. Later a disagreement with a Home Secretary on proposed new drugs laws led to his accepting a United Nations assignment in the Far East, after which he was invited to a chair at the University of California at Berkeley. This career also was somewhat turbulent and terminated when, as acting head of a department, he refused to be an informer as demanded by the Regents (Chaired at that time by Ronald Reagan). He was invited to the State University of New York at Albany and on retirement was awarded the title of Research Professor. He received early recognition with the award of the Francis Wood Memorial Prize of the Royal Statistical Society for his epidemiological study of deafness. Thereafter he received honours from several U.S. and international organisations. He officially retired in 1982 at 65 years of age and has since resided in Cambridge where he continues to do odd jobs in research.

DEDICATION

To my family and especially to my wife, Barbara,

whose half-century of support extended even to the endorsement

of my several protest resignations,

though the cost to her was greater than to me.

Printed posthumously by Leslie's widow and family

and rededicated to his students everywhere.

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Sheffield Telegraph fro Figure 3.

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PREAMBLE

This book is best read as two books intertwined. The time-line runs from the 1933 depression until the present and tells of significant episodes of policy advice alongside aspects of personal history. Many stories may be seen either as analogies deployed in applied research models or simply as anecdotes.

Governments, major industries and even charitable organisations have recourse to policy advisers: persons that the popular press refer to as the "faceless ones". The activities of these persons have an impact on all of us, yet to the average citizen they remain a somewhat mysterious army. They claim all kinds of expertise and experience yet their opinions are seldom made directly public. The press reports, not the advice given, but the decisions made by the managers, politicians or administrators. Indeed it is seldom that the information upon which policy decisions are based is itself made public.

Much policy research tends to be associated with methods termed "think tanks", "focus groups" and the dissemination of the outcome by "spin doctors". The popular press comment usually gives the public a poor impression of these techniques. This viewpoint may occasionally be both substantially and dramatically justified. However, justified or not, it applies only to a small proportion of the available techniques, mainly those which, superficially, do not appear to require complex analysis. The main misunderstandings arise with those research techniques which are 'person orientated' rather than 'machine orientated'.

Of course, the fact that the public are misinformed or uninformed about almost the whole field of policy research is not particularly important: the public are misinformed on many other things of more significance -- but it does matter to the recruitment of staff. If graduates in the appropriate disciplines are misinformed, both parties and the public they serve will be disadvantaged.[1]

In this book I will attempt to provide the would-be recruit to policy research with a different perspective than the background which might be obtained by studying the available research reports.[2] In addition to shedding some light on the background of an adviser's job in Whitehall and Washington, my purpose is to provide an unorthodox picture of the kind of lifestyle which may be expected by graduates or others who choose to work in policy research; though this is no occupational guide.

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1 I am indebted to a few students at the Institute of Criminology, Cambridge, who were aware of this. Nick Baylis with the support of Dr. Loraine Gelsthorpe of the Institute set up a dinner party for my wife and myself on the understanding that I would discuss the background to some of the aspects of my research career. Their challenge was not only a major stimulus to this writing but gave me an insight into the philosophy underlying the sundry methods I had deployed.

2 The New Scientist in an editorial (2nd Aug 1997 -- 2093) pointed out the deficiency of scientific papers as a historical source and commended a publication by Sandia Laboratory scientists along much the same lines as I now propose.

A Historical Note: Policy Research Types?

A frequently quoted, useful and simple classification of professions proposes three major categories, namely those that are concerned mainly with people, with things or with ideas. This classification is expanded by combinations and different degrees of emphasis upon the three major features. I do not regard a highly focussed motivation towards helping people to be appropriate for those who would carry out social research or provide scientific advice: such persons should undertake social work or similar outlets. Interest should be in finding out what is happening and perhaps how, and there should be less concern with *why*. But how is the necessary degree of detachment to be achieved? I can present no simple formula. I cannot theorise about my own activities, I can tell only about them and do my best to avoid any excursions into self-discovery. I may say, right away, that I take the view that a research perspective develops out of experiences which have a large random element.

While I accept the autobiographical basis for this presentation it seems to me to be absurd for anyone to suggest that my research style, or patterning is significant of my particular personality and that this in turn derives from my early life experiences.

It was in the pursuit of interesting (now designated policy-relevant) questions that my work took me from a career in aircraft engineering, through economic forecasting to end my days in the field of criminology. Administratively these employments represent three different careers and involved both academic and government service. It was, indeed, only after retirement that I began to realise that there was a philosophical continuity in the ways in which I had come to state problems and the methods I had used in the solutions. It would, however, be dull and probably pointless to discuss this trend abstractly, while it may be interesting (and sometimes amusing) if I tell the stories as an off-the-record historical and personal account.

If, today, I were present in a public meeting and someone asked, "Is there a criminologist in the house?", all who knew me would expect me to respond. It is a classification I cannot avoid. Yet when questioned by the press or lay persons, I know that if I say that I am (or was) a criminologist, a totally inaccurate picture of my activities and background of knowledge would be assumed. I would be expected to know a lot about criminals and certainly able to comment upon whatever sensational case was currently in the headlines. I would merely mystify folks if I tried to explain that, even while I was functioning as 'a criminologist' (which was only a part of my policy research work) I did not deal with criminals, but with crime and punishment.

To avoid embarrassment in spontaneous conversations such as those which arise on long flights, I would often respond to inquiries by saying that I am "a has-been statistician". If the "statistician" bit did not ensure termination of the conversation, the "has been" seemed to do so.

The classification of occupations noted above may be misleading for students who aspire to enter policy research. For example, I regret that students often opt for a degree course in one or other of the social sciences because they hope thereby to avoid

mathematics. They ought to be wrong about that! I certainly found it essential to use mathematical models to provide the information needed by governments, in the Royal Air Force, and in the Wartime Social Survey and later as a criminologist. Nonetheless, I think it may be interesting (and perhaps helpful) to would-be students if I avoid mathematical detail. I will try.

Advising governments.

The history of political change may be well documented from the political and administrative viewpoint, but few records describe the background to the advice or the experiences of the advisers. None of the research reports of projects which I have directed revealed the chance meetings which changed a project's direction, the politics of its funding, the mistaken specifications of problems later resolved, the false trails taken, nor did they reveal the ethical challenges involved in my role as director, author or co-author. In my research reports it would have been quite inappropriate for me to discuss any of the personal problems which were coped with during the projects. Though there certainly was drama in the research activity this could not be reported in the official write-up. Nonetheless the dramatic situations which occur in research work are as powerful and as plentiful as any elsewhere. Perhaps some of these true life dramas should be told?

The Style of Presentation to be Adopted.

The dramatic incidents in which I was involved seldom derived from personal concerns, but related more or less directly to research. However, being bombed by local police with tear-gas canisters on one's way to give a lecture at one of the major universities of the US might be thought to have some dramatic content with personal overtones. There were other events which were somewhat unusual. I am persuaded to include a sample of these though I remain doubtful of their relevance to my central theme, namely, telling what a role in social research is like at the operational level when advising on governmental policy.

I believe that social research is a worthwhile activity and that if sound methods are deployed, the results should be taken seriously. It is a career I would recommend. It is both challenging and demanding. Research is not an activity one can locate firmly in the workplace and leave it there during evenings and weekends. Research involves the whole person, and those around can hardly escape being drawn in.

In so far as I shall be writing about myself, it is to employ this self as an instrument to convey more significant information than a mere historical account. In my previous writings, especially those which reported officially sponsored research, I had available much more information than I was able (or allowed) to put into the reports. Some of this information I remember and since I am now 'out-of-school' I can tell a few tales. Some of the politicians and military men may, incidentally receive rough assessment.

Obviously I can discuss only those events which have concerned me in some way and where I have direct knowledge (that is my only 'data base'). But I will try not to get trapped into explaining myself. Attempts at self-discovery seem to me to border on the absurd, and I will quote only minimally from work which I have published elsewhere.

In the telling of those research projects which were regarded as successful I cannot restrict myself only to those projects which were acted upon by the authorities. In some instances there will inevitably be a tone of "I told you so!". Or, perhaps (remembering my civil service training) I should say, "the current deplorable situation demonstrates that research findings should not have been ignored". When I was presenting research results it was always necessary to be tactful: after all the work had been sponsored by the authorities, and perhaps they deserved some credit for merely asking. Maybe they were hoping for endorsements of the policies they wanted to follow, and in many cases went ahead without the support of research. At times, inconvenient findings were suppressed.

INTRODUCTION: OUTLINE AND RESERVATIONS

Historical analysis can identify patterns which were invisible at the time the events were recorded. Indeed such patterns are not directly observed and their superimposition on events may or may not be helpful. Regularities may be seen and conspiracies imagined where there was nothing more than inelegant muddle. I have expressed from time to time my opinion that too little attention is paid to the highly significant function of muddle: whether obvious muddle or well hidden as administrative practice. [3]

Though the popular image of scientific work suggests orderliness, the work of scientists is not immune to the intrusive muddle. While most people 'do' some science in the course of their general education, few receive much instruction on the design of experiments and research procedures. Those theories demonstrated by the use of methods that rely on obvious gadgets or impressive apparatus are easier to envisage and dominate the popular view. In particular, the paper work sciences attract the attention of few playwrights or novelists. I know of no fictional stories where the hero is a statistician. Yet statistics is central to all forms of research design.

An analogy for the structure of my writing here might be that of erecting a brick building; using a large quantity of mortar! The research projects are the 'bricks' and the social and domestic environments are the mortar. The mortar is as essential as the bricks. This thought adds a rationale for the inclusion of some items that I would otherwise have omitted.

Social Research v. Political Expediency?

Employment, necessary for financial support, entails certain conformity even for research scientists. This in turn can involve conflict. In retrospect I can isolate four key incidents which seem rather like nodes in my life's network of research projects. These events were the result of my adopting what I believed to be an ethical position. I think I should give a brief indication of these events now in order to provide a concise background for the reader. The events will feature in more detail in their correct sequence later.

The major dramatic situations that characterised my policy research careers illustrate difficulties at the interface of management or administration and research perspectives. The first may seem trivial. As a young clerical officer in a Ministry of Labour Local office I was said to be insubordinate when I objected to an order from my manager to substitute some "representative cases" for a random sample drawn in accord with detailed instructions from Head Office. This precipitated my first change of career from the civil service to industry. Management cannot understand the operations that are essential to obtain sound samples. The methods are not intuitive.

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3 See, for example, Chapter 5 *Social Deviance* (1964) Tavistock, London

My Royal Air Force commission terminated when an Air Chief Marshal found a questionnaire asking about near accidents in a crew room. He ordered the research to cease forthwith because he would "not have junior officers criticising their equipment".

The conflict of flying safety research methods and the requirements of military "good order and discipline" do not, apparently, converge I was most fortunate that another source of income quickly found me!

Later I found myself at the beginning of in-house research at the Home Office. This appointment terminated when I took the view that a revised official policy on 'prohibited substances' (drugs) was likely both to increase their distribution and the damage they could do. Subsequent events have proved that I was, unfortunately, right. An accusation of 'leaking' was not true, but I resigned. because I did not wish to be identified with the basis of the 1964 Drugs Bill. Unfortunately there was little doubt about the good intentions of the Minister.

Having by that time established some credibility in criminological research, I had little choice but to accept an offer from the University of California. This post terminated when as the head (Acting Dean) of the School of Criminology and Criminalistics I refused to inform on my staff as required by the Regents.

By drawing attention to these quasi-ethical stands I do not wish to suggest that others placed in similar positions would have acted differently. The significance of these events and the resulting career changes was that they led to the evolution of my research strategy -- the latent theme of this work. While conflict situations provide a dramatic interest, they had little impact upon the development of my research style, except that of a catalyst that changed my career and the field of application of the research techniques. It is, of course, the projects described and the developing methodology that provide the "meat". For good measure I include a description of my experience of a sophisticated administrative technique known as "polishing the skids".

Validity of the data

To the lay reader it is probably my work of forecasting both economic and criminal behaviour which will be the main interest. This is because of the outcomes, rather than the method or style of the research. It is true that my 'predictive' (forecasting) work was particularly successful in that the prognostications which were tested by subsequent events proved to be embarrassingly correct.

CHAPTER ONE: EARLY YEARS.

Some of my best friends are psychologists! This chapter is prepared in an attempt to give serious consideration to the clinical viewpoint within that discipline, though I do not find its explanations at all persuasive. I am not convinced that my flying safety, economic or social policy research was dependent in any way upon either my early education or childhood experiences. Nonetheless, in all fairness I think I should make an attempt to record certain events, though I remain doubtful as to their significance.

Educational Background.

Education is a continuing process. I find it unsatisfactory to assume that formal, structured education is the main educational experience in life. It may be that some educational institutions provide a social setting and a network of personal contacts that relate, often quite powerfully with future careers. For purposes of abstract analysis I would distinguish the educational from the socialising features of the school institution, though obviously both are indistinguishable in practical situations. It would be interesting to ask a sample of the general population how many of their pre-teenage school associates they are still contacting, such as by at least an exchange of seasonal greeting cards. It is a reasonable assumption that we would observe a strong correlation with the status of the educational institution attended.

My formal education, apart from providing me with the three-R basic skills, was in my view almost entirely irrelevant. If it was a socialising experience, it was not an experience I can recall. My fundamentalist parents maintained a strong control over all my school associates. Indeed for a number of years I had to participate in religious practices, including singing solos while stood up on church pews. I did not rebel, and for some time I received religious training with a view to the ministry. This was under the tutelage of a Dr. Dalling in Southampton. Perhaps my residual knowledge of New Testament Greek and Higher Criticism had some lasting value? However, if I give any credit to my early educational experiences I have no hesitation in noting the influence of my science master at my Eastleigh school. He gave me zero marks for a piece of work because the answer I provided was "too accurate". I was not entitled to any credit for deriving the answer by simple maths and some deductions ! I had not made the measurements. This early lesson in the calculus of observations was probably the most significant learning experience of all my school years. I am sure that I did not appreciate the justice of this assessment at the time. Indeed its significance may only now have been realised when searching for interesting school days memories. This master, whose name avoids me, was the one who did not discriminate against me (as I saw it) for my lack of sports and such abilities. I had an unrecognised vision defect -- a total lack of depth vision. I also recall that teacher was an atheist and made no secret of this in the class.

My father died when I was 15. That was in 1930. This was the time of the great depression celebrated in the song, “Brother can you spare a dime”. Which reminds me! I had two brothers, but they were much older and neither had kept in touch. My

widowed mother and I managed unaided on our own, partly on the income from the shop which had been my father's boot and shoe store at Eastleigh. I was accepted for a social work course at Southampton University for training as a probation officer. Financial problems made it impossible for me to take up full-time study.

Into Employment.

The Eastleigh retail business needed my late father's skills and practice as a chiropodist, and whatever help I could give was not producing adequate income. Soon the business failed completely. I had to get a full-time job. I went to see the Ministry of Labour at the Wesleyan Church Hall and was immediately recruited to their temporary staff. The only prospering business at that time was unemployment! I was at the Eastleigh office for a few months and then was moved to a small rural office in Bishops Waltham some 12 miles distant. I was 18. We were able to let the shop property and to purchase a small home in Swanmore (a few miles from my office). My salary and the rental from the Eastleigh property provided my mother and I with a very modest living. I wanted to continue my education but now part-time evening tuition was the only available means. I could not continue my probation officer training on a part-time evening basis, so I substituted evening classes in a variety of subjects, including statistics. My health must have been good because I cycled 12 miles each way to Southampton University for these evening classes on several days of the week and in addition took part in weekend activities in the village.

I can accept that an embryonic scientist was probably emerging in my first employment and was demonstrated by my first altercation with management! This was at Bishops Waltham Ministry of Labour. It seems that at this period in my life events were beginning to take place which may have contributed to my personal development and my scientific orientation, though my focal theme of methodology cannot be traced until much later. I will, therefore, begin my personal background story at around that time and continue from there in chronological order in so far as this is possible.

A Note on two aspects of Pre-war Life.

It is fashionable among preachers and politicians to bemoan the impact of the 'permissive' '60s and to claim that the reduced level of moral standards of contemporary society can be traced to the culture of that time. Certainly the mid-1960s, particularly on the west coast of the U.S.A. with anti-Viet Nam war protests, drop outs and hippie mores had wide impact. I was at the University of California at Berkeley during some of these years and I will discuss the events in the time sequence. I note this fact now because I think I must emphasise how different was the pre-war (1939/45) environment of my own youth from any time since. I must present at least a few vignettes of life in some of the areas which were my 'teen age and young environment.

The fact that my first employment was in a government department was certainly a very important factor in my personal development. My posting to a village office was most significant in moulding my social adaptation at a critical time. Perhaps village life in my

youth was little different from that of a century before and very different from anything which can be experienced in England today.

The depression was an added feature impinging on the life of all. It would be misleading if readers interpreted roles and operations in contemporary terms. If my experiences are to be interpreted usefully, this early environment must be related to my social development -- the processes of learning. There was the further factor that my father's death forced me to take responsibilities not commonly required of individuals of my age. I will take a few paragraphs to sketch the cultural settings of my early employment and leisure.

Readers familiar with village life may be aware of the difficulties 'incomers' have in being accepted by the long time residents. It may seem surprising that my transfer from Eastleigh to the Bishops Waltham office of the Ministry of Labour resulted in no such isolation. It was not that Bishops Waltham was an untypical Hampshire village. Status concerns and a clear hierarchical social structure dominated the village life. *"The rich man in his castle -- The poor man at his gate*" did not seem an unreasonable way to order things. Occupations represented social class rather more than the mere possession of wealth, and 'old money' counted more than 'new money'. I suggest that my lack of difficulty was due to two factors, one theoretical and general and the other more specific to my particular situation.

The failure of villages to accommodate and integrate incomers is, I suggest, due to the dissemination of information and the ways in which information is processed within a small community. This was of more impact before television and the more general ownership of motor vehicles consequent upon the development of easy credit.

In the village a person cannot remain anonymous and mysterious as they can in a city -- they must be fitted into the structure, and the social structure of village life is an information rich environment. Less sociologically put; one cannot get lost in a village. As the incomer becomes 'known', a fitting location in the local information matrix is possible and people then know 'how to behave' and what behaviour to expect. I was in a position where I quickly became known. Since well over 20% of the work force was unemployed and employers or their agents were in frequent contact with the office, I quickly got to know a wide sample of the *locals*. But of more significance was the status factor: I was a civil servant: indeed an official! Today this may seem surprising. Civil servants are not now highly regarded. I must clarify.

The contemporary public image of civil servants has been influenced by humorous drama and the concept that Whitehall is populated at the higher levels with Oxbridge graduates who have their own agenda for the nation. True, there may still be a dominance of classicists and law graduates and even those from other backgrounds may occasionally provide support for the public's images -- on the one hand of bumbledom and on the other beneficiaries of a caste system. The civil service structure is, indeed, still reminiscent of a caste, though termed "classes" or "grades" there are industrial, clerical, executive and (top of the form) the 'admin' class. These castes are mirrored in their trades' unions, with the exception of the 'admin' who are represented by the First

Division Association (not a ‘union’ !). Out on a limb is the scientific civil service with a similarly graded structure except that it is so designed that the specialist is always ranked somewhat below the administrative parallel. Until a few decades ago the service was the preferred job choice of graduates, and the Civil Service Commissioners were able to recruit to the Administrative classes a high proportion of ‘firsts’, and indeed a ‘lower second’ was an almost certain bar to entry into that class. Preferences have changed, and the ‘culture of the office’ doubtless has changed also.

My job in the Ministry of Labour though almost at the bottom of the civil service pecking order -- a clerical officer grade - was, nonetheless, of some substance in village life in pre-war times. My accommodation to village life and culture was doubtless further facilitated by my ability to play an organ when called upon to do so, not to mention my ownership of a car.

My car ownership certainly led to some degree of popularity with my age group most of whom were not so privileged. It was certainly a significant feature of my life style. The result was that I was diverted from both my earlier study and my career orientation. Though the dominance of religion continued to diminish, there was still a considerable residual. It was about this time that I became a church organist in a nearby parish though it was by then the organ not the denomination which was of interest.

Some events in my experiences in the village were, for me, rather good fun as I gained a mental freedom from my strict upbringing. Indeed some of the events at that time still have a pleasant flavour. Pre-war adolescent village culture provided many experiences which today may be of some historical interest.

Squires, dances and Conservatives.

Bishops Waltham in the pre-war (WW2) years still retained a number of features surviving from feudal times. Tithes were still expected. There were still a number of relics of the past in active operation. There was a "midnight baker" who did actually bake around midnight and deliver the new bread with each dawn. He also used a faggot oven. This had only one chamber where the fire was set with bundles of brushwood from local coppicing. After reaching temperature the fire was raked out and when the chamber was still hot the dough replaced the brushwood. There was a local bank owned by the 'squire'. This was Gunner's Bank, and the Gunners lived in the 'big house'. Tenant farmers were distinguished from the superior owner farmers. The unemployed were a new phenomenon and, for the most part, were casual farm labourers or building trades workers who were on and off the 'dole' depending upon conditions. Some signed-on when it froze, or rained! A significant percentage were seafaring men who 'signed on' between voyages of the great liners. Calculation of benefit entitlement was complex. Interviewing was challenging and I found it interesting to obtain the necessary information with good humour. I had 'good relations' with the unemployed and seekers for work. At first I was the junior of a staff of three, but after a few months we were two: the manager and myself.

The manager was usually referred to as Captain Wright; a WW1 rank in the Royal Engineers. This was stretching the honours somewhat because civilian use of military ranks (courtesy titles) conventionally applied only for major and above and its equivalents in other services. But he was a pillar of the Swanmore establishment. He was an interesting chap, though he would now be regarded as somewhat exploitative in his allocation of duties to me while he went golfing. But the job paid a fair salary and as a civil servant (albeit temporary) my status in the village was such that I could associate with the sons (and daughters) of the squire, farmers and bankers.

As an occasional Church organist I was 'qualified' to associate on (almost) equal social terms with the main Bishops Waltham Parish organist who was also the conductor of the local Motet and Madrigal Society. I have often wondered since who he really was, and what happened to him. He went up to Oxford after a few months. He was an extremely fine organist and conductor. He introduced me to the Dolmetsch group in Guildford, so he seems to have been well-connected His name was Heath. But I forget his first name. Perhaps it was "Ted"? I would find it too embarrassing to try to find out!

Of course there was no television and most of the 'life' of the village had to be achieved within the village or the complex of villages and hamlets of which Bishops Waltham was the centre. It had a local railway station -- a terminus on a sideline from Botley. Trains were not frequent. A 'bus service to Winchester ran every two hours but ceased in the early evening, and there was an occasional service to both Eastleigh and Southampton. A cinema opened twice weekly. If we were going to have any fun we had to devise it for ourselves. We did. But there were social constraints of class that even the younger generation could not (or would not) breach. We had dances arranged by the Junior Imperial League (now known as the Young Conservatives). These were 'democratic' in that they were open to all at a reasonable entry price, however, only those who presented themselves in proper evening dress or dinner jackets were admitted.

Some dances were held at the 'big house' and a select few of the young folk were invited. A requirement for a male for an invitation seems to have been the possession of a car. I had one invitation, but I think I was too well behaved to be invited again. Say, perhaps that I was shocked at the way the top people enjoyed themselves. I will recount just one incident because it provides a background to life pre-war in village upper class society.

On the occasion of my invitation things went with a swing and were in no particular manner remarkable, until around midnight. Drinks were available and 'finger foods'. At about midnight lots were drawn for partners. One male and car, and one female; selected at random. Then a second hat was produced and sealed papers drawn by the partners. These contained instructions as to a 'bit of fun' which was to take place forthwith at the address noted. The ‘fun’ entailed breaking in and stealing some item from the household and returning with it to the Hall. If readers question the legality of such ‘housebreaking’ activities I might point out that though there was a village constable he had been informed to expect some high-jinx on these occasions. Clearly there was no intent 'permanently to deprive' the owner of the goods, and trespass

without damage would not make a case. So a carefully engineered entry was all that was required. My partner and I had an easy target in a local hotel.

On the evening in question, by 13.00 or so all but one pair had returned to base and only one of the returnees had been seen and reported to the police (that little matter was soon dealt with). But the pair who had the most distant assignment -- some 10 miles -- still had not returned. We waited: tension mounted. Then they entered the Hall together carrying something with great care, swaddled in the car rug. A small white hand was protruding!

Now kidnapping was not on the menu. As we watched somewhat aghast they began to unwrap the body in the centre of the ballroom floor. It was, of course, a stone statuette, and though obviously mature she was just the right baby size. It belonged in a garden. The group decided that a nude was rather out-of-place in the 'house' and we proceeded to apply cosmetics and clothing. Later, in the early hours, it was returned to its plinth with a note attached to the effect that the 'league for garden modesty' had taken matters in hand. A mouse trap stolen from the Matron of the Red Cross was painted with red crosses and returned with sugar mouse in place. I do not remember any other details: and if I did I would not tell.

Despite such occasions in village life everyone's behaviour was reasonably predictable, including that of the village idiot who made a show of 'controlling' the local traffic. He had little difficulty except on market days. (As a technical aside I might say that there was no information overload and simple ‘coping strategy' proved adequate. I later managed to integrate these observations into my theory of 'social deviance'.).

I could have had much more fun if I had freed myself totally from my fundamentalist inhibitions.

It was, with few exceptions an uneventful life with the usual village scandals! I enjoyed discussions about current affairs and music with the previously mentioned Chairman of the Junior Imperial League. I was the secretary of the League. The League members were, on the whole, not much concerned with politics. One’s political party views were never questioned. They were assumed to be self-evident from one’s ‘station’.

Though political content of the proceedings of the Junior Imperial League were almost zero, for some perverse reason my association with that group brought me into contact with the thinking of the opposition. Teaching at the local monastery (The White Fathers) was a young radical activist. He was interested in local footpath preservation. Hiking as a leisure pursuit was not of interest to the Imperial League members who as sons and daughters of the landed gentry, were horse riders. I could not ride but I cycled and walked. I think it was a chance meeting on one of my hikes in the area that I met my left-of-centre discussant. Our common interest in walking meant that we often took hikes together, during which we discussed political, economic and social matters. Religion was barred as a topic.

Perhaps arising from the stimulus of these discussions I began to take an interest in personnel management and industrial welfare. I had some contact with trades union issues in my work with the Ministry of Labour. At that time there was an Industrial Welfare Society (later I believe to become the Institute of Personnel Management) which published a journal and held annual conferences to propagate the developing ideas of personnel management as a subject of study. I was able to join this Society and it afforded me the opportunity to attend my first 'learned society' conference held at the University of Reading. This was a great experience.

World War II Begins.

Then WW2 was declared. This was September 1939. A few days before I had been on vacation in Germany. With many others we had been 'rounded up' by the Consulate staff and rushed back to England. The coach trip from Koln via Aachen, across Belgium was through a thick fog. The coach was packed solid with a multinational, motley group of passengers, including some nuns who stood continuously telling their beads, obviously scared to hell! I am sure I remember this correctly though the drama may have led to some enhancement or distortion at a later date. But back to work.

As a Ministry of Labour officer with some years experience I was not, despite my age, due for conscription to the forces. The work was termed "reserved employment", though this category was continuously eroded as the war proceeded and I could not rely upon remaining a civilian. My office was charged with registration for service or conscientious objection. I tried to protect the objectors from the attention of the 'Captain' who took a poor view of their moral position.

Among my official duties was the preparation of several statistical returns. It may have been that work which prompted me to take up evening classes in statistics. In any event by the time war began I had gained a good basic knowledge of applied statistics and sampling theory. Perhaps this interest had been stimulated by the early lesson in the calculus of observations at school (when my 'right' answer was marked 'wrong'). There was also a philosophical attraction because statistics with its concept of uncertainty provided a good antidote to my earlier emphasis on 'faith'. This knowledge was about to prove very significant and to result in a change of career.

First Case of 'Insubordination'.

My limited statistical theory was sufficient to become the basis of a serious argument with the 'Captain'. We had instructions from H.Q. to take a sample -- clearly a 'probability proportional' design (as I recognised from the detailed instructions for extraction from the case files -- current and 'dead'). I was working up this material when the 'Captain' asked to look at the cases I had 'pulled'. He was unhappy with the image afforded by this group. He demanded that I select 'some more typical cases'. I objected and was overruled. I saw this as a serious matter of intentionally presenting misleading data: almost an ethical issue. Thereafter my relationship with the boss deteriorated.

The sampling incident and my 'insubordination' made it desirable to seek another post. I applied for transfer to the Fareham office. No post was available. So why should I stay in the civil service? I wanted to stay with my mother for as long as possible because, to me, she seemed to be vulnerable. She had never fitted into the local community life. Except for her Methodist Church attendance she was isolated. So I wanted to continue in some form of exempted employment.

The war effort (as various manufacturing processes were known) needed staff of all kinds. Many were exempted from conscription. All vacancies had to be notified to the Labour Exchange. This meant that all vacancies within the catchment area of the Bishops Waltham office were notified to me and there was also a 'trawl' of other vacancies for special appointments. So, when tension began to build subsequent to the 'random sampling' incident, I was in possession of much useful data.

Change of role: Folland Aircraft.

Among the notifications was one for a 'planner' for materials control at Folland Aircraft at Hamble on Southampton Water and hence within easy commute by car. Petrol was rationed but there were added coupons for those needing to use their cars to get to 'essential employment'. I would qualify, and I reckoned that I could drive economically and increase my available leisure mileage. Materials control involved calculating stock usage rates, devising relative charts and providing the information for 'progress chasers'. I was appointed. Memos which I submitted to the management of my department provided the material for my first two publications, both of which appeared as short articles in the Factory Manager. (See bibliography, Appendix ii). I think I gained a taste for writing and publishing from this time.

I owe much to the unknown co-worker who must have suggested to me that I submit my papers to the journal with a view to publication. Needless say both articles concerned statistics.

At Follands the pay was good. Though my work did not require any night shifts, it was a social expectation among the workers that all would take their stints 'on nights'. I decided to take my turn from time to time, and in any event night pay rates were higher. But night shifts were subject to heavy bombing and, initially, we spent much time in the shelters. After some months it was decided that to take shelter during alarms was not necessary and we were advised to duck under the bench when we heard a whistle. The factory had among its staff many persons whose avocations were considerably different from their function at the time. Among these was the night foreman of the pickling plant (metallurgical treatments) and the paint shop. He was an artist! Honest! We had many interesting conversations about metallurgical processes, and one night shift he drew my portrait on a piece of paper toilet towel, see Figure 1.

Figure 1. A pencil sketch of the author by John Wilson, 1941

The Secret Bomber-- It's one of ours!

It is a trivial story, but I cannot resist telling of a case where the British predilection for secrecy was exercised to its full, or even ridiculous level. When German aircraft were bombing the daylights out of everything around, then there were said to be "unidentified aircraft in the area"!. This peculiar bit of officialese led to some misunderstandings. There was an occasion on day shift when we were confined to the shelters for an unusually long period of time because 'unidentified aircraft' remained in the immediate vicinity. The truth was embarrassing to the wardens. Follands repaired military aircraft, but these were cleared of all secret equipment before consignment to our workshops. The IFF (Identification Friend/Foe) box was one of the items we 'got to know' was taken out and could be fitted only by special service personnel. This box, of course, employed radar technology: still top secret. When the aircraft were ready they were taken to an airfield adjoining, fitted up and flown away by Transport Command. But we had our own test pilots who normally would do taxi and run-up tests only or fly within visible range of the local air defence emplacements. However, one aircraft was taken up for flight test. Without its IFF it was identified by antiaircraft units as 'unidentified' in the overcast conditions. Of course when it landed it remained 'in the vicinity', and consequently the 'alert' was not cancelled. It was more than two hours before the error was sorted out. During this time we had been sheltering from one of our own test pilots. Probably a sound risk-aversive strategy.

But it was not all light-hearted stuff. There was the occasion when I drove past a land-mine swinging in the wind on a tree beside the road. There was another occasion when the 'all-clear' had sounded and we watched a low-flying 'plane just over our house at Swanmore. I could see the aircrew clearly and waved to them but was somewhat suspicious and called for my mother to drop down. I too adopted, somewhat rapidly, an appropriate lowly posture when I saw the swastika markings and shortly after a complete 'stick' of bombs was released. These fell on the local brickworks and completely demolished it. However, it had not functioned for a year or more and was almost derelict before the job was completed by our enemy.

But to return to business. When I had set up the materials control as a visual information system using a team of clerks and a gallery of charts (like the corridors of an art exhibition) I turned to 'real' planning and was given responsibility for the oversight of modifications to the tailplane and ailerons of the Bristol 152 and 156. The former involved the first development of a dihedral tailplane to enhance lateral stability. Considerable slide-rule calculations characterised my average day. But it was not particularly challenging; only the location of the work was exciting. I still retain that slide-rule.

Blundering into Conscription.

Then I heard of a vacancy with Tata of Bombay which involved the realisation of the Horsa glider. This was a challenging task, and I thought I could do it. I applied. I was accepted and started work at the office of Tata in Piccadilly. When I had been there for between two and three weeks I received a shock. My conscription exemption was no longer valid! I was exempted for the specific appointment at Folland, not on similar work for Tatas. So began my Royal Air Force service as an Aircraftsman Second Class under training -- square-bashing and jumping from towers and other silly stuff on the beach at Skegness.

On completion of training (passing out!) we were invited to indicate the posting we would prefer. Having my mother in mind I asked for a South of England location. I was, in fact, posted to Lossiemouth, which even then was in the northern part of Scotland. Here I was, at least, away from the bombing and, probably it was this posting that ensured my survival.

As an Aircraftsman Second Class I found few of my associates to have much interest in anything other than, say, drinking and gambling. There was, however, one other AC2 in the mess who was interested in cartoon drawing. He signed himself as "Jonny Morris" in my autograph book. He had been at Lossiemouth for some time and had made friends with some of the residents in the nearby town of Elgin. He introduced me to the Master of the local Freemasons' Lodge who was also the governor of the Elgin workhouse (a Poor Law Institution for the homeless). The governor was a well-educated man and we had common interests in serious music and in the work of the probation service in which I had contemplated seeking a career. I used to visit with him and his family on much of my local leave time. He persuaded me to become a Mason. Ritual was not

really 'my thing', but without doubt it was 'the only game in town' which was worth playing. I was to qualify as a Master and Mark Mason. This was a transition of some significance. My early religion had now all but vanished in a vague ritual.

P.O.M. -- that's me!

My stay at Lossiemouth was not to be a long one; I think about 4/5 months because I had been noted as a POM (Potential Officer Material). After the minimum of 6 months in the 'other ranks' I became an officer cadet at Morpeth. An official photograph of the passing out survives. I was now an officer on "administrative and special duties".

On completion of training and with the rank of Pilot Officer I was posted as Adjutant to 10 MU (Maintenance Unit) located on the Empire Central Training School at Hullavington in Gloucestershire. (This Officers' Mess is now a 'conserved' building.) I recall my arrival. On looking into the Mess and seeing that there was no-one visible with a rank below Wing Commander I sought out the Mess Secretary's office and asked to be directed to the 'junior officers' mess. Of course, there was none; only one mess, and I was in it!

The quarters were good. Life was quiet and routine, though occasionally the test pilots needed considerable tact. One or two certainly lived quite colourful lives. The Adjutant post enabled me to enjoy quite an amount of time off the base to attend concerts in Bath and visit the Tea Rooms, to play the organ in the nearby small town of Malmesbury and rehearse and then perform the baritone solos such as in the Bach St Matthew at St Pauls Chippenham.

10 MU was mainly operated by civilians. Security patrols, however, were uniformed operations and were performed by a small squad of dog-handlers who paraded around the wired perimeter. One of my tasks was arranging the timing of the patrols so that they had no predictable regularity. I used a table of random numbers. I think there was a force of about 20 dog-handlers under a sergeant and two corporals. I did not have to deal with any ceremonies or formal parades except for an occasional dog-handlers' inspection. The handlers also gave demonstrations from time to time and these were the pride of the Wing Commander who had charge of the MU.

There were occasional visits by Royalty to the Empire Training School, but they did not visit the MU. The test pilots and I together with some of the personnel of the School, merely had to wear our ‘number one’ uniforms and stay out of the way on those high days. Any one without a 'full dress' (not all conscripts were required to purchase this outfit) was usually given a day pass to stay off base. Those of us who were 'properly dressed' could stay on base and, of course, watch the flypast and join in some of the other activities in the Mess.

I was told that of all the royal visitors, Philip, the young man later to become Prince Philip, was always likely to ask to see somewhere which was not on the officially planned (and prepared) route. One occasion seems to have concerned the MU and the ECFS. The O/C had refused to give day passes to the un-uniformed, and they were assigned to

a hut away on dispersal where they were to stay until 'the coast was clear'. Philip, while being escorted around the perimeter track, noticed a hut on 'dispersal' and asked what went on in it. The evasive answer he must have received led to his insistence on going inside. On entry he found a group of junior officers in working uniform playing poker. "What goes on in here?" he asked, "Frankly" came the reply, "this is where we hide from you".

It was generally known among the officers on the base that members of the royal household had been evacuated to Westonburt. The 10MU dog patrol kennels were on the boundary of the airfield at the Westonburt end. I did not notice any evidence of the 'presence', except that this involved for me a considerable coincidental advantage, in that trains for London stopped at the little local station. This facility was, of course, provided for the entourage of the resident royals. It was possible to use this service and to visit Swanmore from time to time.

Scared after a concert.

On the base at that time there was little evidence that a war was going on, though nearby towns were frequently bombed. Concerts continued to be held in public buildings in Bath, and I was able to go to quite a number. Performances ceased early in the evening or even late afternoon because bombings were more probable after dark. So it was not unusual for an evening to be extended on a person-to-person basis, or for small groups to continue partying until a more reasonable hour.

So, there was nothing unusual when after a concert in Bath I was invited to evening coffee with a 'few friends'. The party was in a private house. Music was played and discussed. The young lady of the house, I think she must have been of about my own age, was quite attractive. Trains back to my base were not frequent. The last one would have been around 10.30. I would need to leave the house with ten minutes in hand. At ten the other guests departed. My hostess suggested that I stay a little longer. She then disappeared only to come back into the room where she had left me alone. This time she was dressed in 'something more comfortable'. I could see her figure. She protested that, being a Saturday, I need not catch the last train because there were no duties on Sunday. I did not listen, I just simply bolted. I was scared. This was a new experience. I had to keep distance or I might 'get drawn in'. Fortunately the door was not locked! Whew: what I missed! Temptation had been resisted. But I had no sense of winning any victory over the 'flesh' or the 'devil'. My motivation for getting out of there was that I was not going to be trapped by fatherhood into supporting any women. Clearly a young R.A.F. officer was not at the bottom of the pack. I was taking no chances. I knew that 'this sort of thing' happened from time to time in the villages of the Bishops Waltham area. My strategy was clearly 'extreme risk-aversive'. In any case it was all too sudden.

Marriage and setting up house.

Not long after this, one of my (church) organ activities at the Methodist church at Malmesbury led to my meeting with a most charming young woman who was to

become (and remain) my wife. Barbara was working at a local children's' home and the staff, it seems, were more-or-less, expected to attend services. I was playing for the evening service one Sunday night in July 1944. My C of E organ playing had conditioned me to play the Old Hundredth in the chant form, which clearly was not approved by nonconformists. Nonetheless I was invited to visit the Home and to play for a sing-along hymn session (probably with coffee).

I think I was introduced to Barbara by Edna Tutt. My rendering of the chant-form had caused some discussion and Barbara agreed that mine was the correct interpretation. I think it possible that she also endorsed this by quoting her father (Felix Swinstead) as an authority. I knew of his arrangements if not also some of his compositions. It appeared also that she was related to the Swinstead family of Southwick who occasionally visited the Bishops Waltham Methodists, and presumably it was her uncle who used to preach on these occasions. She was clearly intelligent and extremely good looking. It was, perhaps, for these reasons that I began to feel secure, or shall I say, not threatened by this beautiful young woman. Here was someone I would not have to run away from! I think from the start I was thinking of, well, let's say, not averse to marriage to her. I know that I was not afraid of the idea and it soon became my most desired outcome.

From then on all was fast 'down hill' to the Kingsway Hall, where Donald (subsequently Lord) Soper tied the knot. I say it was 'downhill' but this is only metaphorical! A few days after we first met I learned that Barbara was going to have a seaside vacation with her father and mother at Charmouth. I decided to take leave and meet her there. A long push of our cycles up the very long hill from the beach facilitated a long conversation as we walked to the cottage where they were staying. I do not now know how it came about, but by some means the landlady found a room for me in the same cottage. This was now serious. We did not become engaged; there was no time. This was August and we married on the second of January 1945. We spent our honeymoon at Elgin and district, staying at least one day in the Poor Law Institution with my governor friend from Lossiemouth days. We can claim to have started our married life in a "workhouse".

As I write, that was 55 years ago. Our marriage has withstood many shocks, particularly associated with my research career; but it was never in any danger.

Marriage enabled us to set up our own dwelling and I ceased to live in the Mess. Finding accommodation was a problem. Space was at a premium, and the many U.S. Forces in the area were better able to pay the premiums! However, we did find a pad in an old (very old) farm house with great cracks between the floor boards; these were useful for disposal of dust when sweeping up. The house was some 6 miles from Hullavington and I had to cycle to the office -- occasionally in 'full dress' and ready to carry out the occasional inspection of the dog-handlers. The last quarter mile from the road was down the farm track, elevated above the level of the fields by some four or five feet. Staying on the track given only the illumination permitted in the blackout was important, but not easy. Barbara usually would cycle with me but stop at her workplace in Malmesbury.

An Unfunny Thing happened on the way

In snow, one dark morning when I had a dog-handlers' inspection to carry out, I was cycling with my new wife in my full dress uniform (long greatcoat and all) when I disappeared over the edge into the field, which, remember was at least four feet below. My cycle, not designed for such descents, divided itself into two equal parts on impact. When Barbara saw me emerging from the dark depths with a disintegrated pedal cycle (perhaps my appearance was also rather un-officer-like) she burst into uncontrolled laughter. For me, the situation lacked any element justifying this hilarity. I think this was the only time I have ever thought about divorce! I do not remember how the situation was resolved but my Officer Commanding was a very relaxed 'technical type', a Wing Commander who may well have been sympathetic. The airmen, naturally, did not mind missing the inspection so they did not ask for an explanation of my absence.

The Officer Commanding H.Q. Wing.

The domestic situation at the farm house had to terminate when my posting from Hullavington so determined. I had been at Hullavington for around two years and was due for promotion from Flight Lieutenant to (Acting) Squadron Leader. This meant ceasing to be an Adjutant. The war in Europe was ending and Control Commissions were being established. I was appointed as Officer Commanding the Wing (880 Wing?) which was to be stationed in Oslo.. It was now the Spring of 1945

By the end of my four weeks training course for the work of the Control Commission, the German forces had surrendered to the Norwegian underground. This was a mere one or two days before we were due to move in. We did not know whether all hostilities had ceased or whether some 'pockets' of resistance might be expected. The flight into Oslo was by Dakota transport. Fortunately I was flying with a Norwegian pilot who knew the conditions at the airfield. As we came in to the approach we saw that the runway was occupied by German forces who seemed to be digging holes in it. Our pilot assessed that he had no choice but to put down because, once committed to the landing pattern, the Dakota did not have enough power to go round-again and avoid the mountain at the back of the city. We, and most of the squadron put down, but a few decided to go round again, and did not make it. I did not have to officiate at any funerals because the victims were flown home.

Unsuccessful at Administrative Duties

My duties as an "Officer Commanding" were mainly managerial and judicial. I had to conduct 'Orderly Room' every workday and hear applications or allocate punishment to airmen who had become drunk and (too) disorderly, including one who decided to take a swim in the Palace ornamental lake! This meant that I gained experience in application of Kings Regulations -- a sort of statutory law for the R.A.F. including the setting up of Courts Martial and Courts of Inquiry. Things were going reasonable well until the King of Norway's return. This called for a parade with my Squadron taking the tail end -- the air services being the 'youngest service' yielded status to the Navy and Army on such

occasions. The University had resumed a few days before this parade and the students were enjoying themselves on all possible occasions, and in all possible ways..

The ceremonial parade to welcome His Majesty was a problem for me. I had spent all my time since basic training on civilian units and had carried out no formal duties beyond inspecting line-ups of ten or a dozen dog-handlers, and even that had not always gone smoothly. Now I had to march at the head of the Squadron, in public. I did not do this well. My excuse was that the students crowded out the last contingent -- mine! I know that when I called a halt at the Cathedral and did my about-turn to face the troops, there were none present. It was put about that I had given the order 'right wheel' and then wheeled left; the troops naturally obeying my order. And I did not even say 'goodbye'. Naturally this incident, among my family, is the only significant event of my wartime career.

Downgraded from Administrative to Research.

Within a few days I was posted to the Air Ministry and back to Flight Lieutenant. This demotion was just great. I was to work on flying accident prevention. I could live with my wife. But I was required to wear uniform when in public places, probably because hostilities were still continuing in the Far East. This uniform business was to result in further embarrassment for me and much amusement for Barbara. The occasion was when together we went to collect from a U.S. air-base a used pram for our on-the-way first child, Arnold. I was in my officer uniform. Nonetheless, I was pushing this very rickety second-hand U.S-produced pram. When we walked past Windsor Castle gates the sentries immediately sprang to attention and, presented arms. Returning the salute (as required!) proved rather difficult. Irks (R.A.F. 'other ranks') seemed only to give salutes when it could be difficult to make the required return. The R.A.F. was not renowned for its respect for such matters of 'discipline'. The 'number one' hat, for example, always had its wire removed and was sat upon several times before being worn.

Domestic life was difficult by reason of the wartime rationing of fuel but I am sure observers would say that Barbara and I were devoted to each other. I seem, in retrospect, to have lived in two worlds -- one at home and one away. At home I did not think about work but cobbled things together to provide a few of the necessities such as lamps and bookcases. Things became somewhat different when I became involved with research. Research activities and home duties interacted. Barbara's degree in English enabled me to present better reports than I could put together unaided. There was another interesting interaction between my work experience and home life. Barbara, who was used to her father's substantial woodworking was always doubtful that any item I made would be there next week. In her theory it should have crumbled under its own weight because it lacked any structural strength. But I pointed out that even eggshells are strong and my 'stressed skin' carpentry withstood the test of time: my carpentry owed more to my experience of aircraft design than woodworking. Stresses were resolved by counter-forces rather than by constructive strength. (An analogy perhaps with my later research designs?)

Air Ministry: accident research.

It seems, on reflection, that I did not choose research as my career and that it was the posting to the Air Ministry which set me on that track. If this had not happened I might, on discharge, have tried to take up a social work vocation, such as probation officer for which I had partly trained before the war. But the challenge of saving lives through the use of my statistical approaches proved quickly to become a dominant drive.

From now on research with statistical foundations was to be the focus of my working activities. It was also to be an intellectually stormy one. My difference with the 'Captain' in the Ministry of Labour as to sampling procedures would pale into insignificance. Whenever I saw actions which seemed to me to be breaches of scientific ethics, I was stirred to a fervour of religious proportions. My reasoning was not related in any way to religious doctrines which have nothing to say about ethics of scientific inquiry. My thinking is simple -- I am responsible for me! I could not pass off (onward or upward) responsibility for any decision by claiming that I was (merely) following orders.

Not all work at the 'Airworks'

My posting back to England and to Adastral House in Kingsway meant that Barbara and I could live in part of the top floor of her family house in Swiss Cottage and I could commute to Kingsway by 'bus. While, as I noted, the war in Europe had ceased, South East Asia Command was still a dangerous area. But that part of the war was remote and was mainly seen as within the American sector. There were extreme shortages of all foods, domestic goods and clothing, but the streets were again lit and cars headlights were in use again. It seemed that the whole war was over.

Life for me, in almost all respects at this time was most productive. I was able to write up research in one or other of the two journals which were concerned with safety. The one, a serious and restricted category booklet published irregularly. The other, a monthly 'popular' coloured magazine format intended for crew-rooms to advise pilots of safety precautions such as warnings of low flying, failing to make preliminary checks, avoiding certain cloud formations. The tone of the latter was in part humorous and stressed that deeds of daring were not desirable. A leading cartoon character in this journal was a "Pilot Officer Prune". He 'lived' on the floor below the Accident Prevention Branch -- we were just below the roof. Prune's definition of a good landing, for example, was any landing that he could walk away from.

The research group consisted of five or six officers under a Wing Commander. The unit was known as PA2b and was a sector within Training Command. The personnel, with the exception of myself, were ex-operational air crew. A Squadron Leader Chandler was the team leader (we were to meet again much later). The data in respect of each accident was extracted on to punched cards which could be sorted into categories using needles. There were no computers and machine-sorting mechanisms for punched cards, though available, were reserved for code breaking and such high level technical operations. The use of machines in ordinary data-processing was to develop later.

A secondary use for the cards was in the making of model aeroplanes which could be flown from the roof of Adastral House. If any model succeeded in crossing Kingsway and landing on the roof of the Stoll Theatre which was then opposite, it earned the constructor a lunch time pint. There were few successes, and none were scored by me. I just helped in paying for the beer.

Idea of cause is rejected.

The data base we used was restricted to non-operational instances because it was to provide continuity with peacetime aviation research. I enjoyed more freedom of inquiry here than in any other non-academic post, until my wings were clipped (right off!).

The conceptual base of our work in PA2b did not strike me at the time as unusually significant, but much later it became a central idea in much of my research designs. There were strong parallels with issues in criminology, indeed in 1997 I presented a paper at an international meeting in Sicily drawing analogies with PA2b model and the developing strategies of crime prevention. This paper later appeared as a chapter in a book published in 1997. [4] Much credit must be given to Wing Commander Lester who, I think, persuaded the authorities to set up the unit in the first place and to take a non-causal and non-blame orientated theoretical base for the operational research techniques we were to deploy.

In the study of flying accidents there was plenty of scope for all kinds of application of statistical methods. I was in my element. My first and probably a fundamental contribution to this field was to stress the need for sound measures of 'exposure to risk': a theme which has underpinned most of my work since. The official publication, Flying Accident Digest. presented me with an opportunity to put forward my case and to show the impact of the two kinds of risk assessment. The questioning of a 'productivity-based' measure of flying hours on this ground of its limited value as a risk measure led me to further examination of this accepted yardstick. For example, total flying hours (summed experience) proved not to be a sound measure for assessing a pilot's likelihood of making errors. Flying hours as a measure of pilot experience needed to be qualified by 'hours on type'. Though somewhat technical and specialist, I should, perhaps say a bit more about this categorising of flying experience in relation to risk assessment because there may be unrecognised analogous cases of which I am unaware.

Exposure to risk

It had been conventional to assume that pilot experience was well assessed by the total flying hours, or flying hours on aircraft of a particular function: such as 4-engined transport, single-engined or twin-engined trainers. An experienced pilot would undergo 'conversion' training for a new type. Surprisingly I found that pilots with a large number of total flying hours and relatively few on type were more likely to make errors resulting

4 Newman G. Et al (1997) Rational Choice and Situational Crime Prevention, Ashgate/Dartmouth

in accidents than pilots with very much less total flying experience but rather more on the particular type. It seemed that in a stressful situation when quick action was needed, the response would tend to that of the dominant habit pattern rather than the more recently learned response. There was a problem of extinction, and not merely of memory and learning. [5] This is now generally recognised. However, the important point in this story is that it illustrates a useful strategy in research, namely, assumptions as to the homogeneity of measurements (let alone their linearity!) should not be assumed. Indeed, challenges to such assumptions (when identified) can be powerful tools of investigation.

Another general strategy in my research designs was also beginning to emerge at this time. This was a concern for patterns. Could any be identified and measured where they were expected or did any patterns appear where none were to be anticipated?

One immediate use for my measurements of risk (e.g.: hours and/or sorties) was to look for these patterns. How did the hours-based measures relate to sortie-based measures? Did these seem reasonable? Did injuries and causes of death of air crew show patterns? One study within this category of research strategy was the examination of patterns of injuries in serious accidents. It was triggered by air crew opinion regarding the reason why tail gunners seemed more often to survive while other crew members died. There were various beliefs about moments of inertia. However we made a careful study of the positions that crew were advised to take up if a crash landing seemed probable. The risk of death was greatest for crew at the front of the craft while the mid-upper gunner was a somewhat safer position with the tail gunner showing the lowest risk. The specified crash position for the tail gunner was to sit with his back to the line of flight, while no position was specified for the mid-upper.

I do not know who sorted out the issue, but the reason was simply that the tail-gunner's crash position (sitting backwards) had its effect because the human body can withstand far greater negative 'g' forces this way than if sitting facing forwards. The mid-upper gunner with no laid down position was likely to be facing in either direction. The accident data fitted the physiological facts. I spent considerable effort in lobbying for the adoption of backward-facing seats in passenger planes. I recall having a long audience with Lord Longford/Packenham who was then Minister for Aviation, and getting nowhere!

It is not relevant to the development of my research strategy so I will record only two features of the dramatic home situation at this time. Our son, then just under a year of age was near death for some days and to add to the difficulties, there was a total collapse of the public utilities services. This fact, together with a plumbing fault resulted

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5 This is a somewhat technical point which I do not want to elaborate here. A set of graphs illustrating the phenomena appears in Flying Accident Digest No 6. A copy is also available on file with the Royal Statistical Society from a paper delivered to the Study Section in 1946/7 session.

in our having to cope with a coating of ice almost an inch think on the kitchen floor. This persisted for several days.

In addition to soldiering on with my research, as I thought, quite inoffensively, I had teaching commitments at the City of London College and Regent Street Polytechnic. The additional funds were useful if not essential. I leave the drama of this state of affairs to the reader's imagination. While all this was going on at home, my career in research was heading for disaster in the form of difficulties with an Air Chief Marshal but there was no indication that this was in the offing. When the crunch came it was as unexpected as it was shattering.

Inter-service and interdisciplinary research.

Flying safety was not, of course, the concern only of a small research team in Adastral House. The Royal Navy flew from carriers and there were implications for design and training. This meant that there were meetings with staff in the mainly civilian scientific teams known as 'operational research units'. Nigel Balchin's novel "Small Back Room" gives a good indication of the culture of these units. So I met like minds and since we were all 'service personnel' we could discuss findings which would have the label of 'top secret' slapped on them. I was, for example, able to read the first publications of Abraham Wald's work on Sequential Sampling and other statistical materials from department S.R. 17 of the Ministry of Supply. More particularly I met kindred minds in the other branches of research in other services. Among these were many who remained friends until, with international movement, we lost touch. Among the latter was Stafford Beer, who was to become director of research at British Steel and later the founder of Sigma, an industrial consultant company with which I was to have a brief association -- including the publication of a monograph titled "Human Factors Research".

The Air Ministry position also gave me opportunities to contact scientists in the academic world. It was challenging and fun to discuss with those concerned at the Medical Research Council Applied Psychology Unit at Cambridge who were working on the “Cambridge Cockpit”. From them I learned the importance and utility of simulation and modelling. Then there was the ergonomics research group at Cranfield, an example of whose work provided a useful quotation in a 1997 publication. I was also pleased to have the privilege of taking tea with the ageing, distinguished social statistician Caradog Jones at Gonville and Cauis Cambridge. These and many other visits and discussions made a great impression on me and my scientific orientation.

All operational research scientists were working in a strange environment of secrecy. Discussion of ideas and testing of arguments were officially permissible only within departments. This was not adequate as an exchange of information to meet the requirements of the scientific process. It did not occur to me that I should have been more secretive about my research at least within the scientific community. Saving lives did not seem to call for security classifications..

The Founding of the Operational Research Club.

A number of selected operational research scientists in the different Ministries and arms of the Services arranged secretly to meet weekly for 'tea' at the Royal Society's building. It was a distinguished company including at least one FRS and one Knight. My statistical background enabled me to communicate across the 'hard sciences', and it was part of the foundation belief of O.R. scientists that cross-disciplinary discussions could be productive of innovative solutions. One was not, therefore, discouraged because one might have come from a 'soft' discipline; we even had at least one psychologist as a member!

The fact that we were discussing matters which were 'secret' did not seem unethical and certainly it was not unscientific. I hate to think what might have happened had the place been raided! After hostilities ceased, this group developed into an open Operational Research Club and later into the Operational Research Society with its own learned journal. As the declassification of documents progressed, many papers were read to, and published by sections of the Royal Statistical Society and the British Psychological Society.

Critical point in my life?

My life and especially my research activity was about to receive a major shock. Let me review where I stood up to that point. When I left the Ministry of Labour for Folland Aircraft I realised that though I had some knowledge of statistics, I knew nothing of aircraft engineering. While I was concerned with provisioning and working with progress chasers this was not a major problem. But I became interested in aircraft and engineering and began to study for the examinations of the Institute of Aeronautical Engineers. While I had nothing to show for my studies, the knowledge proved useful when I was called up to the R.A.F.. Moreover. this knowledge was also taken into account, together with my statistical know-how as a major factor in my appointment in the research at the Air Ministry. It may be because I had no paper investment in the special field of aeronautics, that I began to think that flying accidents might be considered in the light of more general principles, such as pattern analysis. When I was appointed the dominant thinking was that aircraft safety could be improved mainly by engineering and training. My early exposure to the available data led me to conclude that accidents were not too often a matter of aircraft design or training of air crew, but rather the interface between the human operator and the machine. I saw that I needed to get informed in the fields of industrial, occupational and even social psychology. I began reading and attending meetings with this in view.

The fact that PA2b was within Training Command facilitated my making contacts with psychological research work both within the services and in academic institutions. Among those contacted was Bernard Ungerson. His special field was the devising of tests to facilitate selection decisions. Various tests had been constructed to test potential abilities. Volumes have been written on this aspect of the war effort,

particularly in the U.S. Of particular note are the series of official reports contained in the several volumes of *The American Soldier.*

I was particularly interested in the statistical basis of "G" and test construction problems in general. I became closely associated with the British Psychological Society as a frequent guest of sundry members. In particular my work was appreciated in Industrial Psychology and Professor Alex Roger of Birkbeck College and I became good friends. I was fortunate in that my early, though brief, training for the probation service had steered me in the direction of some useful material in psychology,.

I began to enjoy attending the meeting of `learned societies' and applied to become a Fellow of the Royal Statistical Society. This was, I believe, the most critical step I could, and should, have taken. I was duly elected a Fellow on the 28th February 1946 at the age of 30. Life had begun

At this time my research was so much concerned with the linkage between man and machine that I came to have close associations with the university departments carrying out research in this area. I had many discussions with the psychologists in the operational research sector, including Cyril Burt (London University), and staff at the National Institute of Industrial Psychology (NIIP). My officer rank gave me a useful introduction, and my research was beginning to be known outside the services. Cyril Burt (later Knighted) was editor of the journal Statistical Psychology. Later when I became a member of the editorial group of Applied Statistics we kept in touch so as to ensure that overlap of the topics covered by our respective journals was minimised. My main association, however, was with his assistant Charlotte Banks. She was later to join the Home Office Research Unit (after I had left for the U.S.) My publications reveal my close links with some aspects of psychological research over these and a few following years so I will not comment further.

Research leads to conflict of values.

It was with that background and what I considered collegial support or at least academic credibility that I put into effect the critical research project that was to terminate not only my R.A.F. career, but any association with flying. While I cannot say that I enjoyed service life, in fact few aspects of life in the “officer class” appealed to me, my research into flying safety was a most enjoyable experience perhaps because I was able to feel reasonably effective. By then, though, the collapse of this world had already been put in train by me!

The method that caused this outcome concerned a new approach to data for safety research, it was similar to accepted methods in other fields of application. It was not the research design which caused any problem. The difficulty was fitting the project into a service setting. The same project might well have run into similar conflict if it had been attempted in any hierarchical organisation. Perhaps of more interest than the story of the termination of the work is the story of how it began.

Links with the past can always be found and research is not immune from prior influences. No project is self-contained. During my time with Folland Aircraft I was interested in quality control. It was learned quite early on in quality control that to maintain a quality we should not wait until, say, a lathe was turning out rejects. We had to devise measurements which correlated highly with the machine's drift or tool wear so that we could reset it or repair it before it produced wasted items. In other words, we sought data which would anticipate failure.

I began to think that in quality control there might be an analogue useful in flying safety research. Could we obtain data which might anticipate trouble with any aspect of new aircraft? This idea was particularly significant at the time because we were just beginning to fly jets -- Meteors and Vampires. There were very few of these aircraft and hence very few data concerning accidents. But accident data were beginning to build up. It seemed reasonable to expect that more pilots would have experienced near accidents than had suffered actual ones. Some of the latter, unfortunately were not able to tell about what happened. Why should we not see whether we could get data which related to the experiences of those pilots who had avoided an accident which they could describe? I had to have a base of comparison and since almost all Meteor pilots had flown Spitfires I could ask for comparative assessments in order to provide a general base. The Air Commodore in charge of Training Command who had the oversight of PA2b approved the scheme and I published a chapter in the "Restricted" publication, "Flying Accident Digest" saying what I had in mind and asking for the co-operation of pilots.

The first few reports to come in looked very promising. However, there was an inspection on a base (I know not which) that was carried out by an Air Marshal. His name is unimportant, but it was Bennett (I have not checked spelling). He happened to see one of my questionnaires and began to study it. I was told that he became 'livid' -- certainly the consequences support that assessment! He issued orders that the project must cease forthwith because (and I quote) "I will not have junior officers criticising their equipment". My superior officer who had approved the project was posted to South East Asia (I met him some 35 years later when he chaired my talk at his Rotary Club and though he still blamed me for his undesirable posting he also saw the whole matter as more tragic than funny).

For my part I could not accept the A.M.s' requirement of 'good order and discipline' as a superior requirement to the potential for saving lives which my method afforded. It was already clear to me from the few questionnaires which had been returned that we could anticipate, and then offset some possible dangers. The method looked as though it would yield results as effective as the quality control system had been in the anticipation of 'rejects. I was using data on 'almost disasters'.

I remember one specific case. It was the instance for which we had accumulated some data before the AM's inspection. It was a problem with the low pressure cut out differences between the Meteor Mk1 and Mk2 due to a modification of the oxygen system.

Safety quiz for R.A.F. pilots

Evening Standard Air Reporter

R.A.F. pilots at airfields all over the world are to be asked to give their views about the airplanes they fly and offer suggestions leading to greater safety.

First aircraft to be selected is the York transport, and about 100 York pilots have been given a list of more than 50 questions to answer.

Afterwards pilots of Meteor jet fighters and Spitfire XIV.'s are to be "quizzed."

The "quiz" is being organised by Air Commodore A. C. H. Sharp, D.S.O., Air Ministry Director of Accident Prevention, and the details have been worked out by Flight Lieutenant L. T. Wilkins, who is a Fellow of the Royal Statistical Society.

Pilots, it is hoped, will "confess any foolish actions, because an aircraft is not accident-proof until it is foolproof."

They can speak their minds without any "spectre of disciplinary action."

Figure 2. Cutting from the Evening Standard Monday June 10th, 1946, giving details of the 'safety quiz'. The project was stopped as soon as it came to the notice of the Air Chief because juniors were not supposed to criticise their superiors. Air Commodore Sharp, I am sure, wished he had taken less credit for this research (of which he approved). He was relieved of his Air Ministry job and posted to South East Asia.

I protested the suspension of this research and stated that I was not prepared to continue with my commission if I was not permitted to carry out research to save life. The Air Chief did not back down! Rather than accept a posting outside research I decided to resign my commission, which fortunately I could then do (my 'demob' number having been declared). We will never know how many pilots may have been injured or killed unnecessarily in the interests of preserving 'good order and discipline' Some military men may think this is a principle the preservation of which is worth a few

deaths! Perhaps it was with the same principle in mind, in the previous war, that deserters were often shot.

Unexpected consequences.

There had been a number of significant events between the promising beginnings and rather more dramatic end of my Air Ministry days. Many more years later I was to learn how a really trivial event during the Air Ministry work was to have a major impact on all of the family and many others. I refer to my activities which led to my first invitation to California to assist in setting up a research section in the Department of Corrections and the Youth Authority. Of course this visit led on to all my other work in the United States. Other events, trivial at the time, remain perceived as trivial -- but who knows -- later? If this happens I will write a sequel. Don't hold your breath.

The trivial event which provided the 'trigger' for the major consequences which commenced with my first invitation to the United States, derived from my add-on activities. It happened thus. I was invited to give occasional lectures in research design at Birmingham University. This meant travel by infrequent trains. Considerable time was spent in the Common Room while awaiting the time to begin talking. In the course of these waiting periods, and also on the train to and from London I met Lee Cronbach who was then Cultural Attaché at the United States Embassy. We were both interested in decision theory and statistical estimation; this was, of course, Lee Cronbach's special field. I found Lee a most interesting person and we enjoyed each others' company.

It was some twenty years later that I learned that these meetings were the reason for my invitation to Sacramento. When the State of California was required (at short notice) to establish a research and evaluation component in the Department of Corrections and the Youth Authority, Douglas Grant was charged with staffing. Doug was a psychologist but was aware of the importance of sound experimental design. He contacted the President of the American Psychological Association for recommendations. The President at that time was Lee Cronbach. I received a letter, completely 'out of the blue' at the Home Office inviting me to Sacramento. But that was in 1955 -- ten years later than our discussions en route to and from Birmingham. It was another ten years before I learned of the involvement of Lee in the Californian connection.

More about California anon in its proper time sequence.

CHAPTER TWO: ENGINEER BECOMES SOCIAL SCIENTIST

The Wartime Social Survey.

The situations reported in this chapter continue to relate to my theme of research design, but in respect of quite different features from those noted earlier. My focus so far has been on methodology and particularly the use of analogy or models drawn from superficially disparate or even irrelevant fields. I shall give further examples of projects that illustrate this idea, but here I would draw attention to the environment of research. The armed services may require managerial skills such as those possessed by the Air Chief, and perhaps he was correct in that the R.A.F. was not the place to carry out the research I wanted to do. It may be no more than merely unfortunate that the skills of managing a Command in the Royal Air Force did not, and perhaps could not, fit with unrestrained research operations. At this distance in time I can think that it is possible that Air Chief Marshal Bennet was a good commanding officer.

My experiences in my next career impressed upon me the view that the ways in which a manager should manage a research team differ significantly from the ways in which he should manage any other task force, office or production process A good production line or sales force manager does not automatically qualify as a satisfactory research team manager. In other words I would dispute the idea that there is a quality of managerial ability or a general quality of leadership. Moreover it does not follow that those who are expert at the tasks themselves can supervise others. The management (motivation) of research personnel would seem to require a specific skill worthy of study in its own right.

The end of my military service left me without an income and without any clear purpose. I seem to have blocked out memories proximate to that time, because the next thing I remember is that I was employed by the Wartime Social Survey. I really do not remember how I got there! I do not remember applying for the position, and I probably would not have seen myself as qualified.

My resignation from the R.A.F. meant that I could take 'terminal leave' -- a period intended to permit ex-servicemen time to obtain employment. It was not until my terminal leave was beginning to seem likely to turn into my first experience of unemployment that I must have been offered a post by Louis Moss who was the director of a population sampling organisation known as the Wartime Social Survey. By some means or another, it seems that the fact that I had resigned had become known to him. It may have been my knowledge of sampling and research using questionnaires that made me seem a suitable recruit. Both of these techniques were used by the Social Survey for collecting data for official reports on public health and economic conditions. This research was to meet the requests of government departments, but the Survey itself was an “agency” and, at that time, independent. For whatever reason it seemed to Moss that I might fit in with the team and perhaps contribute some expertise.

It was probably on Moss's advice that I did not take leave and accept the ex-service assisted-place scheme to continue the education which war had interrupted. If I had done so I would probably have become a social worker (what a thought!) or perhaps a probation officer. He noted that I had already established myself as an innovative research worker and my Air Ministry publications were more significant and original than most doctoral dissertations he had seen. His advice was "to publish, and show what you can do" rather than waste time jumping through academic hoops. I still think he was right. My lack of qualifications (sorry, I think I might more appropriately say "credentials") has not been much of a bar to academic recognition, except perhaps in England.

The sudden termination of my Royal Air Force career proved also to be a major turning point in the style as well as the content of my research. Even prior to the final project -- my attempt to obtain data on 'near accidents' -- there had been some indication of a change of focus in my work. In following the problems of safety in flying it was becoming clear that engineering considerations were often minor. The resolution of the 'human factor' merely to such categories as 'pilot error' was also, I thought, unsatisfactory. I had begun to move towards research into man-machine relationships in my study of the correlation between kinds of flying experience and 'pilot error'. The attempt to get near accident data took me into the sociological field in the need to construct questionnaires which would not seem 'threatening'.

Wartime Social Survey: Administration.

I think it was fortunate that at the time I was appointed, the Social Survey was not a government department. If it had been, I might well have experienced difficulty with obtaining ‘clearance’. I was not (yet) to become a civil servant. This probably suited my book too, because I was not feeling well disposed towards "the establishment"!

I commenced work with the Wartime Social Survey (note that the title included the term "wartime" though the war had ended) before my service leave had expired. Then, within three months of my appointment, the new Attlee Administration placed the Survey on a more permanent basis and changed its status from an agency to a division within the Ministry of Information. As a result of this nationalisation the staff (including myself) became temporary civil servants in the 'Information Officer' class. I was back in the civil service! The designation of 'temporary' was mainly because the position of 'research officer' was not recognised by the Treasury. The Information class was given parity with the Executive Class. I was ranked as equal to a Higher Executive Officer. While the rank of Higher Executive Officer was reasonably prestigious and carried an acceptable salary, it was well below the civilian equivalent of my R.A.F. rank, but nonetheless we were all happier! I was, however, soon promoted to the equivalent of Senior Executive Officer.

As an example of the status factor as it applied at that time in England, I may note that Senior Executives were entitled to first class travel on official business. As an officer in the R.A.F. not only was I entitled to first class travel on official business, but Barbara and

I were entitled to first class travel when we took leave. These and similar privileges such as separate dining facilities, continued in my new appointment. I confess that this seemed natural enough at the time and I did not notice it as a feature of my employment until my visit to the United States. I had taken the 'class structure' for granted. But to return to my theme.

Research Management

The Director of the Survey, Louis Moss, was a most talented manager of research staff. He understood that the management of a team of mixed disciplines engaged in policy research, called for quite different strategies from managing any other organisation. The financial structure of the Survey was also interesting and quite unusual. The projects undertaken by the Survey were at the request of the government departments concerned. However, the Survey had its own budget directly under Treasury control. The department wishing to have research carried out did not have to pay for this work out of its own funds, but the Treasury had to approve the project out of the Survey's allocation. The Treasury, naturally had to rely heavily upon Moss for assessment of the likely value of the projects requested. Thus, the Survey was able to maintain a considerable degree of independence from the departments commissioning research.

The 'customer' departments would specify the problem, but not the means whereby it was approached. The research design, and even possibly a redefinition of the referred problem, was up to the Research Officer of the Social Survey who was in charge of the study. A typical sequence would be for an initial meeting to be called at the Ministry concerned. Representing the Survey, Moss would attend and take with him the Research Officer who would be given charge of the project. Moss would make it clear that his role was confined to finance (sorting out the budget with Treasury) and other administrative matters, but that all contacts regarding the project should be with the Research Officer. The Ministry was asked to provide a contact official for this.

I was with the (Government) Social Survey from 1946 until 1955 during which time the unit, together with most other research units was subject to differing political climates. In 1946 the post-war euphoria had been expressed by the election of a Labour Government with Attlee as Prime Minister and this continued until 1951. Survey research was understood and appreciated by the Attlee administration and particularly by Gateskill who had previously held academic appointments at the L.S.E. This highly favourable climate changed with the change to Conservative government in 1951 and resulted in a diminished status for most forms of research and particularly any which might impact upon the political sphere of influence. Apart from one project my work was not prejudiced by the political pressures which were mainly felt through the medium of reduced budgets or in the refusal of Treasury to support applications from the departments. One effect of 'political climate' may be noted because it also reflects the skills of Louis Moss.

The Conservative Party (or perhaps it was only the Tory press) had always inveighed against civil servants and no group was more pilloried than the "snoopers" (i.e.

statisticians). So the Survey was subject to staff cuts. There was also a call for an increase in the 'controls' over personnel which conflicted with Moss's style of management. For example, one rule which was introduced required research staff of all grades to be at their desks at precisely 9.30 a.m. and not to depart therefrom until 6 p.m. and no more than 45 minutes were to be taken for lunch. Administrative staff (Principals and above) were exempted. And, by the way, work was obligatory on Saturday mornings until 12.30 p.m. Moreover we were expected to 'sign in' and to 'sign out' each day. For this purpose a book with all names listed was placed centrally, together with a prominent clock. There was a kind of mental strike among the research staff. Were 'they' interested in the conformity of our bodies or the productivity of our minds? Both were impossible because the two requirements were incompatible. Moss, naturally, supported the research staff. But a 'signing in' system had to be adopted, so, senior research staff were provided with their own books which they were supposed to complete regularly. The expressed reason for this was that research duties often took us, legitimately, away from our desks to meetings of learned societies and we were responsible for briefing field staff around the country. This was true.

Moss kept down the number of 'civil servants' in the Survey by designating all field staff (except for a few supervisors) part-time workers. No part-time workers were counted as civil servants -- two halves showed as hole (zero) not a whole! (I do not think that this still applies).

Moss's management was particularly enlightened in that he permitted lunch times to be extended to quite considerable duration. Small groups would walk around the Outer Circle of Regents Park. It was during these informal (and for the civil service 'illegal') lengthy lunch time walks that many serious problems were resolved. Moss took the view that he was running a group so as to maximise our mental productivity and regimentation was counterproductive. His task was to facilitate our work and to keep the routines of accountancy and administration 'out of our hair'. Moss also managed to achieve an interesting duality in that research officers functioned as members of the Survey research team, while also having complete control over their own projects. How this worked out is best indicated by an example which I will come to later.

Internal Organisation of technical services.

The support and technical services were, of course, organised quite differently from the research project managers, though the heads of each major category (field services, coding, and computing) were involved in each project as partners with the project manager. They not only managed their own departments but were responsible for their development. The Project Officer was expected to take note of suggestions made by these three specialists, and it was wise to do so. Priority in the use of specialist divisions was a joint responsibility under the Director. Co-operation between the project staff and specialist staff was most important, and perhaps it is strange that I know of no tensions which obtained because of this division of labour. The heads of field services, coding and computing were acknowledged to have gained much special knowledge because they applied their skills to all projects carried out in the unit.

Each project director produced between two and four major studies each year, but clearly it was not the quantity which was important. So how did we ensure quality? For this Moss relied on the collective intelligence and integrity of the research staff. We were a responsible democratic unit. We did not see our 'duty' as defined in terms of desk-hours, but in terms of the evaluation of our colleagues. I suppose one might see this as a democracy of an elite -- a kind of Plato-type Republic! In this setting one was a member of a group, with both group and disciplinary loyalties rather than an employee of the state. Membership of this group was challenging and demanding, and the criterion of acceptance was valid and (if possible) imaginative research.

The briefing of field staff resulted in my travel by (first class!) rail or air to all of the major cities of the United Kingdom. A briefing tour would normally have to be accomplished within a week or ten days by meetings in the regions to which were called all field staff in the area. The field supervisors were present at these briefings and were responsible for ensuring that the procedures required by the project director could be carried out, and then to check upon the staff performance.

Similarly the coding and computations required were arranged between the project director and the specialist heads of these departments. The interpretation of the data, the write-up and recommendations to the commissioning department were, in the last analysis, the responsibility of the project director alone and he/she signed the publication. There was one condition, the draft final report had to be circulated for all the senior research staff for comment. There was, finally, an 'oral' presentation meeting. This provided a form of collective responsibility upon which rested the reputation of the Survey as a research organisation.

This collective responsibility was also invoked when the Treasury pointed out that some commercial market research organisations charged about half of our price per interview. (As though they were buying peanuts). Our field interviewing costs were much higher because we used only two-stage random sampling, requiring interviewers to visit named individuals in their homes. Commercial organisations used 'quota sampling' methods where the individuals to be interviewed were selected by the field worker to fit certain broad social categories and interviewed in public places. The representativeness of quota sampling is uncertain and there is no satisfactory mathematical base for estimation of error. We considered that we could not take risks with our sampling. Because we were advising on government policy we needed to know the confidence limits of any estimates we derived. However, many politicians and civil service administrators could not be shaken in their belief that a sample of 4000 was twice as good as one of 2000. The support of all research staff for our policy meant that even our social anthropologist (!) had to be convinced and to recognise the mathematical basis of our sampling policy.

Moss and others were able to organise a discussion of the quota v. random sampling issue in the main body of the Royal Statistical Society in which two nationally recognised academic statisticians took part. Of course the findings of their independent inquiry supported the Survey's position. This was a useful lesson for me. It showed me that while one may be confident that a procedure is correct and in accord with all

respectable scientific opinion, it may be necessary to reinforce one's position by invoking external authorities. I used this method to support a position I wished to adopt in the Home Office in later years.

Collective responsibility

Moss's strategy of requiring presentation of one's final report to a meeting of one's colleagues before submission to the commissioning department was, of course, as I was later to learn, similar to the viva defence of a doctoral dissertation.

I remarked in passing that the officer in charge of the project signed the published report. This was unusual for 'official' documents. But Moss maintained that anonymity might be seen to confer a degree of authority which was unjustified in research activity. Furthermore, he had no time for the supposed dignity of secrecy. Unless there were very special reasons, all projects were written-up and published.

It was several years before I noticed and learned to appreciate the significance of Moss's deployments of resources of personnel and their unique suitability in the management of a research team. In addition to the group responsibility for the quality of research, another technique of his invention was most interesting and, I think, one which might be copied elsewhere to advantage. This was designed to foster the interchange of views and critical appreciation of different methodologies.

Communication and diversity

Office space generally was at a premium as so many offices had been bombed out of existence and this was reflected in the space available to individual staff. It was necessary, then (because of accommodation constraints) for research officers to share offices. Moss organised available space so that rather than share with a research assistant(s) engaged on the same project, each senior research officer shared the office with another senior research officer. This meant that each of two room-associates were working on their own, and on different projects. Every six months, or thereabouts, a rearrangement of accommodation would be 'necessary'. The new arrangement still meant that two different project directors shared office. The movement seemed unnecessary as an accommodation matter. However, each change meant that one was sharing with a project officer whose basic discipline was in contrast with one's own. My first sharing was with a social anthropologist, next with an economist, then a psychometrician, and later with a sociologist and occupational psychologist. There was a continuing mix both of personalities and disciplines in the shared research managers' accommodation. The benefits were considerable. Few people like eating (or drinking) alone. So the office duo became the informal lunching duo, often supplemented by one of the prior office duo. What did the duo discuss? Obviously their projects. Thus the design of projects began with discussions among distinctly different disciplines. The idea of organising, stimulating and utilising variety as an aid to problem solving has considerable merit. It was this feature of Moss's management which led to my successful work in several demand forecasting and other studies.

While it is true that I had carried out a few successful research projects prior to joining the Survey, my productivity and originality owes much to its unique organisation and management. It was indeed the moving around of research staff which provided the necessary condition which determined the success rather than the failure of one of my early projects. Though the burden of this chapter is the administration of the Survey perhaps I can best illustrate the value of the structures by presenting a specific example and considering the general features afterwards.

Estimating the demand for Campaign Stars and Medals

Shortly after the end of hostilities the military turned their attention to the ceremonial features of service life! Several million wartime service personnel were entitled to two or three medals each. The manufacturing of these was a function of the Royal Mint. This was no small task. In addition there was the logistical problem of distribution. The government had laid responsibility on the Treasury for the provision of funds to cover the costs of necessary operations and for arranging the distribution of campaign stars and medals. The task was assigned to the Treasury O & M Division (Organisation and Methods). At the same time this Division was carrying out an inspection of the Wartime Social Survey with a view to assessing whether it had any function in post-war government machinery. In the course of the O and M inspection it so happened that the officer concerned (a Mr L. H. Bunker) was interviewing me. It came up to lunch time. It seemed reasonable to invite him to lunch with me. The future of the Survey seemed somewhat in doubt, and with some self-interest I put it to Bunker that he had been asking me questions all the morning, perhaps he would not mind telling me something about the work of Treasury O and M. (I confess to thinking that it might be a nice place to go if the Survey was to be closed down!). He then told me of the medals provisioning problem.

The Navy were demanding that the Mint manufacture all the medals to which ex-servicemen were entitled. The Army and Air Force were prepared to accept the Treasury view that there may be no point in giving these 'gongs' to anyone who did not want them. The question was, what percentage might not want which kinds of medals. If this exceeded a certain number then a special office would have to be established in North Wales to deal with the distribution. If the Navy's (sorry, "Royal Navy's") viewpoint was accepted this might result in unnecessary staffing, not to mention a possible considerable waste of metal and Mint time. I claimed that I could provide a good estimate of the likely demand.

I did not know when I had accepted the project, settled the budget and began work on the design that my colleague Patrick Slater, a psychometrician, with whom I was sharing office accommodation (and a lunchtime associate) had previously undertaken a very similar study for a different department (Home Office). It was probably the next lunchtime that he told me on our walk of his unfortunate experience with his project in which he had attempted to estimate the entitlement and demand for the Civil Defence Medal, (known colloquially as "The Fire-watchers' Medal). It was regarded as unsuccessful because the error boundaries of the estimation were greater than the

average. It seems that Bunker also was unaware of the earlier failure. He did not mention it, and he had agreed that the Treasury would put up a small sum to fund a project under my direction. The amount of money Bunker could provide I interpreted as providing a sample of not much more than 1000 individuals. The error variance due to sampling (let alone any other factor) would be around 5% at the 5% confidence level. Bunker agreed that this would solve his major problem of whether to set up a special establishment in North Wales to deal with the demand.

An answer had to be provided quickly because the Mint were proceeding with the production. A contract was settled and a date of completion agreed between Moss and O and M, and we were off and running on a project which would make or break my career in the forecasting business! What if the Navy (Royal) was right and I was wrong? I thought that I was 'risk aversive', and perhaps I usually am, but on this occasion I was unduly impressed with the expected power of the research design I intended to apply, that is, until I learned of Pat's failure. But then it was too late.

In my discussions with Bunker I had at the back of my mind a new and powerful statistical method of attitude scaling just published in the Journal of the American Society of Statistics by Louis Guttman. It seemed ideally suited to the problem of deriving a scale and estimating demand. Imagine my state of mind, when I learned that Patrick had used the Guttman method! Clearly something different had to be done.

The project is written-up and available in published documents and I will not deal with the statistical theory here. I must, however, disclose that the results I derived proved extremely accurate. The actual demand was less than 1% different from that predicted. There was, obviously a lot of luck and a lot of counterbalancing error leading to this result. But I was, eventually, given the credit. I, for my part, would give credit to three things; firstly the fact that I was able to build upon the failure of another, secondly because Moss's managerial style meant that I got to hear of that failure, and thirdly that I was lucky to find a good, powerful model.

The methods which I was stimulated to develop in order to avoid the difficulties which Slater had experienced, were also to be of considerable use in several other studies. These were a series of projects in which we were estimating the demand for rationed commodities when it seemed that the supply position indicated that they might be released to market forces. The government wanted an estimate of the prices which would be likely to effect a supply/demand balance. If 'too high' (politically determined) a price rise was projected by the research, de-rationing was held over. Only one rationed commodity was de-rationed without consultation with the W.S.S., namely confectionery (the government bowed to pressure from the trade). Rationing of confectionery had to be reinstated because of shortages and a public outcry!

The demand forecasting studies were a wonderful testing ground for new methodologies because subsequent events proved (or disproved) their power. The particular project where my knowledge of Slater's lack of success led to my developing different methods may sound trivial, but it was the successful outcome of this project which had considerable weight in the government's decision to continue the department

after the impact of wartime exigencies had all but ceased. The word "Wartime" was dropped from the original title of "Wartime Social Survey" and the unit continued to carry out policy research on behalf of government departments. There is more to tell than is included in the official report.

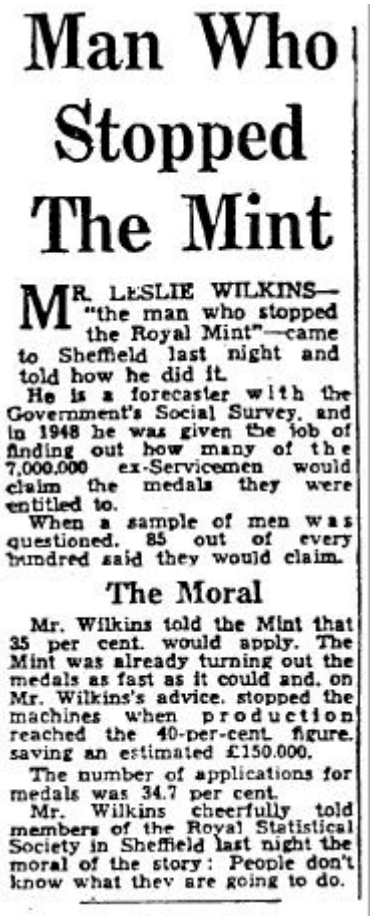
Some of the previously undisclosed features which the official report of the medals demand forecast does not contain may be of interest. In response to a scaled (Likkert) question, about 80% of my sample told me that they would 'definitely apply'. They were asked this question at the beginning of a 30 minute interview, then told what they would have to do to obtain their entitlement, and then asked again. This reduced the stated level of demand only slightly. The difference in expressed interest gave some indication of inertia. ("If it's going to be difficult then I'll not bother"). Interviewers also pointed out the cost to the country of supplying the medals, but no one seemed to bother about that -- there was no difference in the expression of intention to apply before or after *that* information. (Entitlement was nothing to do with the national economy!). I also asked whether they thought that the "conditions for issue of the medals was fair"'. Replies to this question also failed to correlate with intention to apply. If the informant could suggest occasions when he might wear his medals, there was a much stronger correlation with expressed intention. But verbal behaviour did not seem to be a good indicator of performance.

I developed a mathematical model (based on two assumed 'vectors') which took into account 'inertia' as well as 'intent' and came up with a figure of an over-all average of about 35% likely to make applications. This was vastly different from the 70-80% based upon informants' statements of intent. Was I to trust my mathematical model or the verbal assurances of the ex-serviceman and women who had been interviewed? Perhaps there were interviewer effects? There were! Most interviewers were women and most interviewees were men. Medals represented bravery in war. Would the men tend to brag about their exploits (as betokened in their medal entitlement), and did this explain the difference? Fortunately some interviewees were male, and the females interviewers were between 22 and 65 years of age. The older females far more often were told that the medals would be applied for. When being interviewed by the younger females it seems that the majority of the men were thinking of something else -- certainly medals were not foremost in their minds. This interesting finding was useful as a modifier of the crude data, but it was not adequate. A multidimensional model was, I thought, required. It was this model which gave me the figure of 35%. The 12 item vector model clearly had more power than a simple questioning could provide, and I could calculate a range of error no greater than 5%.

It was, of course, necessary in accord with Moss's managerial technique for me to present and defend my estimate before my colleagues. This was not going to be easy because fewer than half had the mathematical knowledge to appreciate the power of vector analysis. I presented my findings to the whole research staff and proposed recommending that the Treasury act upon the calculated median estimate of 35%. The team divided down the middle. While the social scientists were prepared to accept the amendments of the data-based interviewer bias they objected strongly to my total

rejection of the verbal scale and the consequent high figure of about 80%, while the mathematicians were prepared to take the 35% figure. The meeting was, I believe, rather stormy with those of anthropological persuasions most disturbed, seeming to assume that if I was correct, then anthropological research methods were threatened! (Perhaps some should be, and not only for this kind of reason, but I will not develop). The meeting closed with the issue unresolved but with my promise to do further tests as to bias or coding error and such.

Moss made it clear that the Survey's democratic philosophy included the fact that I alone was responsible -- it was my project and I could recommend as I thought best (and take the consequence!). While I was undertaking the further analyses time was running out. It ran out before I had found any possible explanation for the difference. It was around lunch time (I suppose that is why I remember it so clearly) when Bunker called on 'phone to tell me that the Mint had already made about 40% -- reaching my median figure and at my high error limit. Without any further thought, I said that in my view they should cease production forthwith. Then as an afterthought, noted that some of my colleagues did not agree with my figure.



Man Who Stopped The Mint

MR. LESLIE WILKINS—"the man who stopped the Royal Mint"—came to Sheffield last night and told how he did it.

He is a forecaster with the Government's Social Survey, and in 1948 he was given the job of finding out how many of the 7,000,000 ex-Servicemen would claim the medals they were entitled to.

When a sample of men was questioned, 85 out of every hundred said they would claim.

The Moral

Mr. Wilkins told the Mint that 35 per cent. would apply. The Mint was already turning out the medals as fast as it could and, on Mr. Wilkins's advice, stopped the machines when production reached the 40-per-cent. figure, saving an estimated £150,000.

The number of applications for medals was 34.7 per cent.

Mr. Wilkins cheerfully told members of the Royal Statistical Society in Sheffield last night the moral of the story: People don't know what they are going to do.

Figure 3. From the Sheffield Telegraph, Friday November 27th, 1953.

Bunker stressed that he wanted my figure if I had any confidence in it. I had, and he accepted my estimate. The Mint ceased production and the North Wales scheme was not proceeded with. I said earlier my estimate was correct. It was, in fact, too correct. The general demand was within half of a percent of my estimate and within a quarter of the standard error of the sampling. The mathematical model (based on density of probabilities) had proved itself. I did not remind myself of my first lesson in the scientific method and reject the findings as being too correct!

Before moving to discuss more significant pieces of Survey research I might give a gloss to the Medals forecast of demand. It will be remembered that when I presented the results to my colleagues before publication, they did not agree with my assessment and I had promised to look further into the data. This added work was cut sort by the Mint going ahead and my receiving a telephone call from Treasury. Without my colleagues agreement the report on the research could not be published as a Social Survey Report.

Action was taken on the basis of the lunchtime telephone call and I produced a short report for the Treasury supporting my analysis and conclusions. I had to take responsibility for my advice without the full collective backing of the Survey. However, when a few years later the results were known and had also been invoked to support the practical utility of sampling techniques in the course of investigations by the Public Accounts Committee, my colleagues were not only willing for the study to be published but encouraged me to do so. I duly wrote up a report detailing the subsequent tests of the data and giving details of the actual outcome. However, I insisted that my first, personal and unsupported write-up, which I had sent to the Treasury, should be reproduced in full in the final document -- with dates! I did not go so far as to point out why the original report had not been previously published. Most readers might assume that late publication was due to there being no general interest in the work until the results justified the unusual methodology.

The Medals study was an interesting episode for me, and despite the fact that it addressed a very specific problem, its consequences were to be very far-reaching. The success of this small project, strange as it might seem, was to divert my career from statistics into criminology and then from the civil service to an academic career, and, to the United States! The changes were not of my choosing, but were driven along on a series of coincidences and I accepted the outcomes. But there is quite a bit more to tell of my time at the Social Survey before I deal with my transmogrification from 'Research Officer' to 'Statistician' (by official exam and classification by Civil Service Board). It was only in retrospect that the origin of the trend in events and the significance of the medals forecast became obvious.

I must stress that by the use of this project as an example and by noting its consequences for my personal career I do not wish to give the impression that it was a major event in the Social Surveys' operations. It was responsible for a very small budget and proportion of my official time. However the same structure as was concerned in this project was typical of several other projects. As for the medals survey itself its importance to the organisation became obvious only when Parliament challenged the

utility of sample survey methods. As is so often the case with scientific work, it is the most trivial which is seen by laymen as the most relevant.

CHAPTER THREE: POST-WAR SOCIAL SURVEY

At the time, the forecast of the take-up of campaign stars and medals seemed no more than a small project, interesting because of its methodology. It was not given much status because it was obvious that there had been much good fortune in deriving the correct answer and it lacked any element of contribution to the tot al of human welfare.

The Survey was almost under continuous attack for wasting taxpayers' money, particularly by the Daily Express and the Evening Standard. From time to time members of Parliament would also take up this theme and seek reductions in staff, or abolition. At one particularly critical period Moss arranged to stimulate a Parliamentary Question which ascertained the Treasury estimate of the saving directly due to the medals forecast. This proved to be around a million pounds, and this figure appeared in the official record (Hansard).

It was later to prove particularly significant that, under the Conservative government, this information came to the attention of Sir George Benson, Chairman of Public Accounts Committee. There were other projects which were of much more social significance, but this had a popularity beyond its merits. Nonetheless it was one of two projects which I was fortunate to direct that had a determining impact on my career. The second project was the Survey of Deafness which was an adjunct of a monthly morbidity survey which I supervised for some months.

There were several other studies which I completed during my period of service, indeed much, much more happened. I was with the Survey from 1947 until 1956. This is no place to repeat the story of the many and varied research projects. However there were a few which deserve comment, particularly because the publications of the work itself did not give the background nor relate the interfaces with other activities. The study of the Prevalence and Incidence of Deafness included a forecast of the likely demand for hearing aids under the proposed National Health Act and this provided a serious test of the methods when the aids actually became available. The fact that this forecast (also!) proved correct was most helpful. However I do not value the work for its demand forecast but as an epidemiological survey of hearing loss.

The Monthly Morbidity Sampling.

At he same time, but over a longer period than the medals project, I was supervising the operation of a continuing survey of Complaints of Sickness (morbidity). My supervision of the monthly sampling for this project took place in parallel with other projects. The survey involved a monthly sample of 2000 households which was analysed conjointly with and published by the Registrar General's Office. This regular sample provided a framework for a considerable number of methodological experiments and studies of interviewer performance in addition to the work on hearing loss. As an aside perhaps I might remark that I have come to think that while it was the medals study which set me on the criminology track, it was the Sickness Survey which provided me with an

opportunity to establish academic credibility. Of course, eventually these two features merged. But to continue the story.

The Survey of Complaints of Sickness was the only continuing project of the unit, all other studies were designed for specific and limited situations. It was the largest and probably the most important project of the W.S.S. studies relating to civilian morale. The interviewers asked informants about their visits to medical practitioners and their general health. The sample was the classical two-stage random design where the first stage was geographic districts (80% towns and cities) selected with probability proportional to size. There was also a 20% rural component sampled separately.

The population sampling frame was derived from ration book records (ration books were needed to obtain food). We had no reason to assume that any sector of the civilian population was excluded from these records. This stratified random sample provided sound national estimates of most common features of medical conditions as perceived by the individuals concerned. [6] The ailments reported were, obviously, not necessarily those that would have been diagnosed by the medical profession. This was the correct approach because we were interested in the potential 'demand' for services, whether there was or was not an identified 'need'. The sample was large enough to give estimates of regional differences on the major health conditions.

In addition to the time-series data of the complaints of sickness, doctor's visits and such, the sample could be asked a small number of questions which did not relate directly to morbidity. The regularity of the sampling and the fact that the questions were put by trained staff meant that the 'add-on' questions could be changed when the number of replies was adjudged sufficient, thus we were able to collect a large amount of collateral and useful data. This enabled us to carry out methodological research and development techniques of survey design and management.

Some methodological tests were a regular feature, covering such matters as how well events were remembered and questions of interviewer honesty, bias or interviewer/informant interaction. The cost of asking and analysing these additional questions had to be justified to the satisfaction of the director and senior research staff and were not within the personal domain of the officer who had charge of the main project. but, obviously he had more influence than the remainder of the research staff in designing these questions. Though the project did not need the full-time supervision of a research officer, it was necessary to ensure that it was kept on the rails and that interviewer morale did not slip. New interviewers also needed a considerable amount of

6 The proportion of the originally picked sample who were not contacted or did not wish to cooperate was extremely small (around 2%) and some of the missing persons might have recently moved or died. The high cooperation rate for this project was not atypical of governmental sample surveys at this time. Public support, not surprisingly, dropped when sampling was high-jacked by salesmen who used “research” as a cover to their selling: behaviour deplorable because the reduced cooperation in genuine research prejudices the validity of the results.

training in order to ensure that the data were both reliable and valid. Interviewer cheating was always a possibility and was constantly under review.

During the year or so that I was in charge of this health project, I wrote a handbook of instructions for the training of field interviewers. I also carried out some research into interviewer bias and potential fraud. For the latter I was able to devise a statistical test which gave a warning of any doubtful completion of interview schedules. I should stress that Survey staff were meticulous in distinguishing between questionnaires and 'interview schedules'. The questions on an interview schedule were there for the interviewer to verbalise and to record the informant's responses, either categorically or verbatim as required by the research director. Questionnaires are filled in by the informants with or without the help of a third party. The importance of making a distinction between the two types of form (often both loosely called 'questionnaires') is perhaps not obvious. It is that with a questionnaire the informant will usually glance over the whole form ("What's all this about?") before starting to make replies. There is, hence, no effective order in the questioning by "questionnaires" whereas with the interview schedule the order is under control of the research design. Order may be important. It is possible that the asking of one question may influence the reply to one asked later. A danger with questionnaires when questions are put orally to informants, is that there is little control over any tonal inflection which may change the interpretation. A combination of the two methods was our usual approach with interviewers presenting some printed questions to subjects.

In my research I knew which questions were asked and in what order and the order might be experimentally varied. In more than one study the order of asking was an extremely important feature of the analysis and inference. Unlike laboratory experiments or field trials in agricultural research, the interview cannot be replicated. Asking questions not only requests information, it also provides information. If one asks somebody what they think about many public issues the most honest reply might well be, "Frankly until you asked me I had not given the matter a moments' thought!"

With these points in mind it was still useful to 'bank' samples of respondents to the Survey of Sickness, and perhaps other projects, when these persons had certain qualities and to re-interview them in much more detail on the specific features.

Memory, Interviewer cheating and such.

The basic research which I was able to 'bootleg' on to the Survey of Sickness was published by the Social Survey in a Methodological Monograph Series. It may be interesting in passing to note one or two non-technical findings from the work of detecting fraudulent interviewers.

Although the Monographs are available in my files much of the content has ceased to be of interest over time. There were a few results which it may be worth recording here because they are of a more general and lasting interest -- not relating specifically and

only to the survey of sickness or other project with which the methodology was associated.

Most lay persons, and indeed other scientists tend to think that interviewers could easily imagine responses to questions and fill in schedules as though they had actually visited households. Some interviewers occasionally also thought that they could do that. But there is a difference between imagining one or two cases of probable replies on a particular topic and imagining between 20 and 30 (an average assignment for an interview on each project or each month in the case of continuing surveys). This proved an impossible task! The reason is that people in real life are far more weird than even an ex-social worker could sit in a coffee shop and imagine. In more precise terms, the variance of fraudulently completed interview data was far less than the variance obtaining for actual interviews. An even more powerful indicator of cheating interviewers was constructed by taking advantage of the fact that in real life people are somewhat inconsistent and in ways which it was impossible to imagine. For example, think what percentage of regular readers of the Socialist Worker you would expect to vote Tory? Stereotyped thinking would suggest that this was totally inconsistent and no one would do both. I would not guarantee that! Or if asked to name one or two members of the British Commonwealth from a list of countries, would anybody include the Soviet Union?. Just over 1% did do so.

The survey data gave us an opportunity to measure the frequency of a large number of relationships between opinions and behaviour. We found many which did not accord with the expectation logic would suggest. Human behaviour and opinion is partly, but only partly predictable, so that the correlations between beliefs, and between beliefs and actions are usually small, but certainly non-zero. Interviewers who might think of making up interviews (after all novelists imagine people who seem real enough) would have to decide which items go together and which do not, and to get this proportion moderately correct for their sample of 20-30 cases. The size of the sample which each interviewer completed was large enough to provide useful data. Thus anyone who is cheating can be detected by their having recorded more reasonable patterns of responses, but not by the individual replies. These patterns can be detected only by statistical analysis.

The statistical tests identified the suspects, if any, but were not treated as sufficient evidence to act against the individual. Interviewers were not discharged only on the grounds that they had they failed to pass the statistical tests, but were subject to further supervision and 'call-backs'. Only when there was strong direct evidence could interviewers be discharged. If interviewers were found to have falsified schedules, these were to be destroyed and replaced with true interviews carried out by the field supervisors.

Some commercial market research companies use more direct methods. Interviewers are given some items of information by briefings which cannot be obtained from interviewed subjects. If any item from these data are fed back, the interviewer is suspect. This might be seen as 'entrapment' and not desirable on ethical grounds. There are probably other methods also which are unknown to me. Those with which I was

familiar I will not disclose because they are probably still being relied upon. Most respectable sample surveying organisations take reasonable care to ensure that the field staff do carry out interviews as required. A sub-sample is usually followed up by telephone call from a supervisor.

Interview reliability was only one element in the continuous search for more powerful methods of data collection. We made comparative studies of 'panel' methods, paying/not paying informants for giving us information and many other variants of data collection. If I were to tell of all these operations I would end with a textbook of survey methods, and that is not my intent. However one or two variations may have a general interest. I divided one population sample into three equal and similar parts; one was given questions in one order, for one other the order was reversed and the third folded across the middle! I had interviewers ask the same question at the beginning and end of the interview -- most informants did not notice. Research was fun, especially when I did not have to do any of the 'legwork' myself, but just think up ideas and write up the results.

Demand Forecasting -- As a class of problem.

By this time the fact that demand forecasting was possible within useful limits had become known to top civil servants in various departments, if not to Ministers themselves. The Ministry of Health was at this time gearing up for the implementation of the National Health Act and it was necessary to estimate likely costs of various alternative provisions. There would obviously be a large increase in public demand for medical services, mainly in general practice. It was necessary to estimate these demands and the likely financial consequences of the enactment. Previously there had been a limited provision of health care for the working classes, by what were termed, Panel Doctors. To some extent data from the co-operation of the Panel could be used to derive some of the necessary costings. For most ailments the pre-existing data for the limited covered population could be projected to suggest the demands likely to be made upon the health services when the coverage became universal.

There were interesting problems in the interface between the idea of 'patient demand' and 'patient need' or the 'value' of medical provision. In some cases where treatment might have provided some benefit, the public were reluctant to consult a medical practitioner. In other cases expectation exceeded current remedies. The survey, originally designed to deal with complaints as an indication of civilian morale took on another facet of utility as background to the new National Health Service planning.

Estimating the Demand for Hearing Aids

Of particular interest was the question of non-life-threatening conditions such as deafness. The public seldom sought advice, and little attention was given other than to wash out any wax blockage. However prior to the time that the Health Service Act was under preparation the Post Office Research Establishment had been working with the development of the miniaturising of the thermionic valve. They had reduced an effective

tube to about the size and shape of a small acorn. They had deployed this in amplification devices and could prepare a portable hearing aid. This device became known as the Medresco Aid. Transistors (semiconductors) existed in a crude form in laboratories at this time, but it was to be some years before they came to be used in portable sound amplifiers. The possible availability of the government-produced aid (the Post Office had not been privatised) encouraged the Minister of Health to consider whether the new National Health Service would be able to afford to make these devices available 'free-at-point-of-delivery' to all who might benefit.

The Medresco was equal to or preferable to all instruments then commercially available. These were all both cumbersome and costly. A very small proportion of the deaf had purchased the commercially available hearing aids. These were beyond the range of disposable income for most families. They were hardly transportable. They all relied upon amplification by means of thermionic valves. Current consumption was considerable and hence there was a need to have a separate, quite large and heavy battery. I recall when in my role as Chairman of the Applied Section of the Royal Statistical Society that one member would sit in the centre front row with a black box battery pack and a microphone which he manipulated with some success, but not without quite frequent emission of some extraneous high frequencies -- somewhat similar (unfortunately) to 'wolf-whistles'. Lecturers had to be advised to ignore these and to repeat, tactfully, any words or phrases which may have been obliterated by the 'noises off'

The Medical Research Council had been required by the government to estimate the potential demand for Medresco aids, and to commission the necessary research to answer this question. This was a many-problem problem! Little was known about the prevalence or incidence of hearing loss. For most cases a cure was not considered possible and it was likely that few sufferers had sought medical attention. Those who had received medical advice could hardly be assumed to be a representative sample of sufferers. No one knew how many persons were deaf nor indeed what levels of hearing loss over what frequencies constituted deafness and some might have other ear problems which would make the use of an aid impossible. ‘Deafness’ is a simple social category which is correlated with hearing loss, but not defined thereby.

Fortunately, since I was running the Survey of Sickness at the time, I was appointed to discuss the design of appropriate research with the Medical Research Council and was particularly privileged to work with Terrance Cawthorne. He had originated surgery which provided relief if not a cure for those forms of deafness which were due to bone malformation, (a procedure known as 'fenestration'). Cawthorne drew my attention to the literature in the field. I found a study by Beasley who had proposed a 'social scale' of hearing defect. We could modify his categories and use his diagnostic questions in the field -- it meant asking such questions as to whether the subject could hear in specified social situations -- on the telephone; in church; in face-to-face chatting with friends and so on. Beasley had related these social categories to measured hearing loss in terms of decibels and frequency ranges. I saw no reason why his correlations should not apply to data which I could collect from our samples.

My strategy was to use the Survey of Sickness's monthly sample to assemble a list of names and addresses of those in the sampled households who reported any difficulties in hearing. We also had the interviewers' assessments -- they had met the subjects in face-to-face situations or had sought to do so.

The monthly sampled names and addresses of persons identified as suffering from hearing loss having been 'banked', eventually, we had a sufficiently large enough number of persons who were originally interviewed as to their general health. This sub-sample was as representative of the deaf population as was the larger random (two-stage) sample of the whole population.

Interviewers returned to this sample and completed a much more detailed interview which asked another set of questions which included the scale based on Beasley's categories, and inquired as to whether the subject might be willing to have a medical examination. Clearly the totally deaf could not be interviewed in the normal way. A special schedule was completed in such cases. Interviewers were asked also to report on whether the person was wearing an aid. This was easy because it was quite impossible to hide even top-of-the-market equipment.

We could assume that the sample was fully representative of the population because the refusal rate was less than 2%. However, not satisfied with even this high rate of response alone, we required the interviewer to complete a 'non-contact schedule' in respect of all cases of refusal or failure to make contact with the sampled name. The main purpose of this requirement was to ensure that field staff could not ease their task by reporting no contact with persons or addresses which were difficult to reach. The non-contact report included data which might be observed, such as whether the house had a garage. The rateable value of the dwelling was known from other sources and correlated with features which we might expect to be observed even on a cursory visit. Completion of non-contact schedules from the interviewer's imagination was very likely to be found out.

Perhaps it is of interest to note that interviewees who refuse to co-operate when approached often give as reasons for their non-compliance information equally or more revealing than they would have been asked to supply had they been co-operative.

About 1000 cases of 'hard of hearing' were interviewed 'in depth'. This data base provided the information we were to use in estimating potential beneficiaries and the probable demand. In accord with our general strategy, we attempted to solve the problem by using more than one model. In the event three different systems of estimation of demand were used. Though each was based on a different logic and there were differences between the crude figures given by the different methods, reasonable adjustments led to a convergence which gave us confidence that the data were reliable. In fact the differences between the unadjusted estimates related to useful categories which could, in turn, be related to other administrative procedures which might be required of applicants should the supply under the new National Health Scheme go ahead.

It was part of the remit that aids should be considered for "all persons who might benefit"; there was no age constraint and no priorities were proposed. It was a moot point as to whether those who might benefit would, or indeed should, be encouraged to apply. We rather expected that some encouragement to apply would be stimulated, if not officially, then by voluntary welfare agencies.

The older the deaf person, the less likely was it that they would wish to have an aid, though from their hearing loss they might well benefit. Aids remained very large and were to remain so until the transistor replaced the thermionic valve and even the Medresco Aid was not easy to operate effectively. It was clear that there would be considerable waste unless the provision of the aid was accompanied by training.

The failure of those who qualified for an aid to go through the necessary ropes to obtain one -- medical examination for suitability and more accurate tests than our doorstep techniques could provide -- was likely to be of considerable importance in estimating the take-up and consequently the costs to the Exchequer. In addition to the factors specific to the allocation mechanism, there were other social and medical factors which might relate to the inertia of the potential applicants. For example, at that time, among the lay public a social stigma was attached to being 'deaf'. Cartoons of an elderly gaffer with an ear-trumpet misunderstanding his companions was a common source of humour/ ridicule. The aged expected to be deaf; it was ''just old age', and the young often wished to hide the infirmity for as long as possible because it could impair their work opportunities.

On the other hand there was the opposing complicating factor that if aids became more plentiful, the social stigma would diminish as had the earlier stigma attaching to the wearing of spectacles. This would lead to demand increasing with time. The calculations we made took these factors into account.

My report provided estimates of the numbers of persons likely to apply under different procedures. It was also noted that about a half of the hard-of-hearing had an ear problem which would not allow of the use of any insert type aid. The work was very successful. Some four years after the provision of the estimates the demand curve fitted extremely closely. It remained a good fit until the transistor made the hearing aid a quite different device to fit and operate.

In addition to the social features, I was able to identify the extremely close fit of a simple exponential equation to degree of prevalence of hearing loss against age and within the Beasley categories. This finding was most reassuring.

The Francis Wood Memorial Prize

In addition to the provision of estimates of the demand (and the relating of these estimates to the different administrative circumstances which might condition them) some light was thrown upon deafness itself. The study had incidentally been the first epidemiological research into this condition. The interesting mathematical functions which were identified provided a validity test. The condition then known as "deaf-mute"

was believed to relate to very early (or birth) hearing loss. Extrapolation of the function from the appropriate equation gave an estimate of the total population of "deaf mutes" which was verifiable. The 'fit' was correct.

This project occupied rather more time than was covered by my salary, but it was interesting. I was, in any case, rewarded for this effort. On Moss's suggestion I wrote up the research in more detail and published it as an epidemiological study of deafness. The report was then submitted for consideration for the Francis Wood Memorial Prize (a money prize!) of the Royal Statistical Society. Though it had not been awarded for a year or so, it was revived and I was fortunate to get the award. The citation for this award states that it is "for the best social research using statistical methods". Such recognition was a good substitute for my lack of a university degree. Though it was never made overt, it seems obvious that this Prize led to my being twice elected as Chairman of the General Applications Section of the Society and to my being appointed as a member of the Council.

I had been elected a member of the Royal Statistical Society in February 1946 while at the Air Ministry. There is no doubt that the Society and particularly the fringe members who used to seek out my support from time to time on various projects, was of great significance in my development. Nonetheless I think it unlikely that I would have been elected to any office in the Society and certainly not to be a member of the Council without the Deafness Study.

Not all surveys (even those which I conducted!) were as successful as the 'medals' and the deafness projects. Many were routine data collection tasks. Some came up with unacceptable results.

The "Don't tell me unless .." syndrome.

The fact that the Survey undertook research at the request of departments and also had to obtain the approval of Treasury to spend its own budget, ensured that any kinds of inquiry which were likely to prove 'embarrassing' were not started. However, there was one occasion when we got under the wire. We were asked by the Building Research Station if we thought it might be possible to estimate the car-parking demand likely to be generated by buildings of different types and functions. I undertook to design a pilot project to show that the problem was amenable to analysis and to ascertain the potential utility of any formulae which might be derived. A small study was funded. It was very successful. There was no doubt that good estimation equations were possible.[7] However, in the course of the field work we noted that many industrial and commercial establishments were *de facto* using the public highway as part of their production line!

7 The equations could, for example, have been used to require developers to provide sufficient car parking space to accommodate the estimated demand as a requirement of the grant of planning permission. Perhaps this was the main reason for the premature termination of the study? There was at the time a Conservative government in power, so I cannot think that that was the case.

The saving in costs which this utilisation afforded was, as it were, rewarding antisocial behaviours. Some political action seemed called for.

The Building Research Station liked the pilot study results and the fact that relatively simple equations and very simple (mostly negative binomial) distributions could generate a good simulation. They applied for funds to be allocated to us to continue and expand the study. The file went 'upstairs' and was at the last stage seen by the Treasury Minister who wrote on the minute sheet that this project was not to be approved. His reason was, "Any information on this topic can only be an embarrassment". The Minister was later given a Knighthood and still sits in the House of Lords. Perhaps this was a very suitable reward for the honesty of his assessment?

There was another occasion when I was disturbed by political interpretation of research. For a while during my time on the Survey of Sickness the Minister of Health was one who had become previously known as the "Radio Doctor" for his salty health advice in a BBC slot. When questioned in the House he doubled an estimate I had provided. I have to assume that this was not dishonest but rather a change made to cover the assumed uncertainties -- maybe on the advice of one of the administrative classes.

The Royal Statistical Society Connection.

My activities with the Royal Statistical Society increased with my Survey work and replaced the meetings of the Operational Research Club. The Society was organised into four sections. There was the general meeting (with its associated 'Series A' journal), a Research Section (with its associated journal 'Series B'), and there were two 'applications' sections -- The Industrial Applications Section and the General Applications (earlier "Study Section") with their associated journal 'Applied Statistics'.

I was associated with the Study/General Applications Section for some time (on two occasions as Chairman) when we 'spawned' two other learned societies. We began a Medical Statistics Division which later became an autonomous organisation, and we also took an important role in developing computing applications and this doubtless facilitated the formation of a specific society. It is notable that Dr Richard Doll [8] (later Sir Richard) who became renowned for his anti-smoking campaigns and his epidemiological work on cancer, presented a paper on this subject under my chairmanship in 1953. It was an interesting meeting because the chief statistician of British Tobacco and I think also a member of the staff of Wills were also present. I do not recall anything other than calm debate with alternative explanations and further hypotheses being put forward. It was several years before the ill effects of smoking

8 This was, perhaps, the first release in the U.K. of information on the carcinogenic effects of smoking (particularly cigarettes), though a conference on this topic had been organized by a Dr. K Hammond in the USA and took place in 1952 at Cornell. (Alistair Cooke's "Letter from America" BBB (www) 22nd August 1999)

became accepted. Hans Eysenck put forward the idea that the explanation might be that smoking and a higher probability of cancer was a common feature of a personality type.

During my chairmanship of the Study/General Applications Section (first or second time, I do not recall) the Section conducted the first economic projection survey by sampling British industrialists as to their investment plans for the forthcoming year. While the project needed my support to be approved by the Section, this rather unpopular project was possible only because of the enthusiasm of Eric Shankelman, a member of the Section. I believe he was an economist employed by the Electricity Authority, but Eric should have much credit for what amounted to an important social invention. This is now a regular Treasury supported project. The Applications Section also provided much of the necessary elements, personal and theoretical, which led me later to work on the so-called “prediction” methods in criminology.

I was chairman of the General Applications when in collaboration with the Industrial Applications we launched the ‘third journal’ titled "Applied Statistics". I was a co-editor for the first few issues and an article I published in the first issue attracted positive comment in the quality national press. A 'good thing' for both the journal and I suppose, me too. However, this article fitted a rule I was to deduce far too late in life to exploit it -- namely that the more trivial a piece of work the more it will be appreciated. Of my books only the 'readers' produced any royalties, and I did little work on them.

It seems in retrospect that I may have exploited my office to foster some of my own ideas, perhaps (at least for one month) this may have been excessive. The card of notifications shows that I presented three papers at different local meetings. I gave papers in London, Merseyside and Birmingham, 2nd 3rd and 10th February 1954 and it was the shortest month! But this was not a bid for personal aggrandisement -- there were probably easier ways for this. I really was enthusiastic about statistical methods.

Conflict and Co-operation in Scientific Method

R.S.S. Presidents (who chaired plenary sessions and Council meetings) were elected with an interesting constraint. We alternated 'internal' and 'external' presidents. For a two- year term a distinguished statistician, usually an academic, would be in post, then followed a person whose distinction was related to another field, such as a ‘captain of industry’. Conflicting views were, during my term, also balanced in that both Ronald A. Fisher and Egon Pearson (who had similar views to his father Karl) each served a term as President.

The R.S.S. was to provide me with many insights and challenges to my thinking -- more, to my thinking processes. I still recall some actual words spoken at a meeting of the Council of which I was a member. Some distinguished establishment authority had sub-

Figure 4. Council and Officers of the Royal Statistical Society, 1953-4.

mitted a paper for publication in the journal of the society. The paper was doubtless sound because of its authorship. However publication in the Journal was refused on the grounds that any paper before publication "must suffer the rigours of reading in plenary session". The 'reading' was certainly a most significant part of the tradition of testing. A paper would first be considered by two independent referees, the referees would report to the Council, and the Council would select papers which might qualify for presentation. At the same time a proposer and seconder of a 'vote of thanks' would be

nominated and voted. It was the traditional duty of the proposer to point out any deficiencies in the paper. If there was nothing actually wrong, it might still be possible to show that with a little more skill, equations using half the number of terms could be found which would provide an equally rigorous proof. If the proposer missed any points, the seconder was expected to draw attention to them. Then the meeting was opened to general discussion. Finally the speaker could reply. Usually the reply was merely that the points raised would be considered. The whole proceedings were published, including the comments from the floor.

Unlike many proceedings in the Psychological Society, criticism was (usually) directed at the methodology used and not at the person of the speaker. Speaker, proposer and seconder would dine together after the proceedings. But there were one or two exceptions. When the topic departed from a straightforward mathematical framework, particularly if a matter of philosophical perspective was involved, discussion could get quite wild. I remember one rare example of this point was the Fisher/Pearson conflict. This concerned the logic of inference.

I was present at perhaps the only meeting of the Society when the proceedings were anything other than most orderly. Egon Pearson was speaking -- I think proposing a vote of thanks. Fisher jumped up on to his seat a yelled "You don't know the difference between inductive and deductive logic". Pearson took it quietly. The meeting quickly settled down with a call to order from the President. Fisher was said to fail any students who attended Pearson's lectures. It seems likely.

I leaned towards Fisher's views on inference. Some mathematicians seem to reify numbers and elements of mysticism get involved with the concept of significance, and this I cannot accept. So when it came to their viewpoints in respect of statistics I would favour Fisher's perspective. Furthermore I am much inclined to agree with Fisher's arguments when these concern the interface between his biology and his ethical philosophy, and in particular his theory of altruism. My term of office with the Council was first under the presidency of Egon Pearson whom I found to be a delightful person, always ready to explain with considerable patience. The next internal President was Ronald Fisher. A quite different person. I was most careful to stay within the territory I knew well.

In the local groups and sections, as with the main society's meetings, the critical tradition applied though with less formality. Papers were not screened prior to reading but the Committee had to be satisfied with their quality and appropriateness. There was always a discussion period following a vote of thanks. When I presented papers I benefited from the recognition this afforded and if I could survive the criticism I could feel reasonably assured that my approach was probably correct, or the best available at the time.

These procedures, as I learned later, are a basic feature of the scientific method. Practical examples of scientific accountability through “peer review” are to be found in many procedures such as PhD defence, learned journal refereeing, requirements of

grant application evaluation and in Moss's idea for the ways in which his staff should operate.

There is another feature of scientific etiquette (prejudiced by commercial sponsorship) namely that if one might chance upon a method which seemed generally useful but which had not been published in the course of normal work, it should be released for the benefit of others who might be able to make use of it. My article in the first issue of Applied Statistics followed this rule. It was a simple application of regression to data available to the Survey but for which we had no primary use. In the course of our system of sampling (probability proportional to size) we obtained a large quantity of data about households in towns and cities throughout Britain. It was a very simple matter to relate this information from our surveys to other data from such sources as the census. For example the analysis showed that it was possible to estimate the percentage of higher income earners resident in any town by a linear equation based upon data such as infant mortality rates, telephones connected and local taxation figures. This was not earth-shattering but it was of considerable use to marketing interests. I dined out at some of the best restaurants on the strength of this trivial piece of work, J. Walter Thompson or Market Research Services picking up the bills. As a civil servant I could not accept money, but as chairman of a section of the R.S.S. I saw no objection to enjoying the occasional meal, so long as I did not talk shop until the coffee was served.

When the Survey's budget was cut (with a government not too interested in social research) quite a number of the staff, who were declared redundant were able to secure good appointments in advertising or marketing companies. I could have moved too, but I was not "disposable". Those who were "disposed of" did very well, at least financially. One became a director of a world-wide advertising agency and another (Denis Lamberth) who had headed the Coding Section went to Lyons (which was then still a private family company). He became concerned with the development of LEO (Lyons Electronic Office), perhaps the first commercial applications of computer technology. LEO's memory was, I recall, a package of mercury vapour vacuum tubes acting as delay lines. I had been a friend as well as a colleague of Denis and we worked together (for fun, not funds) on a project to predict the demand for ice-cream which made use of LEO's equation-solving power.

In addition to my activities with Royal Statistical Society business I was teaching market research and statistics at evening classes. This part-time activity supplemented our income. We needed it because by that time our family was increasing and I also had some commitments to my mother and later to a cousin. Many other personal and even dramatic events were taking place while the research and the thrills of a developing and challenging career were unfolding.

A General Rule of Demand Forecasting Designs.

There were other forecasting projects each of which required different methods. Only one rule was always applied, that no project would seek to obtain an estimate of demand by means only of one model or method. If we got different results, then at least

one was wrong! The de-rationing of solid fuel provided a case in point. Three independent models were thought up and developed. Data were collected on all three sets of assumptions. In the event they gave quite different estimates of likely demand. One was rejected because the model assumed that there was a meaningful commodity of 'space heating'. Demand could only be met if this fitted the public perception and they were likely to substitute for non-solid fuel. The public had not adjusted to the shortage of conventional fuels. On detailed examination one of the remaining two estimates was also rejected because it gave an unsatisfactory fit to any characteristic demand function. Unfortunately we did not have an opportunity to test the models because even the lowest estimate was so large that de-rationing was considered unwise at that time. Demand for alternative fuels was due to added uses, not as a substitute for the one central open fire which was characteristic of so many households at that time. This may be seen as an interesting example of a frequently observed policy problem in that procedures developed as substitutes or alternatives become additions .

Colleagues carried out projects concerning the de-rationing of clothing and household furnishings.

The Ships that Pass

There were many influences on my thinking at this time and these influences were certainly not linear nor did they all pull in the same direction. There were several overlapping pressures deriving from my roles in the Royal Statistical Society and the Survey. I realise now (too late!) that my chairmanship of a section of that society was a position of some power.

In addition to work associates and contacts within the Society, many individuals sought me out for advice and, I assume, some hoped for help with their projects. A few I could help, as with one or two students at L.S.E. who were working on doctorates, and some have remained in touch. I will tell of a few of these 'visitations', most of which took place during my Social Survey days when I was carrying out various assignments. Visits often related to these diverse projects, though most (with the exception of those discussed, 'Medals', 'Coal' and 'Hearing Aids'), were fairly routine, and do not merit attention here. A number of persons with ideas they wished to see promulgated would approach me. These persons for the most part remind me of the poet's imagery of "ships that pass in the night ...". They must have enriched my own experience in ways now forgotten. Most were one-time contacts and the issues quickly disposed of while others were more persistent, or were those whom I did not discourage. I cannot give any rationale for the selection of my memory or assessment.

The unacceptability of too strict a logic.

One contact sought me out at intervals over some six or seven years to tell me about how his work had developed. I appreciated the logic of his analyses and his attempts to capitalise on it, and I tried to help. He could not accept the kind of help I could give. He was a tragic genius whose logic was perfectly sound, but who did not realise that logic

was not sufficient of itself to bring about change, even though these changes would be most beneficial and even, in the long term, profitable. This was D.S. Blacklock. He was qualified as an accountant. [9] I do not know for how long he practised, but he began to take the view that the profession was hidebound. (I tended to agree, and still do so). He tried to find a publisher for a book in which he advocated a form of contingency accounting. His views were not approved by the Chartered Institute, but his book was published by The Glencoe Free Press who presumably had the manuscript reviewed before acceptance. I do not think that the Press is a "Vanity Publishing" house. On our first visit he presented me with a copy of this work which I placed in the Society's Library.

Perhaps if he had been willing to make a few amendments he might have been able to get acceptance of some of his less radical ideas for revisions of accountancy practice. He was not willing to accept half measures. He went on to develop still further ideas for fundamental change in numerical work and in linguistics. He pointed out the illogical feature of our currency, weights and measures in that it was in part decimal, in part due-decimal and in part binary. Despite this the teaching of arithmetic in schools did not generalise the idea of a base, and was restricted to the logic of a base of ten, with 2, 4, 8, 16, 20 as exceptions for money and weight. (2 pints = 1 quart; 14 lbs = 1 stone; 16 oz = 1lb... and so on) He noted that an extremely large and increasing proportion of numbers were recorded through the medium of a keyboard rather than writing with a pen or pencil. This he rightly claimed was a trend unlikely to be reversed, and a trend which should lead to dramatic development of new technologies. But he did not take too seriously the likelihood of oral inputs to computers without the keyboard being a dominant modem. He therefore turned his attention to the design of an efficient keyboard.

Now he noted, correctly, that we have ten fingers which we use for number and literal input on a not-too-efficiently designed keyboard. But we need 'operators' as well as 'inputs'. It seemed natural that the two thumbs should be allocated the task of 'operators' while the remaining eight fingers should carry out the inputs. Therefore eight was a natural base, and most usefully binary. A natural 'word' was of eight 'bits'. If he had stopped there and pressed his case his work would probably have been appreciated. But he could not stop; he was driven inexorably with his own logic. The keyboard and number pads were not efficiently designed. He had a design which was far superior. It probably was, but the investment needed to replace existing technology was not a matter to which he was willing to give consideration. Even that difficulty might have been overcome, but Blacklock was pressed further -- and I would stress - by his logic. Words needed to be related to numbers, and words should phonetically be related both to meaning and pattern, and furthermore, both should be related to sound. So he invented a language: Tunnish. I remember his great joy when he solved the problem of meaning/symbol/sound and announced to me that he "could now play Tunnish on the piano". He was not fazed by the fact that Esperanto had not been sold successfully. He

9 See for example, Blacklock D.S. (1938) *Accountancy for Economic Management*, Glencoe Free Press.

knew why -- it was inefficient. Indeed it is! But that is not the reason it has not caught on. It is difficult to persuade people to be logical, and perhaps we could not cope with uncertainty if we were nothing but logical? If there was any fault in Backlock's network of thinking it was not the internal logic of the system, though there may have been a logic deficit at the various system interfaces. For example, he did not appreciate, or did not wish to try to accommodate the fact that a language needs redundancy in order to be effective for purposes of communication, particularly in noisy channels.

I suppose it would be said that Blacklock was not 'in touch with reality'. Pity about reality! I must say, with much regret, that I do not know what happened to Blacklock. I learned much later (when I was at the Home Office) that the security door staff had discouraged him from coming up to see me, and we lost touch when I went to the U.S.A.

Blacklock came to see me because of his perception of my ability to understand the technology he deployed. But that was not why 'Arthur' came. Perhaps Arthur Chisnell was my most interesting extra-curricula contact. But he surfaced during my Home Office time, and I will discuss his contribution when I get to that time.

Science on tap ***never*** on top***.***

I am writing now in 1999 and at an age of 84 and with the 'mental set' of 1999. I know that I cannot hope to guess precisely what was my viewpoint when I reacted to the situations I have described or shall shortly describe, and I certainly cannot readopt that viewpoint in their telling. I must, therefore, take 'time out' to reveal some prejudices before I continue with the personal story of events. I can reflect my viewpoints when these are represented in activities which can be substantiated. So the reader may 'make allowances' for personal prejudices which I may reveal.

My experiences in the Social Survey had led me to take the view that there was too close a coupling between the research work of civil servants and party political interests. I had (and still have) a strong belief that the direct impact of present methods of departmental budgeting is not in the best interest of the country. This is because research scientists employed in policy departments (such as the Home Office) are answerable to the front bench rather than to Parliament. If research is unbiased its results should not be hidden from the "Loyal Opposition".

Policy research should not only be independent but, like justice, be seen to be so. I am not arguing for scientists to be allowed to do anything they would like and still be paid by the taxpayer. Service to the country (or, preferably humanity) takes into account the needs of other systems within society than those covered by the currently governing party. To be specific, it would, I think, be far more satisfactory if the scientific civil service were organised in the same kind of way as is the Library of the House, namely that it should serve Parliament, rather than the government. By Parliament I would include Her Majesty's Loyal Opposition. I see no reason why this is not only desirable, but also possible.

I have been accused by some of having a jaundiced view of the position of research within government. Perhaps I had bad experiences and these were unusual? I have no reason to believe that they were atypical. The position of research (particularly research which comes up with the kinds of results that politicians think they can understand) is far too vulnerable while it is answerable to the interested Minister. According to all civil service doctrine, it is a cardinal sin to "embarrass the Minister". It is also forbidden to express views in any public place or in any form which is counter to departmental policy; it would in any case be an embarrassment to do so.

But to return again to history and a happier story in relation to technological and the scientific sector. I have mentioned the "ships that passed". One who must receive a mention was Gordon Pask. He was a considerable influence on my thinking at the same time as I was concerned with the work of the R.S.S. and with Blacklock. Gordon was one of the pioneers of cybernetics and communications. He had a small laboratory over a laundry in Richmond. Here were cubicles where subjects sat and attempted to communicate with other subjects in other cubicles via signals under varying conditions of ' noise' and 'redundancy'. The main point I gained from his work was that there could be a positive value in redundancy. Negative feedback (the theory of control systems) was also a related research area. Pask was developing the work of Shannon and Weaver. He worked closely with Brian Lewis who later became a professor with the Open University (which was then only a dream of Harold Wilson).

The Significance of the Savoy Tavern.

I did not realise at the time how much of a power base my Chairmanship of the Applications Section gave me. It was considerable. For example I could influence the choice of topics and speakers who would lead discussions at the monthly meetings. Furthermore, members of the section could invite guests to meetings which were held at the prestigious address of 2 Savoy Hill. It should also be recorded that after the meetings had closed a number of persons adjourned to the Savoy Tavern where discussions continued. Guests also might often join the members in the Tavern.

I am sure that the large number of persons I met by reason of my association with the R.S.S. have added much to my intellectual and social development. These 'ships...' passed signals which often I regret I failed to receive or decode. A number of these persons I could still name, but I could not integrate their influence into my story.

Prior associations with the Chairman of Public Accounts Committee in defending the work of the Survey with the example of the moneysaving medals demand estimation gave me sufficient excuse to invite him to the occasional meeting of the Section when topics of possible interest were being discussed. More importantly, as it will emerge as I continue, he would join with us in the Savoy Tavern!

At this point, though I did not know it, my career was about to change its direction in a very significant way. One of my ploys involving the Study Section developed its own momentum and took off to places and situations I had not dreamed of. I was enjoying

working on the motley collection of research commissions that made up my Social Survey day. I must, nonetheless confess that I was not entirely a passive recipient of assignments! All kinds of topics were within the compass of my methodology and the organisation of which I was a member. Of course, we were not permitted to work on problems which were not specified by policy departments. However, I saw no reason why I should not stimulate them to ask for projects which they needed, (and which I would like to carry out!). The membership of the Section included statisticians from several government departments and some had commissioned projects with the Survey. My Chairmanship made it easy to maintain good relations with many national, academic and commercial interests as well as governmental departments.

Background and role definitions

I now have a serious problem in telling my story in any narrative manner. The reader needs to know about the kind of environment in which the events and my actions were to take place, though I was almost completely ignorant of this myself at the time. So far in my story, except for a short period in training to be a probation officer, the subject of crime has scarcely been mentioned. But I was shortly to get myself seriously involved with the study of aspects of "criminals'" behaviour. However, as I pointed out in the introduction, it is no simple matter to define "crime" and hence to say what was the area of study within the province of "criminology".

The reader should know a few things I did not know about the field and will then be in a position to observe the impact of my naive activities! If I was going to do research in this area whose territoriality was I invading? Who were "the criminologists"?. How were qualifications to carry out research in "criminology" to be established? Perhaps I had no claim to any of the necessary qualifications? I was not asking these questions. To me, at that time, it seemed a simple matter that data might be collected and analysed using sampling methods, and it was the rigour of the analysis rather than the background of the research worker that was the major criterion. Perhaps I had no claim to be able to design research in an area I had no background knowledge of? In my thinking these questions did not arise because it was the model which was important, and if a model fitted a problem then it was possible to make progress. Decisions as to the fit were usually conjoint decisions with administrators or other scientists in the field. If the field was to be either 'crime' or 'criminals' or both, who were 'significant others'? In sorting out this issue I ran into some serious difficulties.

Significant Actors and Interests in Crime Research.

Many may call themselves criminologists. For example, a well-known crime reporter has so designated himself, detective police workers may have claims, and there are forensic scientists who are often called "criminalists". Unfortunately I cannot proceed further without some more precise definitions. But no agreed external source of definitions exists, and I must rely upon my own authority which was gained over the years following the events shortly to be reported. It cannot be helped that this breaks the

continuity of the narrative, but the reader must be placed in a privileged position of knowing far more than I knew of the consequences of my next moves.

But there is a contingent difficulty. If I am to state definitions and provide background I will have to give some indication of my qualifications for assuming such an authority. Though some, even some friends see me as a criminologist, I am very reluctant to admit being one, the label does not fit my self-image. But I have to accept it because it was my official classification and the heading under which I have been both paid and honoured. However, this does not prevent me from having doubts about criminology as such. What then are my qualifications to provide the reader with a picture of the environment with which I was shortly to become closely involved, and why does it matter?

I will take the last part of the question first. How are criminologists to be defined if they do not do the kind of things the general public thinks they do, or should do? The answer is, of course, criminologists are defined in the same way as other professional are defined. It is not my claim to be or not to be a criminologist which makes this so, but the opinions of 'significant others' expressed in formal and informal ways. At this point in time I was, by the same token, defined as a 'statistician', both by reason of passing examinations and practising statistical work as a paid job, and I accept that designation without question. Why this is so may become clearer after I have established some credentials. I know that this may seem immodest, but I fear it is essential to do so now. It would, I think, be far more immodest for me to assume that my readers knew my background. So I will make a few selections which mark me out now as a "criminologist" and qualify me to describe this field of study.

My appointments included a Chair in the School of Criminology at the University of California at Berkeley, where for a time (on the vote of my colleagues) I was acting as Dean. I was later a professor in the School of Criminal Justice at Albany and one-time Chairman of the Faculty. Among the honours conferred I might note the award of a Fellowship by the American Criminological Society, the Emile Durkheim Award of the Internationale Societie de Criminologie, the Sutherland Award of the American Society of Criminology, and awards by the Academy of Criminal Justice Sciences, and the American Society for Public Administration Section on Criminology. I could also quote in support of my correct designation as a "criminologist" my membership of President's commissions and being called as 'expert witness' to Senate Committees in the United States. A complete list would be boring and I regret if any omission seems to diminish my appreciation of the recognition afforded.

Though my research into crime-related topics was begun in England, official recognition of my work comes almost exclusively from the United States. Nonetheless my work in the U.S.A. took place over no more than 15 years and my other overseas contacts were of far less duration. The rest of my years carrying out research in crime-related topics (begun in 1955) has been in England. My British academic awards relate to my early research and statistics. So, perhaps, there is some possibility that my style of criminology is a different brand from the classical English form? In the mid-1950s criminology in

England and the United States of America were rather differently orientated disciplines. My work fitted the latter better than the former.

British Criminology: some historical notes.

The British and US definitions of criminology have converged over the decades but one has to recognise that the study of crime or criminals (criminology) derived from different roots in the two countries. The collection together of various academic areas under the title "criminology" is of fairly recent origin in Britain. When I made my first venture into this area of research there were many divisions and organisations each with different names and somewhat different modes of operation and concern. All involved in the study of crime related topics but there was no co-ordinating society or journal. Relevant publications might be found under all kinds of headings. Departments of philosophy, jurisprudence, law, psychology and medicine, particularly psychiatry and even cultural anthropology and perhaps more were involved in some way or another from time to time with aspects of crime or criminal behaviour but not under the heading of 'criminology'. As a few examples I may note the Howard League of Penal Reform with the Howard Journal and Police Court Missions had developed into a very significant probation service. There was an international "Penitentiary Fund" and the United Nations had a section called Social Defence. There was an Institute for the Study and Treatment of Delinquency (matching closely the USA National Council on Crime and Delinquency). In addition there was a scholarly interest in Jurisprudence at Oxford and, of course, there had been notable works by philosophers, among whom the best known is probably Jeremy Bentham at the University College London. A prison medical officer (Gorer) had published some very important work which helped to refute some of the arguments for a "criminal type" which were being put forward by Lombroso of Italy.

Official British 'criminology' was represented only by three academic appointments in the law departments of the London School of Economics, Oxford and Cambridge. Criminology today, even in Britain, would include many of the organisations not using that term a decade or so ago. Indeed there were many who disputed the validity (and utility) of crime as a defining characteristic of any discipline. I recall one cynical view which asserted that when faced with public demand to do something about a social evil, politicians who could do nothing could at least make it illegal. Underlying this cynicism is a definition of criminal law as a part of the machinery of government. This view is not universally agreed. Some take a rather mystical or metaphysical view of the concept of law and particularly the concept of "natural law". I prefer an operational definition namely that the definition of an act as a 'crime' (misdemeanour, felony, hybrid, civil or other) merely indicates the particular machinery of government which will be concerned. The variety of available legal procedures is small indeed compared with either the variety of acts which the public deem unacceptable and which, in the common view are, or ought to be crimes. The variety spawned by offenders is as great as the variety of any other category of human activity. Indictable offences (crimes) is not a descriptive category which permits of comparisons over jurisdictions nor, indeed within jurisdictions over periods of time. Much statistical data published by the Home Office

which refers to “crimes” was based on “finger printable offences”. The lack of a valid measure of crime for purposes of comparative analysis presents a major difficulty.

We have, at best, a measure which describes the qualities of an act by the procedures which will be used to deal with the actor if and when that actor is identified. But the description of the act can be based only upon available evidence. At best we can say that the counts of persons passing various staging posts in the procedures can be used to provide a labelling which may have some vague resemblance to the layman’s understanding of crime. That the categories are administratively useful is not, many would claim, adequate justification for the designation of a field of academic study. I have argued this at length elsewhere but briefly draw attention to the issue here to indicate the historical perspective as I was being swept into research in this field of activity.

In Britain the officially recognised field of ”criminology” was limited to the expertise of three individuals who initially came to the country as refugees.. Thus criminological thinking in Britain could not but be influenced by the continental schools. When I first joined the Home Office, Sir Lionel Fox, Chairman of the Prison Commission, used to refer to "the three great British criminologists, Grûnhût, Mannheim and Radzinowicz" indicating clearly by accenting their names their European background. The concept of criminology in England, as in Europe, was anchored firmly within the ambit of legal studies whereas in the United States emphasis was upon behavioural research involving both sociological and psychological research and theory and practical applications to police work. Over time in England academic criminology began to change, deviating slowly from its continental roots and even more slowly becoming more broadly based. As some of the newer universities took up the discipline, criminology began adopting more and more the behavioural studies perspectives of the United States.

In the mid-1950s there was no agreed framework for criminology in Britain and the three criminologists held independent personal academic appointments. There was no learned society nor journal, though “delinquency” was recognised in the title of the Institute for the Study and Treatment of Delinquency. There was, indeed, much discussion among the few committed individuals as to what precisely ‘criminology’ was. The United Nations employed the term “Social Defence” which had support in France and some other continental countries. Such word spinning is not of much interest except that it indicates the lack of a structure which the Home Office might have been able to work with. The British viewpoints all tended to focus upon individual offenders, whereas in the U.S.A. the emphasis was upon crime as a social phenomenon. For the same reason (the focus upon individuals) criminology in Britain sat easily with psychiatry. The English attitude towards sociology was probably most succinctly expressed later by a notable Oxford graduate who, it is reported, asserted that sociology had nothing to study -- there was "no such thing as 'society'". [10] British criminology has only relatively recently come to realise that the problem of crime cannot be simplified to the problem of the criminal.

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10 Mrs. M. Thatcher, one time Prime Minister of Great Britain

It was not until 1960 that criminological research in Britain began to broaden its scope and to prosper. This owed much to the personal attributes of one or two powerful politicians, in particular R.A. Butler during his term as Home Secretary and Sir George Benson as Chairman of Public Accounts Committee. Sir George was also chairman of the Howard League of Penal Reform. The foundation upon which official policy-orientated crime research was based had its origin in the Statistical Department of the Home Office, while the continental perspective on criminology fitted well into the existing departments of law at the three major English universities. It is important to keep in mind the different roots of criminology in the U.S.A and the European and related UK schools. While this is important for the reader to see the significance of the next few months in my career path, I knew nothing of it and for most others the distinction was unrecognised! I certainly did not recognise it until much later. While I knew that criminology was not the same as police work, I had no idea of the variety of interests involved with crime and criminals. Things were to be rather different from that which I had anticipated.

Britain Begins to Recognise Criminology

During my term of office in the Applications Section the Home Office began to consider the potential for an in-house research commitment. The appointee to the newly created position of "statistician" was, in fact, an actuary. While actuaries are mathematically competent and a related discipline to that of statistics their work does not include certain statistical techniques notably those of research design. The emphasis is upon life expectancy and valuations of risks and investment. It is true that social statistics was not a developed field in the UK at this time, and the appointment of an actuary does not suggest that research into crime was a major consideration in setting up the research element. This will now seem very odd because the Home Office Research Unit has gained international recognition in both crime prevention and criminology..

As will become clear, criminological research was begun with considerable caution. The first listed project of the published output of the Home Office Research Unit was a commissioned external grant-aided project. It was carried out conjointly by the Government Social Survey Unit and Dr. Hermann Mannheim of the London School of Economics. The success of this first venture was followed by the development of in-house research, beginning with one member of staff being named as a research officer. Significantly, I think, this appointment was in the already existing statistics department, but under the control of "C" (crime policy/law) Division. This modest (I will not say trivial) start provided a potential which, when the climate was propitious could serve as a seed, indeed this was the inauspicious launch of the Home Office Research Unit. When Home Secretary Butler wanted to put out a White Paper on Penal Reform this small research interest was able to contribute to its drafting. The operational definition of criminology was slowly changing.

Soon criminology was to be officially recognised in Britain and the British Criminological Society was founded -- but still within the European (individual offender-orientated) tradition. Criminal law practice already had an uneasy association with psychiatry (*mens*

rea issues) and this is consistent in that individual cases are the common subject of study. It was many years before British criminology embraced with any enthusiasm sociology, politics, economics and other disciplines.

My difficulty with describing myself as a criminologist is multifaceted. Obviously, because of what has happened to me I was and am now a proper criminologist as so defined in the U.S.A., but in the UK perhaps not, or not unanimously. This is an oversimplification, of course, but it may suffice (in view of what follows) to explain why I still do not know whether, if I could choose, I would wish to be identified as a criminologist. But I cannot choose. I accepted the honours (and salaries) which went with that image and I just have to accept the label. It is not surprising that the layman has difficulty in identifying what criminology is; criminologists have the same problem. But under various labels some good research is now being produced.

Towards an Involvement with Crime.

Leaving aside many events which subsequently have seemed to be of less significance I will try to give the main drift which not only had an impact upon me personally, but upon the direction of British criminology (and, so I have been told, criminology in America). Of course, I knew nothing of this at the time. But with that background let us put the clock back to when I was a senior research officer at the Social Survey undertaking various projects on behalf of sundry governmental departments. I return now to the point in my narrative where I noted the filling of the new post of an in-house “statistician”. at the Home Office. For sundry reasons which I do not think it necessary to seek to specify, I thought that it might be a good idea to get acquainted with the Home Office "statistician" and to introduce him to social statistics of a rather different and broader coverage than aspects of insurance. The appointee was Tom Lodge. I thought such an introduction might well be effected at a meeting of the Section which we would supplement with our usual later deliberations in the Savoy Tavern.

In the light of subsequent events I cannot overemphasise the routine or even trivial nature of the actions which began it all. I had no ambitions in criminology, The Home Office's wide range of responsibilities represented a related wide range of possible interesting research commissions, and I saw no reason why I should not assist the Survey to get business as well as perhaps land some interesting work for myself. While as I have noted, the new appointment did not indicate any emphasis upon research into crime or criminals (academic criminology was not a numerical-based study) research potential in the department was not restricted to matters concerning crime or criminals and it seemed likely that criminological research might have some political appeal. It might appeal particularly to Sir George. His interest in liberal aspects of the treatment of offenders seemed worthy of encouragement.

I do not know how I 'felt' at the time, nor now assess my motivation. In addition to 'selling' the Survey's research services, I might have been getting ambitious and mildly interested in reassignment within the civil service to a policy-making department. It was coming up to ten years since I had joined the Social Survey and getting to be time for a

promotion which was barred by budget. I saw myself as a statistician and looked for promotion from the Research Class (with its parity with the Executive Class) to the main Statistician Class with parity with the Administrative Class and membership of the First Division Association.

The Home Office, sometimes also called The Home Department was responsible for all categories of state business which did not have their own department. It did not cover foreign affairs, the military, education or health, nor matters within the ambit of the Board of Trade or transport. It was in charge of the residual of home matters which were left unallocated to specialist departments or ministries. It was, therefore, organised into 'divisions' which covered such issues as Fire, Police, Crime policy (including the drafting of new criminal legislation), Probation, Royal Household, Licensing, Elections, Civil Defence, and the state-owned brewery, distillery and the pubs in the Carlisle Scheme; it had close association with the Prison Commissioners, who during the latter part of my term of office were abolished and the responsibility taken by a Prisons' Division

Return to the Story.

I now return to the sequence of events, the setting of which the reader is now familiar. It is 1953/4, I am Chairman of the RSS Study Section; an actuary has been appointed to a statistician post in the Home Office; the Survey is in the good books of the Chairman of Public Accounts Committee. I think it must have seemed to me likely to be of general interest to Section members to invite a speaker on crime statistics from the academic world together with the new appointee and Sir George Benson.

There was a criminologist with a Readership at Oxford (one of the three great British criminologists). He was Max Grûnhût who had published papers on crime data and seemed to me to have the best grasp of criminal statistics. He was invited to present a paper to the Applications Section, and agreed to do so. I invited the newly appointed Tom Lodge and Sir George to the meeting and afterwards to the Savoy Tavern.

Grûnhût mentioned the attempts to 'predict' likely criminal behaviour by Sheldon and Eleanor Glueck at Harvard Law School. Unfortunately he had to leave before we got down to any serious business at the hostelry. In the bar, Benson expressed an interest in this part of the paper and asked about the methods as they related to the Medals forecast. In particular he asked whether I thought that the 'prediction' work of the Gluecks could be replicated in this country. We discussed the possibility in general terms. I did not see the estimation of demand for medals (or fuel, or hearing aids &c) as 'prediction'. I tried to explain but since I was not familiar with the Gluecks work it was difficult to comment at the time. I certainly did not consider myself to be in the 'prediction' field which suggested lack of rigour. Sir George, however, saw much similarity. He had received a review copy of "500 Criminal Careers" by Sheldon and Eleanor Glueck and it was agreed that I would study this publication and report back to Sir George in the Commons within a week or so.

I was not impressed with the Gluecks' statistical methodology. While it seemed to work, it was certainly lacking in rigour, and I could not verify some of their tests of significance or Chi-square values. I did, however, extract some suspicious data and write to them asking for an explanation. I was informed that the basic data were no longer available. To indicate my views of their work without becoming too technical I will reproduce a poem I wrote some years later when I was asked by the editor of the Howard Journal to review a follow-on book by the Gluecks. I at first refused because if I juxtaposed my assessment of their methods with the acknowledgement that nonetheless they seemed to work, it might seem that I was accusing them of dishonesty. So, my sense of humour seized me and I wrote a review in limerick form in the expectation that the editor might enjoy it and perhaps pass it around the office for amusement only. I was surprised when the Howard Journal published what must be a rare example of doggerel published in a refereed journal. Here it is: -

Please sing to (and in) tune of "That was a cute little rhyme"

Prediction of crime by the Gluecks,
Reported in several buecks
Is summarised here,
In typography clear,
Are the tables of use to the Cruecks?
CHORUS That was a cute little rhyme
Sing us another one do.

Statistical tables should tell,
Precisely, and clearly as well,
What use can be made
Of the data arrayed,
If the book is expected to sell
CHORUS

If adding a five-factor score,
Correctly assigns less, and not more,
To right categories,
Without any squeeze
Than one of the five, why use more?
CHORUS

In 'jug', or as some say, 'the pen',
The optimal number of men,
Is one in a cell,
But what can one tell
From the size of some of their (n)?
CHORUS

They say that their use by a court,
Would indicate just how they ought,
To dispose of a case,
To a suitable place,
Least likely again to get caught.
CHORUS

This book is commended to those
Who have studied prediction, in prose,
Who know of the uses,
And all the abuses
Of columns as well as of rows.
CHORUS
(signed "ILKWY HO") circa 1956

(I do not think that many who knew me had difficulty in identifying the scrambled "Wilkie Home Office" encoded in the signature!)

If this seems unkind, I think I may say that later events justified whatever sting it had. In the late 1970s the Gluecks 'found' the 'unavailable' data and made it available to John Laub and others who commended their work highly! In the intervening years I found an explanation of why their methods worked reasonably well, despite their statistical naivety. It has to do with the quality of the data and matters of noise and redundancy and error. [11]

The Borstal Project.

I was much more circumspect in my report to Benson. I wrote a somewhat detailed report and despatched it to the Commons. I had criticised the Glueck methods of scaling potential risk (by adding percentages!) and suggested that it would be better to use the discriminant function which had been demonstrated to be extremely powerful in sorting archaeological finds and similar problems.

We duly met for tea. Sir George was forthright. "If I understand your report correctly you think it is possible to predict potential recidivism and what's more you know a better way than they did?" Tea ended with him saying, in effect, that he believed that I could carry out a study which would be similar in effect to the work of 'prediction' -- the Gluecks' term for estimation of prior probabilities. However he was concerned that because I was neither a lawyer nor a criminologist, my work would not 'sell' to the audience he had in mind. He asked whether I would be prepared to work with a criminologist. I had no problems, and Sir George said that he would approach Hermann Mannheim of L.S.E. (one of the three 'great British criminologists'). Mannheim agreed. We met to discuss in his office in the School. As we entered the elevator Maurice

11 I would refer anyone interested to any text on numerical taxonomy

Kendall, who held the chair in statistics also came in. Before we reached the 5th floor it was clear that these two academics did not know each other -- I introduced them there and then. (This must be a comment on something either about academia or the disciplines concerned!).

Benson was able to get provisional approval for the expenditure of about a thousand pounds which would be used in the work which I would carry out at the Social Survey. But before I felt qualified to start this project I needed to become much more informed about the field. The necessary time had to come from my own leisure because no money could be released until work had begun. (A catch predating the famous Catch-22). I took intensive lessons from Mannheim. Mannheim knew his literature, particularly that from the Continent, but he also was well-informed on work in the United States. While he knew what the authors had said, he was never willing to make an evaluative statement, at least, not within my hearing. "A says that ... while B says ..." was as far as he would go. But he was a walking encyclopedia! He gave me many useful references to 'prediction' in criminology and wrote the first two chapters in the publication of the final work. Statistically the field was primitive. I was, however, able to deduce the objectives and gain some knowledge of possible constraints.

Most, if not all work on the topic of 'criminological prediction' had been in the United States in relation to the granting of parole. Estimates of the likelihood of recidivism were, apparently, generally acceptable as valid considerations in decisions by Parole Boards before releasing an offender to spend the remainder of his term in the community. There was, of course, an underpinning of 'treatment theory' in this view in that if the risk was high, the individual needed more time in the prison where he was not only secure but able to 'get help'. I was not too convinced of this line of reasoning, but I did not then consider it within my range of expertise. I could deal with the estimation of probabilities which were independent of any use to which they might be put.

There was no parole system in Britain and only "good conduct remission" was permitted. however, an exception was made with youthful offenders who could be sent for 'Borstal Training'. When sentenced to Borstal a period of detention was not specified but could be as short as 9 months or as long as 3 years depending upon decisions made during the time being served. Detention was in two kinds of young offenders' institutions, one 'open' and the other 'closed'. Over-all the institutions 50% were usually reconvicted within three years of release. There was a similarity with the U.S. parole systems in that the 'unexpired period' on release from the institution was spent on 'aftercare'. Aftercare was usually provided by probation officers. The 50% pass/fail cut was, of course, most useful in that it provided the greatest statistical discriminant power.

Doing Public Relations

It seemed that in addition to my own necessary familiarisation I should do some public relations work; making a few moves to gain acceptance among criminologists and those

concerned with the justice system. It would, I thought, be useful if I spent some time actually in a Borstal and meeting both staff and inmates in their habitat. So I spent some days in Pollington, Latchmere House, and Oxford. While I could not think that these experiences would improve the quality of my research, it seemed essential to its marketing. When I was working in Folland Aircraft and concerned with modifications I found it most useful to crawl around inside the fuselage and see how the two-d drawings looked in 3-d metal. I avoided quite a few mistakes by this strategy and I also gained some popularity with foremen and charge-hands. Similarly I made it a rule to do at least one day's field interviewing on every survey I directed. This had one clear utility; if supervisors said that a particular question could not be asked, I could say that it could because I had done it. Or put another way, I thought that it was unfair to expect field staff to do anything I was not prepared to do myself.

There was an attendant benefit in my decision to spend time in penal establishments. I was still a Research Grade in the Social Survey -- not a member at this time of the Home Office establishment and hence had not been subject to full security clearance. Before I could enter a prison or Borstal I had to be approved by the Prison Commissioners (it was not until later that the Commission was abolished and the activities incorporated into the Home Office). There were certain advantages in still being something of an outsider at this stage of the project. I was independent of the internal hierarchy. I got to know the Chairman of the Commission, Sir Lionel Fox, on a personal basis. He was seen by prison staff as dour and seemed to inspire awe in all ranks up to and including governors, nonetheless he was greatly respected. He did not waste words. He was one of a very few living V.C.s and he was entitled to many other honours which appeared on official documents appended to his name. He invited me to discuss the proposed research at his home. He had a penthouse in a fashionable part of London. Other residents on lower floors had their name plates in full: "Admiral Sir ... (but no VC!) ..." and so on. The top floor apartment was occupied by "Fox". Once in his home he was, in fact, as easy to relate to as a very human single syllable -- 'Fox'. I liked this change of image coincident with the change from work to non-work. I liked the way he left the trappings of office at Whitehall and I tried to emulate this principle of separation of roles.

In addition to my contacts with the Prison Commissioners, I gained a remote association with the Howard League through the Chairman, the same Sir George Benson. This, in turn, was instrumental in my being privileged to meet the renowned philanthropist, Marjorie Fry, in hospital shortly before she died. She was pleased to hear of the Home Office's recent commitment to research and the Borstal project. Before I departed she asked me to think about the possibility of compensation to victims of crimes of violence. And that is another story which I must tell later (see p. 103).

But to get on with the Borstal story. Only one humorous situation comes to mind in relation to this project. But humour and research are close cousins, so I will tell it. Mannheim was a pleasant personality, but he was almost the stereotypical German. He spoke English with a heavy accent, and was extremely widely read in several languages. In Germany, before he became a refugee, he had been a judge. This fact also spoke to

his being somewhat 'sober'. I tried explaining the statistical (not actuarial!) methods I proposed to work with in my approach to 'prediction', a word which I still resisted using, insisting that we were estimating probabilities or basic risk, not predicting anybody's behaviour. He seemed to have some idea of the 'Philosophy of As If' which, in any event, was due to the German philosopher Vaighinger.

I tried to point out that it was the model which was the important thing. The subject matter was not of overriding importance -- once one had 'got into' the model. In order to illustrate this point I told him that the Borstal problem could be modelled in a way similar to that which I had modelled a problem of the quality and acceptability of kippers. Put simply it is that the final criteria in both Borstal training and kippering is the acceptability of the product by the public. In both cases we have as our sample individuals who exhibit large biological variation; (no two herring/Borstal lads are exactly alike). The processing is largely uncontrolled and 'traditional' rather than technological and hence we might expect variation due to a number of factors which we must try to identify. With kippers these included the age of the herring, how long after being landed before it was frozen, how long it was in the smoke kiln, features of the kiln, and so on and finally, how it came about that it was then a 'kipper' rather than a herring. But 'aftercare' (how stored and cooked) was also important in determining the quality of the product. A more superficial similarity was irreverently suggested to me: both have been caught and become browned off, two-faced, gutless, specimens... Mannheim approved of neither analogy!

To obtain the data base for the appropriate matrices we (i.e. the Social Survey) employed selected members of the field interviewers. They were given one day's intensive training and then had the task of "interviewing" the files. I need not go into any more detail about this study because it is published by H.M.S.O. Titled "Prediction Methods in Relation to Borstal Training".

The publication in 1955 was extremely well received on both sides of the Atlantic. Baroness (Barbara) Wootton was good enough to reveal, with appropriate subtlety, that Hermann had provided the historical and legal background in the first two chapters, and that the remainder was clearly the work of the 'second author'. [12] I had been able to make use of the discriminant function and it had worked remarkably well.

Though the Home Office continues to list the Borstal research as the first project of its Research Unit, it was, in fact, completed while I was still at the Social Survey. True the Home Office put up some of the money and approved of the Survey's participation. In fact I completed one further project before moving to the Home Office. I had no time to complete the write-up which was passed to Chris Scott. This was a study of the information-search strategies of scientists in the larger research establishments. I remember that scientists not only read more scientific publications, but also more non-science and art subjects (particularly music) than did managerial staff of the same status! But that was not what the inquiry was set up to find!

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12 Wootton B (1957) *Social Science and Social Pathology,* Allen and Unwin

Collateral events with the Survey years

The spotlight of my story has been focussed on my work and work related environments. The impact of the work situation upon the home life is not my theme. If it were it would demand skills of insight which I do not possess. However, there are somewhat special reasons for noting some aspects of the background to the work. With some occupations it may be easy to 'stop work'. but when the work demands concentrated thinking, it is difficult. Thus, when if I say something about the off-duty hours, this represents no clean break. Physically one may be off-work but if there is an interesting problem, it will occupy thought even while one is feeding a recalcitrant infant. I cannot, however, recall anything in my home life which impeded my scientific activity, whether in teaching, learned societies, or work. Obviously I had great support and understanding in all these matters. Many of my friends used to complain that their wives did not 'understand' them -- I was well understood and helped. My manuscripts were much improved by the critical reading Barbara gave them whenever she had time to do so.

Some possible effects of residence location.

My first commute was from Hampstead to Adastral House in Kingsway, a matter of two short 'bus trips. Next we moved to Harrow and the commute from Harrow to Baker Street (Social Survey) took about 40 minutes each way. The move to Croxley extended this time to about an hour (10 - 12 minutes for the walk to the station). Then my transfer to the Home Office in Whitehall involved the commute from Croxley to Trafalgar Square station and took 85 minutes at best and could be as much as 2 hours if a connection was missed. A concomitant factor was that travel at off-peak times was slow and the trains infrequent making evening visits inconvenient and my evening meetings at various societies became a problem and I had to be quite selective.

The interaction of home accommodation and education of the children probably had some impact on my work. But at this time interval it is not possible to fit the two 'worlds' together. They were both accommodated. Without doubt the long leaves and the fact that I could largely determine when I was on or off duty facilitated the interface. But to return to the work story.

CHAPTER FOUR: HOME OFFICE DAYS

At this point I would like to suggest that the main features of my research career as of this time (end Survey; begin Home Office) had been strongly influenced by two projects (hearing aids, and medals) and their results. In addition to these two, there was an earlier piece of work in the Royal Air Force days which concerned the measurement of exposure to risk. Of lesser importance, though not altogether routine were two other Surveys, namely the demand for solid fuel and adolescent behaviours. The fuel rationing project was procedurally interesting but did not involve the addition of new methods to those I had used previously in forecasting studies. Other projects were mainly routine and are not worth special note; they are listed in my bibliography. My study of Borstal was partly Survey and partly Home Office and had its own history. This certainly changed the topic to which I applied my methodology, but in terms of innovative research design, as I see it, it was not as significant as the three prior projects, though its impact was certainly greater.

In common with the Medals study, Borstal was also a very low budget project. In each case the 'vote' was around one thousand pounds. Valuable research is not necessarily expensive!.

The main interest was the challenge to develop new methods and formulate research problems with respect to political concerns.. I did not want to devote my time to a particular kind of subject matter. It was the nature of the structure of the research problem which was attractive. Apart from the use of the discriminant function for mixed types of data the degree of originality which characterised the Borstal project was not in its basic design but derived from the orientation towards decision theory. The former is somewhat technical and I leave that aside, but I would emphasise now the philosophical aspects of this work. I decided that we had to focus upon the judge's decision in committing the offender to Borstal and the information I would use would have to be constrained to be that available at the time of sentencing. I could not evaluate Borstal training independently of the judicial decision to award that disposal. The criterion was then the outcome as defined by further proven crimes at set times after release -- when the lad was free to commit further offences. Whether he was or was not on aftercare at the time was irrelevant. The objective was to provide judges with an indication of the likely successful outcome should they decide to send a particular client to Borstal, where this decision was based on information they would be able to elicit at the time of sentence, and, then by making the most effective use of that information.

My design was rejected by many social workers because I accepted 'the opinion of the judge' as the condition for inclusion in my sample. It was represented that some lads might fail because they were not suited to the treatment, and that it was unfair to be critical of Borstal Training on the basis of these kinds of data. My views on the at risk requirement was also questioned. It was argued that the full treatment was for three years and included the period which was spent on aftercare, so a reconviction while on aftercare in the community was not a failure of the treatment, because it had not been

completed. If I was to evaluate Borstal, I should base my evaluation only on those cases who had completed the whole package. It was difficult to persuade many that I was not concerned to evaluate 'treatment' but I was interested in the outcome of a decision. The sentence was the most important feature (ask the accused!) and it made no difference to the offender if his social worker thought he was not suited -- it was the judge's decision that was the correct determinant of the sampling frame. Later decisions might be made and these might be separately considered. In fact on sentence the offenders went first to a Classification Centre (Latchmere House) where they underwent various tests and were allocated to a selected Borstal. With these considerations the project was becoming rather more than the routine assignment I had at first envisaged!

Criminology begins to become of interest.

The writing up of the project became more than a normal survey reporting task. Unfortunately the contract had been agreed in the terms in which I first assessed the amount of work. So a fair number of out-of-office hours were required to produce something worth while. The Home Office decided that the report was of sufficient standing to have it published as a book by HMSO. Sir George was very pleased with the study and the attention it received in the professional journals. He encouraged me to think about joining the Home Office statistical division. I was willing to consider this and applied for re-certification as a member of the statistician class. This was a promotion, statisticians having parity with the administrative class while research officers had parity with the executive class. I had to pass the appropriate examination of the Civil Service Selection Board. My pass certificate was signed by Lord Beveridge and Professor Allen. Reclassification was followed by appointment to the Home Office as statistician with special responsibility for research. I did not foresee that this change of location would result in much change in my kind of activity. The remit of the Home Office was wide enough. The promotion and change did, however, mean that I had added an hour's travel each day to my commute time.

The new appointment entailed moving to an office on the top floor of the Home Office H.Q. building in Whitehall, facing and looking down on to the Cenotaph. If I had been interested in Royal and other parades, this would have been a great bonus. My office was in the Victorian architectural style. The floor had probably been divided because my lighting came from a fanlight and a half moon (well last quarter) window at floor level. Heating was by means of an open fire tended by 'messengers'. Because it was possible that I would be away from the office on business or leave for unspecified periods, this was not lit for me. It was laid with paper, wood and coke and I had to provide the matches and light it. This, of course, did not raise the temperature for at least an hour, and encouraged resort to the canteen for morning coffee while things got going.

Tradition and other trappings

As I came to see it later (after my visit to California) the building was, in parts, ridiculously opulent (the Minister's office and the immediate area around it), while the

remainder was reminiscent of the typical 'servants quarters' of its period. Little had been done to modernise. Only the exterior of the building now exists, so I will give a little more flavour of the setting. The entrance was pomposity petrified in marble and stone, adorned by a uniformed 'messenger', a police officer and a Manx cat. The cat had been a ceremonial gift to a Minister of State (Home Secretary) some unspecified time in the past. It gave the entrance a more welcoming dimension and provided a diversion for those left waiting there for an audience 'upstairs'. Privileged news reporters could get as far as this entrance and the cat became notorious. (Perhaps I should say 'newsworthy' rather than 'notorious', but I don't like cats). Contact with reporters was, of course, banned for all Home Office personnel except those in the Public Relations office. Rarely were reporters permitted into the building. I did not find any of this secrecy at all odd at the time.

History was enacted daily when the despatches to the Palace were collected by a sort of Hansom Cab, drawn by two horses. It was some years later that I realised that I had not only accepted this ritual and other status-conferring symbols (even the size of my office carpet!), but had 'internalised' them. This state of mind was to prove to be some embarrassment when I was being 'received' at a White House meeting with Bob Kennedy on being attached to the Crime Commission, and even then it was the American openness which seemed out of place. But the White House story will be discussed in its chronological place.

Having mentioned carpets, it may not be known that each rank in the civil service has an entitlement to specified square feet of space, and various furniture, including a carpet which at the top of the scale is wall-to-wall, and at the lower end a foot mat under the desk. I was not bothered about the carpet and certainly the absence of a hat rack did not get me worried. However, in the unit we had a Senior Executive Officer who had been posted to us from Royal Household division. He was Stanley Klein. He insisted that I should have my entitlements.

One afternoon, returning somewhat earlier than usual from lunch, I found Stanley standing on his head on the carpet he had engineered for me. When I expressed some surprise because he was no lightweight his comment was typical "Why do you think I organised a full-size carpet for you?" Stan and I enjoyed each other's approach to pomposity! He taught me a number of useful things. One of these experiences was a meeting with an expert safecracker who claimed to be reformed . Stan made arrangements but would not be seen with this person himself. I took my contact to a Soho pub. He was very upset when three young characters entered and he insisted that we immediately find another place. He knew them. They had, apparently just been discharged. He would not be seen with them. "Why not?" I asked. "That's the London airport lot; they got rough". To him, crime needed only skill and roughness (violence or threats) revealed lack of finesse. It may be that this status is now taken over by computer fraudsters.

The Home Office was given authority to spend funds “in the conduct of research into the causes of delinquency and the treatment of offenders, and matters connected therewith” in the Criminal Justice Act of 1948 (Sec: 77 (1)(b). Limited support had been

given to a few projects prior to the establishment of a research position, but if there had been any in-house research prior to 1955 none appear in the public record. When I first took up my post the Research Unit had not yet been started. I had one shared research assistant. It was not until R.A. Butler became Home Secretary that research had a favourable climate for growth. The first two added staff included a statistician and a psychologist -- the former transferred from the War Office and the latter from the Ministry of Health. From then on the Unit continued to grow. Now (1999) there are more than forty research staff involved and the unit has subdivided.

The First Invitation to the United States.

I had been at the Home Office for just over a year when a letter arrived 'out of the blue'. It was an invitation to take an appointment with the Department of Corrections and the Youth Authority of California to set up and manage a research department in association with their statistical services department. It was very attractive and also very mysterious. Why should I be made such an offer? Who could possibly know of my work well enough to give an open contract of this kind? It was many years before I found out how this happened.

I was reluctant to leave the country and the Home Office research unit was at a critical stage of development. It would also have been very disrupting for the family and particularly disturbing to the children's education if they were to come with me and later return to try to pick up their studies. Any major interruption, particularly of Arnold's educational plan at Watford Grammar would have serious consequences at this time. When he had his "O"-levels there would be greater room for manoeuvre. It was also unreasonable for me to consider committing myself to a permanent change of country until I knew more about the situation there. Some academics who had visited, advised me against considering a long time commitment. I approached the Establishment Division with a request for sabbatical leave and at the same time inquired of Sacramento whether an initial period of three months would be helpful during which I could pass on my experience with the British in-house research unit and 'prediction' methodology.. The Home Office were not particularly helpful, but we negotiated unpaid leave for three months. California agreed.

So I went on my own. I was met in New York by Doug Grant who waited patiently for two hours while I underwent a special search. Customs' scanners had picked up 'something nasty' in either my luggage or other bags on the trolley as it passed through their check point. It proved to be the radioactivity of a 'marching compass' -- a wartime surplus I had packed.

Simple when you know! Doug Grant took me via sundry visits to Sacramento, meeting among others on the way, Tim Leary at Harvard who was still highly respected though experimenting with simulating schizophrenia with LSD. Thence to Michigan and a meeting with Hans Toch (remember that name, he will appear in an important role later).

In Sacramento I was introduced to the staff of the embryonic research team, including Don Gottfredson and Jack Bond. Don was a numerate psychologist and he was selected to work with me and develop prediction methods for the California system. Don agreed with me that the term 'prediction' which had been attached to the estimation of probabilities of reconviction was not really appropriate and proposed "Base Expectancy Tables". This term was adopted and gained popular acceptance.

Don was to feature very much in my life in later years. But it was some years before we met again. This visit had a clear link with the circumstances which led directly to my eventually making the United States my 'academic home'. Let us say a temporary goodbye to Don. But before I leave my story of the first visit to the U.S. and Sacramento there is an interesting experience to tell with Jack Bond. Jack was 'into' sports cars and very proud of his Porche. He offered to take me up to Yosemite for a weekend visit. The roads to the mountains were not then (1956) as developed as now and I thought that the experience would be scaring. He certainly drove fast. After about 2 hours driving, he remarked, "That was sloppy. Let's stop for a coffee break". I asked what he meant. His reply, "I clipped a center line on that bend". Driving was not, in California, seen as something one learns after leaving school, but is packaged with school learning. It is taught as a skill not a sport. It is not fun to get away with a risky manoeuvre. I found that attitude towards driving most commendable.

I was impressed also with much of the criminological research going on in California at that time. When I returned to the Home Office I wrote a lengthy report on this. It did not make any impression; well, not a 'good impression'. Crime research had not got much of a head-of-steam at that time, and it was not going to be imported from America !

Scope of research at Home Office

While it was the study of Borstal training which was responsible for my move to the Home Office, the remit of the research statistician was not restricted to crime, prisons or even related issues. The Home Department it will be remembered, dealt with all government business which was not specifically assigned to other departments, among which was the "Carlisle Scheme". When I included this within my earlier list of Home Office responsibilities I did not explain what it was. It might have been somewhat irrelevant at that time; but since it does not now exist and was the sector where my first project in the Home Office was directed I should, perhaps explain.

Requested Research.

The Carlisle Scheme defined a geographic area where the alcoholic beverages trade was under direct state control and administered by the Home Office. There are two different stories which claim to explain why the “drink industry” (pubs, hotels, breweries and distilleries) were nationalised in the north-western border country centring around Carlisle in England and Gretna Green in Scotland. One version is that it was a concession to the total abstinence movement which was particularly strong in

nonconformist churches. This lacks credibility because it was in Wales that this movement was strongest. But here local option schemes were implemented, meaning that local authorities could ban the sale of alcoholic drinks at their discretion. [13]

The other view is that the nationalisation was much more utilitarian. It was put into effect in 1914, that is at the start of the first World War. The Carlisle/Gretna Green area was one of the main locations for the production of arms, and the factories were largely staffed with Irish labourers who it is asserted, are heavy drinkers. This was not necessarily bad so long as it did not effect production of war goods. But it is asserted that drunken habits persisted and production was not secure. (I was told, but did not believe, that the bullets were being put into the shells backwards!). Total prohibition (as in the United States) was not approved and the control afforded by government ownership and management was the option chosen. Perhaps there was also some thought that it was a profitable trade? If this were a history treatise I would find out which, if either, story is supported by documentation. From my point of view the reason for the origin was irrelevant and remains so.

My concern is only that in 1956 no one had got around to denationalising this local industry. Whether the scheme had a different name in Scotland I did not find out; it was probably unnamed.

Some Civil-servant Publicans "Live it up".

An early, if not the first in-house research project commissioned by executive departments of the Home Office was a study of the operations of this nationalised complex. Shortly before I arrived on duty in Whitehall, there had been complaints from residents in Carlisle that the civil-servant pub managers had life styles which could not be sustained on their salaries. A new Jaguar each year, was one feature noted for one manager.

Serving drinks in bars is an opportunity for all kinds of dishonesty which are difficult to identify. For example, drinkers of beer prefer to see some froth on top (known in the trade as 'the collar'). Glasses were designed to hold exactly half or full pints, and froth was not as dense as liquid, thus the customer did not get a full pint in the pint glass. Furthermore beer barrels, being then wooden, tended to shrink with age. When they had shrunk such that they could not hold their full complement, they had to be discarded. Thus, the barrels as received by the publican held more than the nominal amount of beer delivered. (The sizes of glasses and barrels are determined and controlled by the Weights and Measures authority). There was, then, a margin (known as the 'natural surplus') which was due to tolerance in the barrels and the size of the collar. Managers were not entitled to pocket the proceeds arising from either their

13 In addition another system which survives, in part, is the prohibition of Sunday sales of alcoholic drinks. The ice-cream Sundae was invented to overcome this kind of Sunday restriction.

delivery of short measure nor the delivery to them of surplus due to 'tolerances'. They were classified as industrial grade civil servants and paid the negotiated salaries.

The problem was to find out whether any managers were siphoning off for their personal use any major part of the profit derived from the 'surplus'.

This seemed to me to be a good opportunity to use 'prediction methods' in a very different field. If we could derive an equation which fitted the profit figure to sales patterns or other features of the various outlets we could look closely at those individuals who were 'outliers'. The research design was almost identical with that used to pick out interviewers who were suspected of failing to interview all of their assignment correctly. If we could identify the 'outliers' from our equations (a sort of quality control) the Home Office could make further inquiries and perhaps reprimand those who seemed to be filtering off the surplus, but on grounds of 'inefficiency' rather than dishonesty. (Note: the equations did not separate loss due to spillage and 'surplus'; inefficiency and sloppineess had the same characteristics as dishonesty.

The standard accounting system could not be used to convict of fraud or theft or any other criminal offence because the managers were careful enough to return cash to meet the nominal contents of barrels delivered and the sales made. Probability would not stand up to 'beyond reasonable doubt' as a court might decide.

Obtaining data for the model was fun. Obviously I had to visit the scenes of these possible 'crimes'. We had to 'pull' pints (well half-pints) to see what the average 'head' or collar accounted for in fluid measure. We had to estimate legitimate spillage in pouring under busy and relaxed condition. We obtained a good fit to an equation. As a technical aside I might add that it included a term which was identified as a 'suppresser' (a negative sign was required to be given to the sales of spirits). This did not seem reasonable to anyone unfamiliar with regression analysis. How could profit increase while the amount of spirits decreased, and conversely? Accountants and lawyers could not appreciate the mathematical basis for this. It was, of course, due to the different 'patterns of trade' in the different outlets. Pubs in areas where beer sales were more popular than spirits sales were more likely to have a 'working class' trade, whereas the hostelries in the fishing and hunting areas sold less beer proportionally to wines and spirits. The equations took this into account. Explaining precisely how this was arranged to persons whose knowledge of mathematics was limited to adding and subtracting and perhaps the percentages of a balance sheet was almost impossible. I will not try again now!

Never mind! We merely identified the most likely managers to be siphoning off profits by not putting the 'surplus' in the cash register and the Metropolitan Police were able to do the rest by collecting the kind of evidence needed. As one might expect, the worst case was too greedy and was not content with diverting the 'natural surplus' money, he also employed a fictitious barmaid! The Trades' Union defended the accused and once again the story of my research was reflected in Hansard by 'questions in the House'. Some of the Members had some fun by calling the write-up "The Froth Report".

Manning of Fire Engines

Another project concerning fire cover underlined the fact that, at this time, the Home Office Research Unit had a remit with respect to all of the functions of the department (not merely crime and punishment issues). Turntable ladders were designed to operate with a five-person crew. However the drill could accommodate a situation with one person short, but this equipment would be inoperable if two persons were missing. This was statistically an interesting point. The probability of 'no show' (sickness &c) was, of course, a continuous variable, but the resulting availability of the equipment was discrete. From the administrative viewpoint the interest derived from the fact that the case involved the Fire Brigades' Union under the control of a recognised communist named Horner. Suffice it to say that I was present at some interesting meetings during negotiations with Sir Charles Newsome (head of the Home Office) in the chair. Horner's speechmaking was more developed for public performances than technical detailed roundtable negotiating: he was far happier on his feet than seated. This point had, apparently, not escaped the notice of Newsome who very politely insisted that Horner, as everybody else, should address the negotiating sessions from a seated position. Sitting down to speak was for Horner like boxing with one hand tied behind his back. Top civil servants may have difficulties with the philosophy of probability, but otherwise they are smart.

Policy (or is it politics?) and Research Conflict.

It takes some time before reviews appear in the learned journals. While the initial reception of the Borstal study had been positive, it was the later reviewers in the serious journals and particularly in the 'learned journals' in the field in the United States which made the major impact. The next project to come my way was also one which was undertaken because it was requested by the policy department. The department correctly saw the Senior Approved Schools as a somewhat similar 'correctional facility' to the Borstal system. Senior Approved Schools were residential penal establishments very similar to Borstals but were used for the detention of slightly younger age groups. While Borstal training was carried out in two distinct types of institutions -- open and closed -- the Senior Approved Schools were all 'open'.

The results of the Borstal study did not indicate any specific action. It provided decision makers with a risk assessment, but no more. One finding could be read to mean that open treatment was better than closed for the better risks, and the fact that risk could be expressed numerically could (in my view!) have been helpful to Allocation Centre procedures. It was true that the allocation procedures resulted in more of the relatively good risks going to open institutions than would occur from a random allocation, but 'risk' as measured by my equations, did not explain much of the differential. Whether the relatively good risks allocated to closed institutions had other characteristics which explained the decisions was not explored. The 'numbers' (probability/risk measures) were not trusted! The research results, provisional as they were, might have had more meaning if they had been taken in relation to replications of the methods and analyses at a later date. It could then be assessed as to whether the input had similar, higher or

lower risk features and any unstable factors could have been replaced or eliminated from the equations. Unfortunately the publication, resulting reviews, and the impact on the future collection and recording of information in case papers was not investigated. Replications were deemed to be unnecessary. However, a study by Roger Hood (now at Oxford) showed how necessary replication was, but his work was never integrated into Home Office policy. (The weights did indeed need updating).

Many years later there was a considerable drop in the success rate for Borstal Training and political moves were made to close down the system because it was failing. A few persons raised the possibility that the better risks might have been diverted or that, for other reasons, the individuals being sent to Borstal were less suitable.. Put in more rigorous terms, the expected failure rate, given the prior probabilities, might not have deteriorated at all. Indeed the reduction in the success rate for Borstal might have been a consequence of applications of the risk factors identified in the initial research because it identified those whose prior probability of success was good and they may have been seen as '"not requiring Borstal Treatment". But I was then in the United States and I doubt that the sums were done, in any event, Borstal was abolished in favour of Young Offenders Prisons. It seems that even social research has possible undesirable side-effects.

Rather than agree to further research into Borstal, the administration suggested that the same methods might be applied to Senior Approved Schools. This was done but the results were so unpopular that no publication has ever been released. I objected to the censorship but I did not have sufficiently strong ground. I will not suggest sabotage, but merely report the facts. Immediately prior to the arrival of my research assistants to extract data from files at one sampled institution there had been a fire which had destroyed (if nothing else) the sample of case papers from this place. I could not, therefore, claim that the sample was representative! I do not think that the absence of this small subset of cases prejudiced the findings in any way, and the prediction equations were as powerful as for Borstal. There was, however, one very inconvenient finding, which probably was irrelevant, but certainly interesting. It was that it seemed to be adequately demonstrated (by use of these equations) that if relatively good boys are sent to Approved Schools, they ended up worse than 'expected'. The 'expectation' was the calculated risk of recidivism. My theory, and I still think this is true, was that there is contagion in penal institutions. Obviously this finding was politically embarrassing. I remain convinced that was the sole reason for withholding consent to publish the findings. Of course, it was pointed out to me that if I presented the data I would have to report the fire and that too would be an embarrassment to the School staff who had been 'so co-operative'. (Note: Approved Schools were often run by religious establishments -- no further comment). So I was persuaded' not to force the issue of publication. There was, of course, nothing else I could do and remain in office. I still hold to my opinion that the fire had no serious impact on the sample's representativeness, at least not at the level of inference based on proven powerful equations. Political reasons there may have been, but there were no scientific reasons for not publishing the work.

At about this time I was asked by the editor of the weekly magazine NEW SOCIETY (now ceased publication) to prepare an article -- the topic I forget. What I have retained for the information of posterity is the response I received when I recall the objections raised by policy divisions to the draft I submitted, criticising my suggestions that the concept of criminality was continuous from trivial to serious. The official viewpoint was that theft was theft no matter how trivial the item stolen. In fact it was suggested that if I had used paper clips for my own personal use I was in danger of being discharged from the Civil Service. I had to be careful, therefore, in suggesting any gradation of criminality in my article. These comments, together with the suppression of the Approved School research findings adds weight to my argument made earlier that research personnel should not be directly under the control of the government of the day.

Fixing Establishments for Police Stations.

Off the soapbox! A very different but administration-requested project involved the setting of 'establishments' for the Metropolitan Police -- the one police authority which comes directly under the Home Secretary. A considerable time had been spent by a team of experts in visiting police stations (divisions) to work out how many individuals of what ranks and experience were 'needed' for the unit under study. This was setting a figure for the 'establishment' or as they say in universities, 'lines' or staffing entitlements. Some 20 units out of a total of more than a hundred had been completed at considerable cost. I suggested that it should be possible to set up a data matrix and to derive an equation which would fit (map) the determinations subjectively obtained by the experts. That if we could identify such an equation on the basis of the 20 cases so assessed, this equation could be fitted to the remaining units. The system worked well. The equation explained 89% of the variance. Presumably the team of assessors had been reasonably consistent! The 'prediction' of decisions was again to be used much later in the United States Parole Commission Projects and in Sentencing Guidelines. I suppose that these projects were early examples of what are now known as 'expert systems'?

Forecasting the Demand for Punishment!

There were occasions, though rare, when administrative departments requested specific research. One such occasion was quite early in the days of the Unit when we were asked to provide an estimate of the need for Approved School places for the purposes of determining a building programme. This was assigned to me. The data matrices which it had been possible to derive in previous forecasting studies were not possible in this case. Much might depend upon how punitive the climate became in the future. It had to be assumed that the probability of incarceration of youngsters in Approved Schools would not be influenced by political change and that judicial sentencing practice would remain as variable as it was at the time.

The only method I could think of was a time-series. I knew that this was not a reliable method even when one had extremely long series which were based on a constant definition. Only one rule from my past experience appeared useful, namely, spread the load and hope that errors will be uncorrelated. Accordingly rather than simple (or

complex) curve fitting I devised a macro cohort analysis. This meant that I took the crime rate for each birth year for as long a series as I could muster. The results were interesting. Three wave peaks and troughs were found where the excess of delinquency in later years was coincident with the individual passing through their third year during disturbed periods of history, namely World War 1, the great depression or the 2nd World War. There was no indication of any abnormal rates of crime associated with birth dates or first year (babyhood) within these periods. This was not expected because it had been common practice to generalise Bowlby's "maternal deprivation" theory to mean that family difficulties in very early life were a cause of juvenile delinquency. Clearly if there was anything in my data it indicated that 'maternal' deprivation was not involved. My data could only relate to fathers' absence or correlates of that absence..

I was persuaded to publish the findings. I pointed out that a hypothesis which is derived from a data analysis gains no further support from that data. However, it was the hypothesis which interested people, including some of the 'higher ups' in the Home Office. The original purpose of the analysis was forgotten and the data were utilised to make statements about social impacts in childhood.

Certainly inferences from Bowlby's theory were not supported and the main interest turned on the age involved: namely around three years of age. The publication by the Stationery Office under the title "Delinquent Generations" attracted considerable attention and the study was replicated with similar results in Denmark, Poland, New Zealand and perhaps elsewhere where usable data were available. However, some very distinguished statisticians suggested that the results could have been due to a statistical artefact. This is still taken up from time to time. I would be willing to say that the negative point was reasonably demonstrated, that is, Bowlby was not correct. Beyond that I would say only that I find very attractive the idea that early socialisation is more critical than babyhood nurturing. I do not regard it as proven. Nor am I convinced that this issue is worth the cost of research based upon a more satisfactory design.

It is unfortunate that this was one of the most popular and publicised of my projects because in my view it was least rigorous. Press comment on publication was interesting and varied. The Daily Express remarked in its editorial that "government scientists spend hundreds of pounds and many months pouring over masses of data and emerge with a conclusion which is obvious to any layman" (Or words to that effect), Another press editorial comment which I remember more precisely, said, "Clever people" (I liked that bit!) "often spend much effort to arrive at conclusions which simpler minds arrive at directly" The Independent, however, recognised the scientific nature of the analysis, but doubted that the results derived were correct. Thus, for some the findings were accepted without any concern for the evidence and for others, despite the evidence, the findings were suspect. (It was ever so).

Attempted Suicide: A Crime?

While I recall that I had two personal 'missions' for which I awaited an opportunity to set the necessary research in motion, I do not know where the ideas originated. The first was a scheme whereby victims of crimes of violence might receive compensation from the state. The second was the decriminalising of attempted suicide for which (as will become clear later) I had more than the immediately obvious reason. The first opportunity I had related to suicide. The opportunity to begin work on the compensation project came later. I think my previously mentioned visit to Marjorie Fry may have been my source also for the latter idea. I was, in any event, familiar with Durkheim's classical research as well as Sainbury's study of suicide.

I began by examining the statistics. This was in line with my general approach to research and was, of course, a continuation of my style of thinking in the flying safety research. I wanted to establish measures of 'exposure to risk' and find systems for calculating 'rates' which were useful. My prior work on sortie-based accident rates had some parallels. Certainly at this time the Unit (Lodge and myself and perhaps one other) were concerned also about the use of the 'clear-up rate' as a measure of police efficiency; I was later to get into trouble for pointing out the deficiencies of this index. Attempted suicide rates and clear-up figures varied between police forces in ways which we could not explain and clearly represented an unsatisfactory state of affairs. We were also not satisfied with comparative data on crimes of violence. I will have to hold over telling of the development of the compensation case until later, but this was a much more complex matter than the suicide issue.

So, how is it that attempted suicide ceased to be a crime? There was probably much more background than I can now remember. It is probable that my thinking was in line with a contemporary climate of liberal opinion. The Samaritans were already functioning and were concerned to provide help to persons who were likely to do themselves harm. A number of psychiatrists, as I have subsequently learned, were interested in the Samaritans' work. The legal position of this society (and that of any psychiatrists who worked with them) was not at all satisfactory while the 'attempt' (about which they might well be informed) was a criminal act. I may have been aware of this organisation. I have subsequently learned that this Society had an arrangement with the City of London Police and instances of attempted suicide were normally not prosecuted but referred to this Society. Conditions elsewhere were, however, very different. I am not aware of any lobbying on this matter and it was certainly not raised by the 'establishment'. If I was 'lobbied' I do not recall any contacts, and, in any event, such contacts would have rendered me liable to disciplinary action! So I feel some justification in claiming to have been a significant player in the drama by putting into train the operations which led to attempted suicide ceasing to be a police concern.

I was interested in attempted suicide for other reasons than those which may be obvious. Of major significance was the fact that while attempted suicide remained classified as a legal matter (crime, misdemeanour or whatever) it would not be fully accepted as a medical matter. The decriminalisation was concordant with my view on the general philosophy of state intervention in cases of self-inflicted harms or 'morality'

such as abortion and euthanasia. It was my view that Britain overreacted punitively, in general, to disapproved behaviour. There were limits to what could be achieved by making rules. The law and punishment, in my view, gave the wrong image to many behaviours of which attempted suicide, whether it was committed as a 'cry for help' or with firm intent to die was one example.

My pursuit of this philosophy was to get me into serious trouble when I pressed this analysis in relation to the use of a number of 'substances' (mind altering drugs). Some were also already illegal and others were being added to the list of 'prohibited substances'. The grounds for this action was asserted to be the welfare of the customer. The attempt to use legal procedures in the medical sphere of expertise seemed to me to be unsatisfactory. While there was little doubt that most of the 'prohibited substances' were harmful, and we might even assume them to be so harmful that the taking of them was tantamount to slow suicide, the law did not seem to be an appropriate medium for remedial action. It might well be a medical (psychological or mental health) matter, but not one which could be effectively addressed by the courts. Doing good for individuals was not a police matter. It could be an excuse for erosion of civil liberties. The decriminalising of attempted suicide would be one step towards making the situation somewhat tidier -- logically if not also administratively.

The drug issue is, of course, a much more complex matter of use and supply and, in particular, the medical model does not adequately incorporate the economic issues. The suicide analogy is simplistic, but not irrelevant. I will return to this subject later because it eventually reached a climax which led to my departure not only from the Home Office but also from England! But I must now finish off the story of attempted suicide.

I think I mentioned that as a statistician I had parity with the administrative classes and could originate policy issues. Attempted suicide was a policy issue. I instructed Precedent Office to extract all files which had established precedent in the matter of attempted suicide. At the same time we examined the statistics of police action to ascertain how cases were dealt with in practice. Many laws just fall into disuse rather than being repealed. (There is one in Georgia which prohibits eating peanuts in church and in D.C, one which requires all taxi drivers to have a shovel and broom in their vehicle -- guess why). Perhaps the attempted suicide law was interpreted in a reasonable way and there was no need for change? What I found was extreme variation in the action taken in different police districts from strict legalistic interpretation to reference to other authorities (case 'otherwise dealt with'). The lack of consistent interpretation gave me a chance to prepare materials for submission 'upstairs'. But that would not be adequate to get 'attempts' decriminalised. (There was little practical point in getting successful attempts considered). I would have to argue against precedent. Precedent may not be changed without sound cause.

Shortly a huge pile of files arrived on my desk, laid out in historical sequence. About halfway down the pile was a note by Winston Churchill when he was Home Secretary. He saw no reason to depart from established precedent and accepted the argument supporting the illegality of the act. To take or attempt to take one's own life was to rob or seek to rob His/Her Majesty of the life of one of His/Her subjects. While it is true

that the British are 'subjects', not citizens, this was not an acceptable 20th-century lemma. This precedent had to be set aside. It did not prove too difficult.

My argument was that the earlier doctrinal basis for punishment was then under challenge and that the extreme variation in police practice was a bad thing. I argued that it was a simple matter to remove the crime of attempted suicide from Statute and that there were other agencies than the police who could act in such cases where action additional to medical attention was called for. In due course attempted suicide was deleted from the criminal code. I can claim to have been instrumental in this, though perhaps I was saying only that which had become accepted opinion. But most certainly the current drift of opinion on attempted suicide did not include slow suicide where 'drugs' were concerned! My viewpoint is still rejected!

Liaison with External Research Organisations

Meanwhile under the Ministerial direction of Home Secretary Butler, the research unit was becoming almost solely concerned with criminal justice matters. Other divisions of the Home Office did not request research assistance, and there was more than enough pressure to take up criminological issues. Butler was particularly interested in these. A White Paper was published associated with his name to which the Home Office Research Unit (now somewhat larger than the three originals) had considerable input.

Associations with the Universities.

It will be recalled that interests in crime/delinquency/prisons (and “matters connected therewith”!) were distributed over many organisations each with a slightly different remit. While the Home Office could rule out some of these organisations on grounds of their party political involvements, there were still many who had claims on the funds covered by the 1948 Act. Among the several organisations which were provided with funds for research to be carried out by individuals associated were two major studies of prison (Bristol by Fred Emery in association with the Tavistock Institute, and Pentonville by Terrance Morris of LSE). Roger Hood who at that time was associated with Mannheim at the LSE carried out a follow-up study of Borstal, and Leon Radzinowicz at Cambridge was supported in respect of a number of studies, including one on sex offenders. Richard Hauser was funded for a study of homosexuality. From this sample list it will be evident that the Home Office had a problem in allocating money to the diverse interests and being able to argue good reasons for its selection.

The situation was made more difficult than necessary for both the administration and ourselves by reason of the attitudes and perhaps personalities of the "three great British criminologists". To say that they would not cooperate was an understatement! Even the published literature makes clear their lack of respect for each other's work. Refugees from central Europe seem, in general, to be highly competitive and for this to be expressed most pointedly when other refugees are involved! A later refugee from Hungary (Stephen Schafer) was not able to gain any assistance from members of his discipline. We had to tread most carefully if we were to remain on reasonable terms

with “the criminologists”. My later experiences in academic life made it clear that our three (or four?) criminologists were not unduly atypical. At Oxford, Grûnhût, probably because of his health and poor vision remained outside this turmoil. The Home Office did provide him with an assistant for a short time but until his somewhat premature death he beavered away mainly on his own in his upper room at the Bodleian.

Another task which fell largely to me was the overview of research which had been contracted out.. One incident which arose in connection with one such project involving prisons throws light on the culture of that time. In minuting files (making confidential notes on the specific page) one would always address the individual by their last name only. Thus, I would minute a file to Lodge, or Cubbon, however informal notes not for the record might be passed addressed to "Mr". I was told that this was a tradition which derived from the fact that most high level civil servants were ex-public school boys and that it was the custom in such schools to use last names, (with suffix "junior" if two members of the same family were to be distinguished). One day I received a serious complaint against a research worker (an LSE doctoral student who was interviewing in prisons in connection with a sponsored project). He was insulting the prison officers ("guards"!). He was not calling them "Mr"! To them the "Mr" was a necessary symbol of respect, for the assistant governor it would have been incorrect and probably resented. Assistant governors were akin to the 'officer class' in the military. How the distinction between cases where it was or was not appropriate to use “Mr” was established I do not know. It is most likely that this is no longer practised. I think the “old guard” would find it difficult to sort out a “Ms, Miss and Mrs” protocol!

In-house v. Contract Research

Perhaps the question of the merits of organising research “in house” as against contracting or making grants in aid was given adequate consideration at higher levels. If so, the details of any such debate did not filter down to my level. I think that the current balance emerged as things developed in the typical “British” fashion. I have no criticism of the ways it worked out in the end.

The diversity of approach to funding did not seem to raise any major problems, and the quality of the “in house” work has been recognised world-wide. However around 1958 there developed the idea that a body with a crime mission was required to which the Home Office might look for training and certification. It was recognised that quality teaching at the level envisaged would require a research base. The Home Secretary gave this his personal support and the idea was also warmly received by the Chairman of Public Accounts (Sir George Benson). The question of location was limited, namely, in alphabetical order, Cambridge, London, and Oxford. I opined that a British Institute of Criminology should be located in London. This was not an academic point but it seemed reasonable to have the policy-related discipline with strong Home Office associations at the centre of government. (I think Radzinowicz must have known this and came to regard me as, well, unhelpful). Though Mannheim did not have many years of service before he was due to retire, an approach was made to London University to ascertain whether they would house such an institute. The idea was rejected as a decision in

principle against “institutes”, or more institutes of any kind. It seems safe to infer that the view taken by London University was that the setting up of "institutes" was eroding academic quality . I do not think that the subject of criminology was necessarily unattractive.

The Cambridge Institute

While the Home Office continued to fund specific projects to a variety of bodies, the idea of establishing an “Institute of Criminology” persisted. The fact that London University could or would not provide house room did not reduce official interest to proceed. At this point, Cambridge took up the cause. Discussions took place at the highest level, and Leon Radzinowicz was appointed and an Institute established. I was not party to these discussions in any detail. I saw no academic objection to the siting at Cambridge though I would have liked to have seen a more behavioural science orientation. The dominance of law-based criminology was, it seems, being reinforced by locating the Institute in such a prestigious law faculty.

Home Office policy at the administrative (political?) level was never clear to me. Why funds were disbursed in the way they were between the main competing university departments was not determined by the research staff, though we reviewed any proposed research designs and commented on the budget. Official histories of the establishment of the Institute have been published. All official histories are official! I will leave this matter at that point.

As it happened, both the in-house and the grant funded research increased beyond all anticipation. However, the in house element increased far more rapidly and reached a much greater proportion than any one external organisation. Of course, the Research Unit did not undertake training, but the environment made it possible to employ graduates from a variety of disciplines and to develop team work. Official research staff had no teaching commitments and promotion did not depend upon demonstrated individual scholarship and publication.

In-house research has its own kinds of problems and external organisations undertaking research for official bodies are dependent upon the nature of the contract or grant. High level administrators have no experience of ‘the culture of research’ and tend to treat the research output as subject to the same rules or precedents that apply to other departmental business. It was, probably, conflict with these traditions which led to some early difficulties with the publishing of my own research findings.

Eel Pie and Arthur. Inoculation against evil influences?

While my work at the Survey attracted various persons to contact me on a variety of issues, these were mainly statistical. The Home Office brought a different set of persons who wished to make contact for their own reasons. Many were routine concerns. One, however, was far from routine. This was 'Arthur' whose name I dropped earlier. Unlike Blacklock, Arthur was driven by intuition rather than logic. Nonetheless he was more realistic and much more concerned with real life and equally as innovative. His focus of

concern was the disapproved behaviour and problems of some young people. His last name was Chisnell, but Arthur to all. I do not know why he should have sought me out. The connection was through Gordon Pask. who probably became involved with Arthur by reason of living in Richmond.

Arthur would have been accepted today as an imaginative 'outworker' and probably also accepted by legitimate social work authorities. He was ahead of his time, outworkers had not been invented, or perhaps they were his invention. His formula for helping youth who were probably going the wrong way was not that of 'street worker'. Arthur did not go out to find his problem kids, he wanted to attract them to him. His clientele would identify themselves if he provided the appropriate kind of 'bait'. This had to be such as that it would be taken only or mainly by the kinds of youth who needed help which he was able to organise. First he had to create (rather than find) an environment which could be given an image attractive to his target clients. To this end he set up Eel Pie Island Jazz Club. He provided a stage where bands which were popular with the 'youth culture' could play, and the current popular dance styles could be expressed, or indeed, developed. He also had to provide alcoholic drinks, but limited this to beers. Some bands which later achieved international recognition were provided with early support by Arthur on Eel Pie Island. Arthur (he might not agree) went out of his way to give Eel Pie Island a bad name with the 'establishment', though he had some associations with the Establishment' night club. He wanted it to be the kind of place where a teenage girl who had just had a row with Mum would think of going for a bit of real sin. But the 'fly trap' for potentially wayward youth was not quite what (he hoped) it seemed to those who attended.

Arthur had developed his own youth culture; kids who were prepared to 'go so far' and no further. He also had a back-up of 'contacts'. The lone girl arriving would have to join the club and give a name and address. As she was passed into the dance hall, Arthur would signal unobtrusively to one of his selected young folks. The person so signalled was to keep an eye on the new entrant and ensure that no real harm came to her, but certainly to stay out of the way unless occasion to act should occur. These helpers were not obviously trying to be helpful but rather to act as guardians of extreme boundaries of harm. Most were young men or women who had first come to Eel Pie Island for quite different reasons.

It was Arthur's strategy (for which he had developed a theory as well as practice) to try to introduce those who had similarities. [14]Most of his team were probably 'students' (with a heavy preponderance of 'art' students) who could mix well and who enjoyed the *with-it* environment of the Island. He would often introduce persons with a phrase such as, "I think you will find ... fun to be with.. He/she is, like you, interested in ... " However the introductions would involve one of his 'workers'.

Contacts were Arthur's stock-in-trade. In addition to mixing and matching the

14 It is interesting to note the analogy provided by my style of doing carpentry. I noted that my method was not to seek strength in the structure, but in a resolution of forces. Arthur sought a resolution of forces by counterbalancing personalities rather than bits of timber.

participants in the Club he made a point of getting to know people in authority who could help the young people who showed up at the Island. He persuaded as many as he could to visit the Island and meet some of those who used to hang around the 'membership desk'. When the situation was right he would introduce the facilitating contact to the individual. He contacted me at the Home Office. So far as I know I was able to help at least one young man, who had left home and was in difficulties with an academic father; it transpired that he wrote well. He was interested in journalism so I was able to facilitate his obtaining a scholarship to Chicago. He is now a broadcaster. He may not wish the disclosure of his background, so he had better remain anonymous.

Arthur knew many members of Parliament and took a strong emotional interest in the Home Office Research Unit and the difficulties it sometimes had with politicians. For example he knew of my experiences with the car-parking generation study and the approved school project. But he also took a poor view of my attraction to the U.S.A. and my desire to find somewhere where research was less shackled than in the HORU. He feared that I might emigrate. Eventually, of course, he proved correct, but that was many years later and after my stint in Japan. He exploited my 'sore point' and managed to extract a promise from me that I would not consider leaving the Home Office if the conditions of service could be changed so that the scientific civil service was more removed from political interests.

It was expected that a Labour government would be elected. Arthur arranged for me to meet several MPs. Austin Albu I recall was one. But it was the meeting he arranged with Peter Shore which was to determine the outcome. I recall our discussion in a taxi near Downing Street. Shore was at least honest. He pointed out, somewhat lacking in originality, that information was power. When they were in office, they had their hands on that power and it was too bad that, when they were not in office, they had to make do as best they could. So politics is a power game no matter who plays it. I left on my second leave-without-pay sabbatical in the United States soon after this disappointing outcome of my top-level lobbying. I suppose that even this discussion would have been out-of-order. If I had stayed around and it came to the notice of 'establishments' I would have been reprimanded. I lost touch with Arthur when I took up my overseas appointment. It seems that he 'vanished' fairly soon after the enforced closure of the Eel Pie Island club.

Policy relevant controls

I have told of one of the first criminological projects -- the study of Approved Schools. It may have been in response to my report on my visit to the U.S. when it was made clear to me that my being in a policy-making department conferred status but it also meant that I had different responsibilities from those in research institutions or academics. It was absolutely unheard of for a civil servant to cause his Minister any embarrassment (in public). Somehow or another this principle was interpreted to mean that all publications had to be approved by the representatives of the 'front office'.

This ruling had a far more powerful effect than the persuasion not to publish my Approved School report -- the study which suggested the idea of 'contamination' in

penal establishments. It was never suggested specifically that anyone would find this an embarrassing finding. While I reluctantly accepted the suppression of the specific findings of this one study, I was less prepared to accept the need for a general censorship of research.

Unfortunately I could not get the support of the actuary who headed the statistics division. He was always anxious, too anxious, to go along with the establishment. While he professed to be in favour of publication of research as a general policy, the views of policy divisions had to be considered. His acceptance of the prohibition on the publication of 'embarrassing' reports (where the definition of embarrassment was made by 'front office') seemed to me to be getting too close to political considerations. The surveillance of publications by non-research departments and the rationale for this began to seem more onerous: shots across my bows. It was particularly disturbing to have this ruling expressed with such enthusiasm by an individual who had claims to statistical sophistication and a research ethic. I had expected support for an open policy for publication similar to that of Moss and the Social Survey, and as my report had indicated, in the United States.

So I began to think of building some protection, in addition to that afforded by my status with the Royal Statistical Society. It seemed useful to try to set up some means whereby I might avoid the Official Secrets Act, or at least that others might be able to release information which should be published. I represented that the department should consider offering 'fellowships' to American academics whose work in criminology had achieved international distinction. This ploy worked: for a while.

Accusations and 'Security'

Under the research visitor scheme (which, of course, cost the department nothing) we had with us Professor Don Garrity (later President of San Francisco State) and Bob Smith (who developed the probation subsidy scheme in California). We provided facilities for these visitors to collect data from prisons and probation services for their research. I also found it possible to enlist the advice of Don Garrity in the interviewing procedure for new recruits. He was able to throw some light on the way in which 'security forces' operate between countries. Perhaps the person concerned should not be identified because the events might still be 'used against her'. In our interview a Ms XX was considered to be a very good prospect. She was placed at the top of the list of candidates we wished to employ. However, before she began work we had an order from 'security' to say that she was not acceptable. No reasons were given: reasons are never given -- it's part of the British way of life! Garrity thought that was 'unconstitutional', one should have a right to know the 'charge' and to face one's accusers. He also recalled that at Berkeley, XX had been rounded up together with several hundred students by the local police when a 'free speech' demonstration had taken place. It was not claimed that she had taken part, but merely she was in the vicinity with many other students. She had not been detained nor questioned, but she had her name and address 'taken down'. This mass roundup of students was much

criticised in the press and California Senate. It was an overreaction of the police which was not repeated.

The rest is conjecture! However, it seems that this information was passed to the security services (MI6) in this country and was not amended. The fact that Garrity was able to provide this information enabled me to make representations to the effect that the failure to grant 'security clearance' was not justified. Of course, no admission was forthcoming to confirm our beliefs, but a limited clearance was granted. She was not to be admitted to any sector of the Home Office where civil defence matters were dealt with. None of us in the Research Unit knew where these places were and certainly had no desire to roam there ourselves, cleared or uncleared. Ms XX did work in the Unit for a short time, but then secured a university appointment.

While on the subject of officious activity I had some troubles myself. During my first visit to the U.S.A. I had given a lecture in Los Angeles, at U.S.C. or U.C.L.A. I reported on my work on the attempt to obtain statistically useful data, and had apparently drawn attention to the fact that 'crimes known' and 'clear up' rates were not particularly satisfactory indicators. Victim surveys were not to be 'invented' until many years later. I stated that 'crimes known' was not particularly apt, since crimes could not be known unless they were reported. But many crimes reported were considered to be 'unfounded' and were not included in the counts. The 'clear-up' rate was also capable of being interpreted in various ways. For amusement I illustrated my points by showing how, without actually being dishonest, a police chief could vary the 'clear-up' rate by several percentage points either up or down, merely by use of valid interpretations of law. In particular the rate did not discriminate between crimes which were easy to clear up and those which were difficult. I may even have suggested that it would be unsatisfactory from the public viewpoint if the police were to seek to maximise their 'clear-up' rate -- they could merely maximise the 'easy pinches'. This was not exciting stuff -- indeed it was trivial, and obviously I went on to discuss problems of 'risk measurement' and other statistical matters of slightly more significance.

Disloyal again! Really?

It seems that in the audience that evening was a certain chief constable (from somewhere in England!) He wrote to the Home Secretary (fortunately R.A.B. Butler) complaining that a civil servant had been releasing official information and making defamatory statements about the British police (which everybody knew were the best and least corruptible of any in the world). More seriously his accusation amounted to a charge that I had breached the Official Secrets Act and that 'action should be taken'. I was required to appear in the 'front office'. I was asked several questions as to precisely what I had said. I had my notes. The P.U.S.S. merely remarked that what I had said was so far as he knew completely true and what did I want him to do about the accusation. Would I be willing to forget the whole thing if he tore up the letter forthwith? I saw no reason to pursue the matter further, though, for a chief who received much public acclaim it did seem that I was behaving with more 'charity' than he.

I doubt that I would have survived this challenge if there had been a different Home Secretary or if the issue had not reached that level. Butler was sufficiently intelligent to know that what I had said was correct and he was not about to defend the sensitivities of a pompous policeman.

Victims of Violence.

I am reasonably certain that it was from Marjorie Fry that I picked up the idea of the state paying compensation to victims of crimes of violence. The official viewpoint was that the criminal courts would look after the criminal aspects of any violent crimes and the victim had resort to the civil courts for damages. For almost all cases it would, of course, be futile for the victim to go to the civil courts because the offender would be unable to make any payments. The precise logic which held that the state should, therefore, compensate the victim was not clear, though some expressed the view that the state had assumed the role of protector of loyal subjects by removal of approval for private revenge or duelling. I awaited an opportunity to move into this area. The precise sequence of events within and without the office are not remembered.

My concern with statistics led me to note that the trends for the categories of crimes of violence against the person had a very peculiar time trend. The total number of recorded crimes against the person (Grievous Bodily harm and Actual Bodily Harm (GBH and ABH) had increased, while at the same time lesser categories of assault had decreased. The legal definitions of these categories had not changed. It was, I thought, unreasonable to suppose that if (A) hit (B) in more recent years he had hit (B) harder or with a weapon, because weapon use was separately covered in the law. It seemed more likely that public (and judicial) attitudes towards violence against the person had become less tolerant and lesser harms were progressively being seen to be more appropriately classified in the more serious categories. The figures for 'violence against the person' were quoted in the press and the 'increase' in serious crime was assumed. This seemed to me to be statistically unsound. We needed measures which were less liable to subjective assessment between categories and which might be used for comparisons over time. A major difficulty with the definition of crime is the legal concept of 'intent'. Intent is not a statistical datum!

It was with some difficulty that I was able to persuade my superiors that it was worth my effort to design a project and request police time in investigating violent crime from the perspective of its consequences for the victim.

Eventually I was able to obtain a representative sample of all violent crimes (both indictable and non-indictable) and misdemeanours. For these the police were to provide a description of the physical harm inflicted; such as whether the victim was 'treated and discharged' or detained in hospital or lost time from work. Whether injuries are inflicted intentionally (criminal) or accidentally, cannot be identified from the actual details of the victim's condition. Even a person injured by gunshot might have been attempting suicide. We could remove from the individual records of injuries all indications of the fact that the injury had been due to a criminal act. We then arranged

to pass the sample to the Industrial Injuries Compensation Board for assessment as to the amount of money they would award in the event of an injury at work having the impact noted. This money sum estimate was primarily intended to provide a better measure of seriousness than the classification in law. Measures of this kind could be compared both over time and location, and adjustments could be made for the changing value of money. There was also a base for useful comparisons in the compensation assessed in cases not due to crime.

It was possible to gross up the estimates of compensation which victims of crime would have received if compensated according to that scale. It transpired that the total sum of compensation which would match the crime figures at the time was remarkably small. This was, of course, due mainly to the scale at that time which the Board had determined. The civil courts would award, on average, much higher sums for the same suffering.

The results of this research were reported to the Attorney General of New Zealand (Dr. Robson) by an intern from that country (Frances Baker) who was working in the Research Unit at that time. It seems that in the light of our data, Attorney General Robson drafted a Bill to provide compensation to victims of crimes of violence in New Zealand. This Bill was passed by the legislature and New Zealand was the first country to make provision of this kind.

The idea was not so readily accepted in Britain. Here the legal establishment put forward many objections. I probably remember the least reasonable! People could throw themselves in front a bus and get injured, or engage in fights and not take sufficient concern as to the chance of injury. These were put forward as serious objections. In sum, it would be the undeserving poor who might benefit. Poverty was acceptable, but being undeserving was not.

Chance and a foreign revolution intervene.

By some means, not now recalled, the idea of compensation by the state came to be packaged with the idea of requiring the offender to make restitution to the victim. There seems to have been a lot of that not uncommon commodity, plain and simple muddle around! I think that the two concepts must have been somewhat confused. It was, in any event, clear that if the idea of compensation/restitution was to be acceptable in this country the legal niceties had to be addressed. While I was not impressed by the objections to the idea of compensation, I was not a lawyer and had no credibility in that sector. It was by chance (and the revolution in Hungary) that just at this point a refugee came to the office begging for work which was in accord with his background in his home country. The revolution had caused considerable suffering to academics, particularly in the departments of law. There was precedent for refugees from Europe to gain appointments in criminology in Britain! Perhaps being a good lawyer, Schafer was aware of these precedents. If all three distinguished British criminologists were ex-refugees from Europe -- why not a fourth!

I took the opportunity of trying to interest Stephen Schafer in the compensation/restitution idea. What was needed to satisfy our establishment was a distinguished legal authority to provide an apologia. Schafer was willing -- he would have done almost anything: he was on 'the bread line'. He was willing to write up a research application to investigate the historical and legal background to victim compensation. We worked on his draft. Barbara had a challenging time sorting out his tortuous florid European style and putting the case in English. I added some civil-service jargon, put it all on a file, appended minutes noting my support, and sent the file 'upstairs'.

Lodge (Director of the Unit) initially supported the idea, not because he was keen on the concept of compensation but because he wished to help a refugee from Hungary. He passed the file upwards and a meeting was set up (I was not present) to discuss the plan. When it was not well received, Lodge seems to have not taken up a defence of the project but merely reported back to me the fact that the meeting had been unwilling to proceed. He asked whether I could suggest any other ideas which might involve Schafer. I did not favour dropping the matter without a fight., so finding something else for Schafer to do was not my priority! Lodge did not put his views on the file, but sent me a loose note . It seemed courteous to replied in similar form, though this seemed slightly `irregular'. I retained my response after it was returned to me with a further note appended. I should now disclose that when I departed from the Home Office I took with me some of the personal notes which Lodge and I exchanged on this matter. It had been his choice not to place these on the record. However, perhaps because the "record was not informed" by official file annotation, various versions of the origin of the Victim Compensation Board have appeared. It seems not unreasonable to reveal the actual history. For this reason I copy the contents of my initial note. Rather than reproduce this by photocopy I transcribe below.: it will be easier to read. -

Content of initial note in exchange regarding Victim Compensation.

> "Mr Lodge;
>
> I cannot agree with Mr Cubbon's view of Dr Schafer's project. The aspects of compensation and restitution which might be involved in Schafer's sample would not be viewed only from the point of view of continental law (is there such a thing?) but from the point of view of a criminologist and legal philosopher having no strong ties with any legal system. I fail to see why Schafer's " ...(?)... with legal problem" should be useless. Schafer has experienced major changes in legal systems within Hungary and presumably the implementation of the S of S views on restitution would require changes, both in the law and in legal philosophy. A person who has experienced at first hand the difficulties of putting into effect a changed legal philosophy should be able to express useful views about likely problems and ways of meeting them.
>
> Mr Cubbon in his penultimate paragraph seems to discount "public airing". I should think that an informed "public airing" based on a sample of cases and an unbiased judgement as seems likely to be provided by Dr Schaffer had much to

commend it. Without public opinion to support the concept of restitution there us little chance of making headway . Even more important is the fact that until such a study is made we have no means of guessing what are the essential concomitants of such a policy.

Is it worth working out a more detailed plan in the present climate of opinion?

L.T.W. 29/1/58"

Lodge's handwriting is rather more clear, and I reproduce his note in Figure 5.

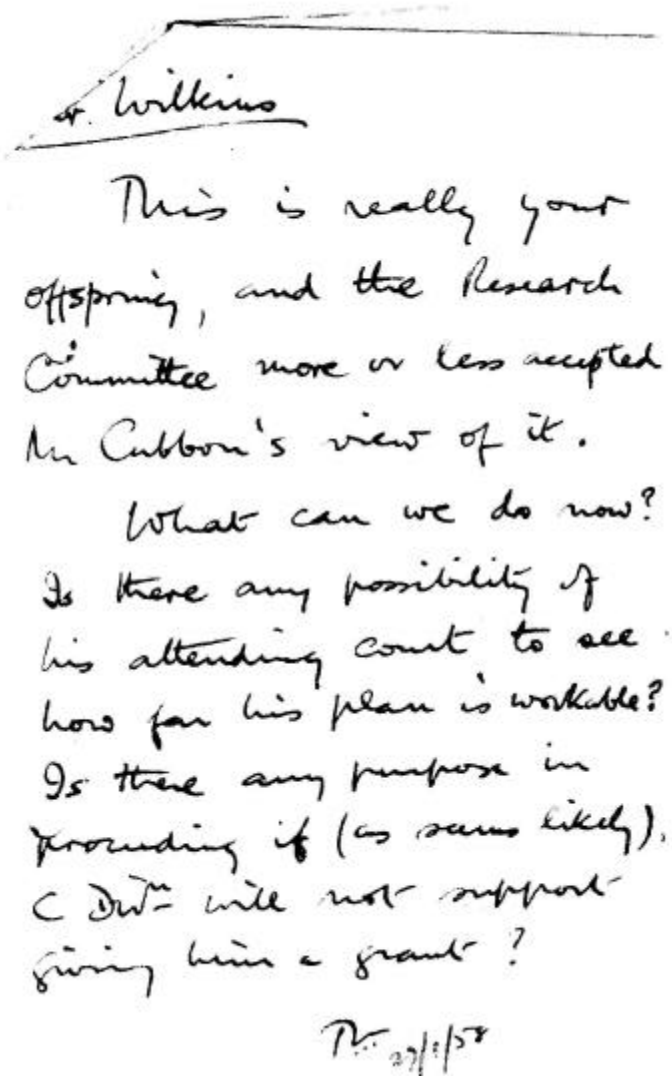


Figure 5. A note from Tom Lodge 29/1/52.

The project had almost foundered when Tom Lodge sent me a further note asking me whether I wanted to push the case and leaving it to me to take up the matter with "C" Division.

I took up the challenge and managed to convince Brian Cubbon that the project was worth the money and would not bring the Office into disrepute. In the end Schafer wrote up his work as a research report and Barbara assisted him to put it into shape for submission to publishers. It was later published as a book and Schafer was established as a "Victimologist". On this reputation (and perhaps his background in Hungary too) he obtained a chair in criminology and migrated to the United States.

Eventually a British compensation scheme was set up, but it was hedged with all kinds of legal constraints. The cost of administration probably exceeded the money paid out! But that fact gave no concern because the authorities were sure that no one who was not entitled received anything!

Opportunities and Risk

A very different project also seems to have had far-reaching effects. This project which had probably derived from my prior interest in valid measures of risk was of more general significance. I refer to the work which I began in the R.A.F. with the sortie-based flying accident rate. I was not satisfied that the base for crime rates for all and any crime category was the population. For example, while the risk of a crime against the person might reasonably be related to the number of persons who could become victims, this base was less logical for crimes such as theft of or from motor vehicles. The analysis of motor vehicle theft when considered against the number of licensed vehicles had remained almost constant while the official crime rate of motor vehicle theft had increased rapidly. This finding is, of course, related to the recently developed theory of 'crime as opportunity'. However, as a statistician my interest was limited at that time to providing better measures of risk.

I was at the Home Office from 1955 until 1964 and during these years, in addition to the Borstal study, I published a number of papers and one book. It is not of interest to the general reader to go over these contributions nor to discuss the many administrative duties which were also involved, such as drafting replies to Parliamentary Questions. I have noted the research which was of some interest but unpublished, such as the Carlisle work, fire engines and police establishment studies. One other research of some non-technical interest was a time-and-motion study of the work of probation officers. It was not unreasonable to say that it showed that some case workers needed their cases more than their cases needed them.

President's Commission Visit.

In 1962 I received another invitation to the United States. This was to work for a year with the President's Commission on Youth -- the President being Kennedy. This time it was possible to take my family with me. The Ford Foundation provided adequate

financial support and travel was also covered. I made a number of inquiries as to the education available in areas within commute distance of my main base, which was to be at the Department of Health and Welfare on the Mall in Washington D.C.

Bethesda Chevy Chase was voted best area by both British diplomatic types and by American research workers who had been on the Social Survey staff or visited the Department. So we managed to arrange to rent a house in Underwood Street, just off Connecticut Avenue. A good 'bus service enabled me to commute to H.E.W. when this was required and this combined with the express intercity train service to Philadelphia enabled me to give seminars at Temple University in the Department of Sociology.

This visit was noteworthy for an incident which encapsulated the major differences between Whitehall and Washington governmental public relations policy. Soon after my arrival I was to be 'received' at the White House and to be formally introduced to the Commission top personnel, including Robert Kennedy. I duly attended the function which was 'low key'. The room was on the major corridor of Presidential offices and when we broke up from the reception I was asked if I was willing to talk to the press. I was a guest and I saw no reason not to agree. I assumed that since an official had made this request all necessary precautions had been taken. In the Home Office I would not have had direct access to reporters but the department would have prepared handouts from my briefings.

I was escorted two doors down from the reception room and taken to a room where there were several persons with communications equipment of various kinds. One, apparently important person came up to me and introduced himself by his name. He began asking questions and I became somewhat apprehensive. I thought I should clear up the security issue. So I paused and enquired whether I was correct in assuming that he was a member of the White House public relations staff. He seemed rather annoyed and with some dignity and force informed me that he was "Reuters". I was so shocked that I lost my composure, and exclaimed, "Christ! What, in here?". The press presence inside the White House and on the corridors of power was unbelievable. This and other experiences served to give me more insight into the Watergate scandals and to be somewhat dismayed that in the UK. any similar misbehaviours by the powerful could not have been pursued by the press. In Britain there was no tradition of investigative reporting which went much beyond the level of gossip columnists. (The present situation is better in that the television media have adopted investigative journalism techniques and Parliament is beginning to open up government information. But we still do not have the freedom of information which characterises the U.S. system)

This visit from the Home office viewpoint was sabbatical leave, without pay. I could interpret this as a temporary release from the strictures of the civil service and the need to have all public pronouncements 'cleared' before publication. If nothing more, this was a time consuming device which often was adequate to prevent any release of the information. It could always be claimed that by the time it was published, the situation had changed (been corrected!). I had twelve months in which to write and publish a work I had long had in mind -- a largely theoretical piece which I realised in the book, Social Deviance.

Authorship under difficult conditions

I took the opportunity afforded by freedom from supervision to assemble some thoughts on crime and the criminal law and to prepare a script for rapid publication. With the use of an electric typewriter in my bedroom and plenty of erasable bond paper (paper which enabled corrections to errors with an ordinary eraser) I managed to complete the work and submit to a publisher and receive agreement to publish before I was again subject to the Home Office administrative constrictions. Social Deviance was published by Tavistock in Britain and by Prentice-Hall in the United States simultaneously. So, if the Office had tried to interfere they might have been able to bar Tavistock, but they could not control Prentice-Hall. This was one which I won! However, I am sure that my popularity with the administration was diminished thereby!

The writing conditions were by no means ideal. A family of four children in a foreign environment made extremely heavy demands on Barbara. Ferrying kids, ours and their friends, took up much of her time. So this book shows evidence of some haste, not in formulating the ideas which had been simmering for some time, but in the style of presentation. A point reviewers noted. But they also noted the 'scholarship' with strong approval, particularly the concept of 'deviance amplification'. This idea 'took off' as a widely accepted theory. The work also expressed some theoretical positions on the use of 'prohibited substances'. As will emerge later this was the feature which would have ensured censorship if it had been possible. My haste was justified, and despite this, I think I can say that some bits are well written.

Family in Washington

I remarked quite early in this work that research work was not something one could keep separate from home and leisure, while I have tried to avoid boring the reader with family details which might illustrate the impact of the two worlds. It will be self-evident that spending a year on sabbatical leave overseas is an event with wide-ranging consequences. My year based on Washington was no exception. It is tempting to say something about this, but to do so would breach my criteria, I can trace no direct association with research thought or activity. The impact was greatest on the children's education and we found ourselves committed to considerable commuting across the Atlantic; a pattern that was to persist for the duration of their education, a period of ten years. We were fortunately able to make use of sundry "affinity groups" and their trans-Atlantic cheap flights. There were some interesting experiences such as when we joined a rugby club -- a game no one in our family understood.

But to return to the official aspects of my visit and membership of the President's Commission. Travel within the United States and Canada was part of the sabbatical package and the work of the Commission involved sundry states. My visits to California and Alabama were memorable. Taking the last first. There had been racial problems in Birmingham Alabama, and somebody had the idea that a foreigner might be acceptable to both sides of the conflict. I was briefed (very briefly) and off I went. I thought that I could always ask questions of whoever met me.

For starters, it seemed (to me!) that a resolution would be more likely if we could get the action out of the hands of politicians. So I thought that the Chamber of Commerce might be a useful first contact. There was at that time a boycott by the black community (and some sympathetic whites had joined in too), and this was surely hurting profits. I doubt that my visit had much to do with the result, but in the event the situation was defused and the strike and boycott of black consumers was resolved. However, I remember being met at the airport with an off-putting greeting -- "thank goodness they have sent an Englishman; you'll understand our problems with these Negroes!". I almost began to see myself as a diplomat!. But others did not, it seems, share my view. I was not sent on any other missions.

I was able to visit the University of California at Berkeley where Joe Lohman, a member of the Commission was Dean of the School of Criminology. The family travelled cross-country with me in an old Ford Wagon which had belonged to a house decorator and still showed signs of this. However, it was so inefficient that it was fantastically reliable; there was nothing to go wrong! We took several weeks going overland; mainly camping in National Parks, but with the occasional motel. On the final leg of the journey we camped in Yosemite State Park. We had a vague idea that there were bears. But our eldest decided that it was too hot to sleep in the tent or car and curled up on the ground. We were somewhat disturbed to observe a bear sniffing around him while he continued to sleep soundly. We knew that bears were vegetarian and Arnold was not in a 'vegetative state'; just sleeping, so it was safest for us to do absolutely nothing, which is precisely what the bear also did. Arnold still takes a rather poor view of the incident of which, of course, being asleep, he has no direct knowledge!

California -- second time.

During this visit I was to become officially a member of the staff of the School of Criminology at Berkeley for purposes of teaching in the short summer semester. We managed to secure rental of a furnished house on Le Roy Avenue on the northern boundary of the campus and within easy walking distance of Haviland Hall. I gave sundry lectures including one seminar which was attended by Professor Blumer, distinguished Head of the Department of Sociology, Dean Lohman of Criminology, Calib Foote of the Law School and others. It was a great meeting. I did not realise at the time that this was more than a mere guest seminar to enable me to visit old friends from my Sacramento days. It was also much more than just a fun trip arranged by Lohman who had come to know me as a co-member of certain committees within the Commission. It was, in fact, part of a 'head-hunting' strategy. I was, obviously, after my performance, marked down as a potential member of the faculty should occasion arise. Later, it did. Butler would shortly cease to be Home Secretary and my Home Office period would terminate 'in smoke'. But before I come to that there are a few more features of my Home Office work which might be of interest.

During our stay on Le Roy, we had many pleasant meetings with Bob Smith (of Probation Subsidy fame) who had previously been attached to the Home Office

Research Unit. Some days we would mysteriously find boxes of strawberries on the front door step: Bob had dropped these off after a trip to the Central Valley.

Back to the Home Office.

On my return from the U.S I tried to accommodate to the old perspectives I had left behind, but this endeavour was not recognised. I presented a report on my experiences on this my second visit as I had the first. (I did not mention the White House incident). I noted my visits to Synanon and other drug related projects. I do not think that this report was any more appreciated than my first. The UK -- almost by definition -- could not learn anything from the U.S! As for California, everybody knew that the U.S. had a slight tilt to the West and everything loose rolled there! The Home Office did not relish my comment on the research funding policy and publication policy of the National Institute of Mental Health and the National Science Foundation.

Criminological Society Founded

I did not realise how much the year in the U.S. had changed my views. The situation in the unit was much as I had left it. In addition to actual research there were concerns with the administration of criminological research and training. The official policy was to encourage development of social work methods. I expressed my views which were not universally popular.

It was around this time that my prior association with Mannheim led to my becoming involved in the setting up of the British Criminological Society. The movement to set up such a society derived from the Institute for the Study and Treatment of Delinquency (ISTD) which was related to the Portman Clinic, a psychiatric treatment unit which worked with delinquents. Mannheim became involved with two psychiatrists who were advocating such a society. I acquiesced in this largely because of a measure of loyalty to the department and respect for Mannheim. The first meetings were held in the Portman Clinic building. There was little participation or interest shown in this new body by academic law departments. The 'treatment' philosophy and casework methods were mainly of interest to professionals, particularly probation officers who were the main participants. I was not enthusiastic.

Of the early meetings I recall clearly the reception which my presentation of the Borstal study received. I doubt that a single person present had a clue! Estimation and management of risk was seen as quite immoral if it was seen at all. The view which received most support was that my approach did not help in 'understanding' the delinquent. I was, I think, somewhat sarcastic about the concept of 'understanding' as a criterion of scientific quality. Whose understanding was to provide the yardstick? How could the subjective satisfaction of the scientist be regarded as a measure of the quality of the work? The essence of the debate is published in a special number of the British Journal of Criminology.

I recall also an occasion when a paper was being presented on something to do with homosexuality. This attracted the attention of a pompous Assistant Secretary. His branch had some responsibility for Approved Schools and hence of delinquency. He had attended meetings of the new society with fair regularity, often contributing to the discussion. He was a classicist, and everyone had to know that! At any departmental meeting when he was in the chair he would introduce the topic with some quotation in Latin or Greek, and then, in a patronising manner, 'spy scientists' and translate for the benefit of the ignorant.

Most of the members of the Crim Soc did not appreciate either his style or his particular brand of learning. He would all too often join in the discussion and again quote from some ancient text. However, it just so happened that I knew a magistrate whose full-time job was as a master in a well-known public school. He also was a classicist and an authority on textual criticism. The talk on homosexuality (it must have been around the time of the Woffenden Report) gave me a good opportunity to invite my magistrate friend. True to form our H.O. classical scholar decided to hold forth, quoting his Greek. It seemed, however, that the text from which he quoted was a totally discredited authority on the subject: I do not know why, or how -- this was what my magistrate whispered to me. I persuaded him to point this out to the meeting. Which he did. My friends all saw to it that he was rewarded at the local hostelry after the meeting. We had few lessons in Greek thereafter -- indeed our pompous member seemed to think that his future presence at meetings was unnecessary!.

The Crim Soc, had broad Home Office support. As I noted, it was based in London and had its roots in the Portman Clinic and The Institute for the Study and Treatment of Delinquency which gave a psychoanalytic bent modified somewhat by the link with LSE (in the person of Mannheim). The Society's links with academic criminology, jurisprudence or law were very limited. Grûnhût was by this time ailing and he had considerable problems with his vision. He was located at Oxford and did not, so far as I know, have any influence on the Society's development. Radzinowicz kept at a distance -- at Cambridge. If he had any influence it was through the medium of officialdom and the Home Office. He seemed to wish to avoid any co-operation with Mannheim. Perhaps a kind of respect for 'turf'? I do not think that he had any objection in principle to the formation of such a society. Lawyer refugees seemed either to avoid or to be somewhat antagonistic towards each other. The animosity expressed by Lopez Rey (who was associated with the Cambridge Institute) for Mannheim and even made explicit in his publications was beyond my comprehension.

As to the value of a society concerned with "criminology" I must admit to having some doubts. As I intimated previously, my doubts derived from my view that criminology is not itself a well-defined field of study; it was study of behaviours of which society did not approve. In my view research into topics regarded as relating to 'criminology' should be undertaken by qualified persons in terms of some parent discipline which defined a specific field of expertise. If, say, they were psychologists, then their field might be, say, cognition. I could accept that there were fields of interest but I thought that methods of study were more important. Team research had obvious merits.

Where a method of study of a form of behaviour was defined, it was possible that some of the behaviours could be disapproved and possibly legislatively defined as crimes. I was a statistician who, from time to time, might study data relating to judicial decisions; but that would be deploying decision theory, not 'criminology'. It was a long time before I was prepared to accept the label of 'criminologist' and I remain reluctant to wear this badge.

A Home Secretary tries to 'do good'

I discussed earlier my study of attempted suicide and its removal from the statute book. My view on attempted suicide derived from a general principle. It was my firm belief that the machinery of government should be concerned with managerial, not 'moral' matters and any social problem should be considered in this light. Criminal justice did not seem to offer any appropriate procedures for relieving in any way the problem of suicide. The same logic, I thought, should apply to persons who sought relief in the taking of mind-altering drugs -- which many saw as slow suicide. Up until early in 1964 this was more or less the accepted official view. People who did not like their minds as they were and tried to alter them were engaging in a form of self-medication which might be dangerous. But was it more dangerous than other forms of self-medication or folk remedies? What justification could there be for defining the former as criminal? Surely if punishing the former was assumed to reduce the problem, then the latter should similarly be punished? Thus, presumably, for example, Christian Science remedies would be illegal and herbalists would have a difficult time with the law.

Unfortunately Butler (under whose Secretaryship research had prospered) had moved on and we had a Minister who wanted to do good. I think he was genuine in this respect. To this end he was favourable to the increase in police powers, and seemed particularly disturbed by the 'youth culture'. My position became less congenial. I certainly had little sympathy with the new Minister. He was Henry Brooke, the (Conservative) member for Hampstead. He was a Methodist lay preacher and doubtless a pleasant person. Many in the Prison Service who knew him liked him and approved of his policies. However, as I saw things, his intent to do good was not tempered by concern for civil liberties nor did he see any need of scientific evidence. One of his early attempts to do good was to propose strong action against mind altering substances.

"Purple Hearts". Moral Panic ... or Something Else?

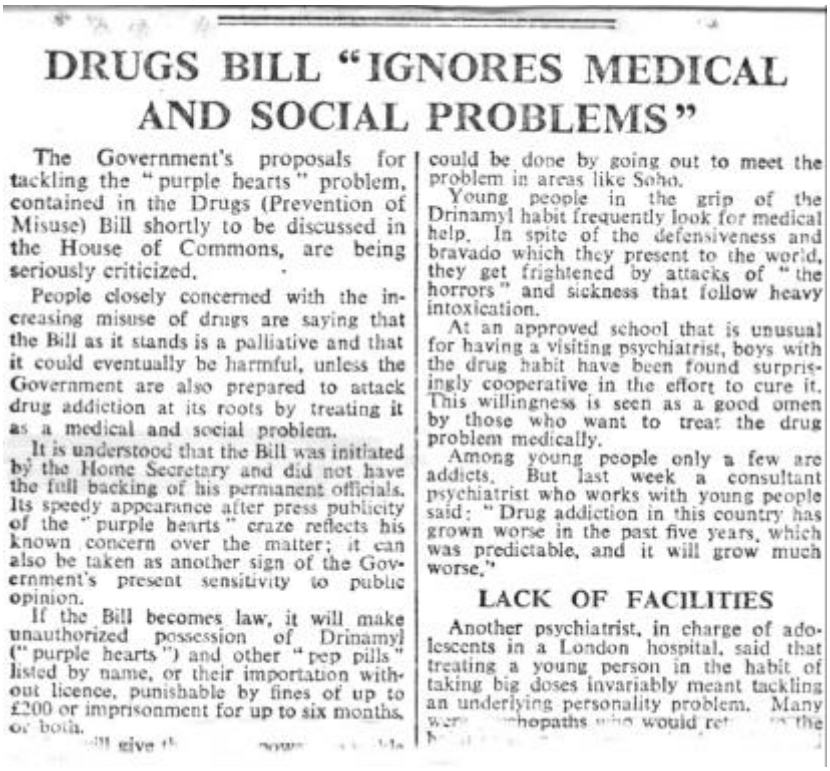
While there was some use of 'hard drugs', such as cocaine and heroine mainly by the medical and related professionals, the use of amphetamines was beginning to become apparent, particularly in the Soho community. The colour and shape coding of pharmaceutical products had, by some major mischance (I hope!) resulted in the design of these substances as heart shaped and coloured purple. What better image could any advertising agency have thought up to make these attractive (for non-medical reasons!)? Press panic on the use of these pills seems to have been the reason for the Home Secretary deciding to ‘do something’. No matter how intractable a problem, Home Secretaries can always do something about it, they can declare it to be illegal. This is

what was proposed together with added powers of search for the police. As I have told, I had informants close to this Soho set. From these contacts I learned that the price prior to the reports of the proposed legislation was 8 to the pound Sterling. The day the announcement was made the price increased 500%! It was, of course, to go even higher. The peddlers of purple hearts were then not only selling the pills, but profiting by the risk created by the law.

Civil liberties were threatened because Henry Brooke's Bill gave the police considerably extended powers of search. As though that were not enough, persons who wished to escape from the drug habit (or addiction) would be less likely to resort to medical help. While I saved a copy of my note --- a piece of about 5-600 words of objections to the proposed new legislation -- it has faded such that it is not possible to copy here. I quote a phrase which indicates that it was a piece of advice which it is most unfortunate was disregarded.

> " The `purple hearts' problem is only the first of many problems of control of sensory expansion substances which will become available in future."

For the rest, the Times story follows very closely the remainder of my comment. In relation to what happened later it is important to note the third paragraph which states that the Bill "...did not have the full backing of his permanent officials." This was true, if



DRUGS BILL "IGNORES MEDICAL AND SOCIAL PROBLEMS"

The Government's proposals for tackling the "purple hearts" problem, contained in the Drugs (Prevention of Misuse) Bill shortly to be discussed in the House of Commons, are being seriously criticized.

People closely concerned with the increasing misuse of drugs are saying that the Bill as it stands is a palliative and that it could eventually be harmful, unless the Government are also prepared to attack drug addiction at its roots by treating it as a medical and social problem.

It is understood that the Bill was initiated by the Home Secretary and did not have the full backing of his permanent officials. Its speedy appearance after press publicity of the "purple hearts" craze reflects his known concern over the matter; it can also be taken as another sign of the Government's present sensitivity to public opinion.

If the Bill becomes law, it will make unauthorized possession of Drinamyl ("purple hearts") and other "pep pills" listed by name, or their importation without licence, punishable by fines of up to £200 or imprisonment for up to six months, or both.

... could be done by going out to meet the problem in areas like Soho.

Young people in the grip of the Drinamyl habit frequently look for medical help. In spite of the defensiveness and bravado which they present to the world, they get frightened by attacks of "the horrors" and sickness that follow heavy intoxication.

At an approved school that is unusual for having a visiting psychiatrist, boys with the drug habit have been found surprisingly cooperative in the effort to cure it. This willingness is seen as a good omen by those who want to treat the drug problem medically.

Among young people only a few are addicts. But last week a consultant psychiatrist who works with young people said: "Drug addiction in this country has grown worse in the past five years, which was predictable, and it will grow much worse."

LACK OF FACILITIES

Another psychiatrist, in charge of adolescents in a London hospital, said that treating a young person in the habit of taking big doses invariably meant tackling an underlying personality problem. Many ... hopaths ... would re... the ...

Figure 6. Cutting from the London Times, Monday April 20, 1964.The article includes the paragraph which was used to accuse me of liasing with the Times sub-editor: "It is understood that the Bill was initiated by the Home Secretary and did not have the full backing of his permanent officials".

an understatement of my position. Since I was the only one objecting, it appeared that I `had leaked'. But first, let the Times give its story. Figure 6 reproduces part of the political news page.

I confess that the Times position was strikingly similar, though perhaps slightly milder, than my faded minutes now seem to indicate. Perhaps it was unfortunate that the Times story included the words "...the Home Secretary did not have the full backing of his permanent officials". Of course, the writer intended by the inclusion of this line in the article to provide a clue to my friends that I was not merely saying "Yes! Minister". I was also glad that this preserved my integrity without my shouting about it myself!

For those unfamiliar with the powers and duties of civil servants of Principal rank and above, I should point out that I was not out-of-order in objecting to the Minister's proposals and in letting this be known in writing, provided these objections were noted on 'minute sheets' on files and passed upwards. At that time it was not held that one owed any duty to the government in power, but rather was accountable to “the public interest”. In some departments this orientation was further indicated in their titles, such as "Her Majesty's Inspectorate of ...". We served 'Her Majesty' not the 'front bench'. This situation was changed drastically by the decision of the courts in the case of Ponting when it was ruled that "the public interest" was to be defined as "the interest of the government". This case related to Ponting's objection to Thatcher's ordering the sinking of the Argentinian ship, the Belgrano during the Falklands War.[15]

In my case I was in breach of no regulations because I had not made public ("leaked") my disagreement with the policy. I was, however, accused of inspiring the criticism of the Minister and departmental policy on the strength of the second paragraph of the Times article. I remember the morning of the 20th April 1964. I arrived at the office slightly late and on my desk was a message that I was wanted 'in the front office'. No indication of the reason for this was given. However, as I started on my way I met our First Division representative and indicated that I was 'sent for'. She asked me if I had seen the Times, because that was probably the reason and I had better read the piece first. I pointed out that (only!) "top people" read the Times, whereas '"lively minds read the Guardian!” I looked at her copy. I was not prepared to apologise. I went downstairs. I had not contacted the Times.

The True Origin of the Times Editorial

I have not previously disclosed the background to the Times editorial and WHO advised them of the position. In fact it was a senior member of WHO (World Health Organisation), namely Charles Winnick. He had been on a visit to Geneva and called me for further details of the new drug legislation proposals. He told me that he was dismayed. He assumed that I was doing all I could to resist this development. (He knew my views from my visits to the U.S. and the Presidential Commissions evidence). He

15 Ponting C. (1985) *The Right to Know*, Sphere, London

could divert to London on his way back to New York if he could help in any way. I agreed that he might, because I thought I could arrange for him to see some people in 'C' Division. He came, having only one day available. A few of the technical staff talked with him, but "front office" refused to talk: pointing out that his visit was unofficial!

Charles and I were both disappointed at the results within the office. He came to see me before departing. He had a couple of hours before his flight. It so happened that I knew a sub-editor of the Times (Francois LeFete) and on calling him he expressed interest to talk with Charles. The next thing I knew was when the Times' article was drawn to my attention. (I have not told Charles the outcome of his assistance with our hopeless cause: there seemed to be no need to do so).

The Accusation and a change of job

Of course I was asked to give an account of my behaviour and to explain the Times article. It was obvious that I was suspected of 'leaking to the press' -- a serious breach of civil service rules. I had not (on this occasion) provided the press (any press) with my views, though they might have been inferred from my book, Social Deviance which had just been published.

The Permanent Under Secretary suggested that I should be posted to some other department -- I do not remember whether this was to follow an official reprimand or any other action. I opted to resign.

CHAPTER FIVE: THE SCOTTISH EXPERIENCE: (FIRST VENTURE INTO UNIVERSITY LIFE.)

I include the details of my first and very short term of experience of university life, not because it reveals any development of my research skills, but because it provides another insight into the conflicts between the research viewpoint (i.e. mine) and those concerned with 'good order and discipline'. The problem on this occasion was not so much a matter of 'style' as of 'territory'. The experience contributed much to my learning, and, eventually even perhaps an increased degree of tolerance of non-research perspectives.

There is a managerial technique termed "polishing the skids". It has been known to be used both in research departments and in all kinds of organisation where employees are subject to agreed contracts. Few readers may experience it, and it is to be hoped that fewer will find it necessary to put it into effect. It is a way of getting rid of unwanted staff when one has no authority to actually effect their discharge nor to directly ask them to resign. My experiences at the receiving end of the techniques took place as I sought a post following my difficulties in the Home Office. I made a detailed record of the critical events which I think add up to a colourful example of both the skids method and the different styles of thought which may characterise academic and administrative roles within university governance. The story may also provide an example of my incorrect inference from observations with which I was too closely involved. In other words, another warning about the unreliability of introspection.

Escape from the Home Office depended upon my securing "approved employment". Enlightened readers may be unfamiliar with this feudal concept, so, I will give a short explanation. As an "established civil servant" (statistician class) I had both tenure and a non-contributary pension. Tenure provided considerable security though it was possible to be discharged 'for cause'. The pension was a valuable feature and though technically we did not contribute, the value of this 'perk' was clearly taken into account in salary negotiations. The value was such that an individual who had been in the employment from the age of 20 until 60 received 50% of the final salary. But more importantly the sums payable were sheltered from inflation. However, the technical non-contributory nature meant that whether a pension was paid on retirement was a matter of 'discretion' and not a matter of a 'right' of the retiree. In particular, the clause which was of concern to me was that if I were to resign to take up other employment without that employment being 'approved', my pension would be forfeited. Approved employment was limited to non-political appointments in the United Nations and British universities.

I applied to the United Nations but the quota system meant that my chances under normal conditions of obtaining a post were practically zero. So I set about getting a position in a university. It was apparently a good time to look around because two or three 'senior institutes of technology and research' ("Sisters)" were being set up. A post as Reader in social research methods was being established at the new University of Strathclyde in Glasgow. The job specification fitted my qualifications very closely and I

applied, was appointed, and duly resigned my Home Office post and my civil service grade. The 'approved employment' clause would safeguard my pension. I did not realise how much power this was placing in the hands of my new employer. The rest of the Scottish experience is best forgotten, but in the cause of honest reporting I will briefly repeat here the notes I made at the time in my appointments book -- I did not keep diaries.

Sequence of events at Strathclyde -- from contemporary record

> May 1963 --- Interview successful. Appointed Reader in Social Science Research with links with Sociology (Prof. Jahoda) and Politics (Prof. Potter).

It may be noted that I had requested details of both my duties and facilities. It was hoped that I would spend most of my time on research with assistance of graduate (postgraduate) students. I asked about housing my library. I was shown an office which was then a store but was a very reasonable size to accommodate my books and equipment. I was told that it would be prepared for me and ready when I took up post in mid June. In the interim I had secured a house in the new town of Cumbernauld which would not only provide my residence but be a kind of social laboratory as the town was being developed around me.

The local development agency knew of my interest and we had discussed the plans and problems.

The next detailed entry records the beginning of the end of the Scottish experience!

> 19th June Office not set up, though construction work seems to be completed. cannot find anybody in authority. Hang around university all day making myself available to students
>
> 20th June Decide to spend time at university and fixing House in Cumbernauld. Talk with Jahoda
>
> 21st June Deposited crates of books and requested that they be accommodated until an office was ready for my occupation. Promised that this would be completed when I returned from the U.S. on the 13th July. In the mean time what I had been told was to be my office continued to be used as a store. I had no desk, and no chair! Had to accommodate myself in the Common Room.
>
> 13-17 July Returned from U.S. on 13th July. Situation as I had left it. I had now built up considerable arrears of work but there were no facilities at the university. I was promised (17th) that the necessary work would be done and an office ready for me when I returned from a Meeting of the Home Office Working Party.
>
> 22nd July Returned from Home Office. No change. Exactly as I had left it! I go to see the chief of "Works" myself. He promises work will be carried out by "next Monday".17th Aug.: Still no change. No location in which to work, except that

there is now provided a desk in a corner of the General Common Room. No telephone or any other equipment. No point for my electric typewriter. Visit "Works" again. Hang around for 2 days and then get fed up and return home. At least I can type there!

7th Sept: Return to Strathclyde. Still as I had left it. Visit Works office again and get promise of work by Monday 14th.

14th Sept: No trace of workman. I note that this seems to be more than just inefficiency. Call on colleagues and ask for meeting to discuss my future contributions and conditions. Meeting fixed for 3.30. The result of meeting seemed promising. A location would, I was assured, be provided. It was not what I had been promised or of the size &c that I had been led to expect. Still, a place to sit and write. Begin to set up papers, and unpack library.

15th Sept: To office. Overnight rain had flooded the room and my papers were badly damaged including some books and valuable RSS journals. Was this why I was allocated this office?. 10.30 am. The rain entry is now so bad that there seems danger of electrical faults or even injury. Report this.

16th Sept. To University and find that a Lecturer (lower grade than Reader) was in the office which I had originally been shown and promised. It was well set up. Noted that the occupant was Scottish!

17th Sept. Builders have now taken down part of the ceiling of my 'location'. My books are back in the cases -- more or less. I had now been appointed for three months (and paid salary) but provided with no means of carrying out any work.

The significance of a lecturer being given priority for office space seemed to be saying something loud and reasonably clear! These actions decoded as a good example of a technique known as 'polishing the skids'. It seemed that I was expected to resign. I did so. I should have realised that my problems were arranged by the authority with charge of works and buildings, and were not supported by the academic side. The academics were not well organised to sort out the goods and chattels people! I suppose I could have just 'sat tight' and found ways of relaxing! I later came to appreciate that there were pockets of intense Scottish nationalism. I had, perhaps run into one! I ought to have been more prepared for this. I knew from my experience with the Social Survey of the delicacy of negotiations to bring Scotland into any 'British' survey for studies such as the prevalence of deafness. I had had to visit the Scottish Office in Edinburgh and personally explain the reasons for the desirability of a full countrywide sample and obtain separate permission to proceed. It seems odd to have to note my problems with Scotland now because I have four grandchildren who are all born and bred Scottish! All is forgiven!

Not even my resignation resulted in the satisfaction of the chief of Administration of Strathclyde. He wrote to the Establishments Division of the Home Office rejecting the

terms of 'approved employment' because I had resigned. Looked at from my viewpoint this was a nice attempt to save a few thousand pounds a year from my 60th birthday onwards! But he was protecting his funds.

Fortunately news of my availability for work was widely known. I was invited to join Stafford Beer in his management consulting company, "Sigma". Stafford had been one of my associates in Air Ministry days. There were problems in that this was not 'approved employment'. But I think a more serious consideration was that I found the life of a business consultant did not fit in with either my own or Barbara's style. The business car (a Mercedes) was OK! Travel, entertaining and selling research to industrial big shots was not my thing. I was glad of the money! I worked for Sigma for three or four weeks, during which time I wrote a monograph which was published on behalf of the company explaining what human resource research could do. It was a presentable publication.

We were pleased when a telephone call was received out-of-the-blue; well it was actually from Geneva. Ed Galway of the United Nations Technical Assistance Bureau thought I would be just the right person to replace Norval Morris in the Asia and Far East Institute in Tokyo. It was a special assignment with the Technical Assistance Bureau of the UN. I was not clear at the time as to whether this type of post was 'approved', but it did not seem to be of any concern.

In the outcome it seems that my U.N. appointment was just in time (by reason of entitlement to leave) to save my pension by giving 'continuity' of 'approved employment'. However, I was careful not to inquire as to my standing in this respect because I did not want to draw attention to any possible problems. It was with some measure of surprise that I learned, just before my 60th birthday that I was to receive my full pension with continuing provision for Barbara.

Strathclyde -- A charitable explanation?

After my academic experiences in other universities I can now imagine a probable and more reasonable explanation of what happened to me at Strathclyde. My thoughts about Scottish nationalism may have been quite incorrect or certainly exaggerated. Normally one could place much blame on pure and simple muddle, but the shambles at the University of Strathclyde could not really be put down to this most common cause. The incidents were certainly carefully planned to get me out. But I do not now think that the "state of war" in which I had become involved had anything to do with nationalism nor was it of my causing, rather it was territorial defence. After Berkeley I came to realise that continuing bitter conflicts often exist in universities between accountants and administrators and the academics. The Chief Administrator had not been present at my interview and was probably not consulted about my appointment. Strathclyde was a new university and he needed to establish his power base. It was too bad that I was in the way of his objectives. I was told that he was shortly deposed. But I was in Japan by then.

The U.N. appointment provided me with a 'knight's move' into employment which was clearly not approved --- at Berkeley! But it was much more than that.

CHAPTER SIX: THE JAPANESE -- UNITED NATIONS EXPERIENCE

In addition to carrying out research, academic life includes teaching and assisting others in research work. This may involve overseas assignments and some policy research workers may find themselves in similar positions to that of my experiences in Japan. I had not planned anything like the United Nations mission into which I was now about to be precipitated. Indeed I doubt that I would have thought myself able to carry out any such mission. However, others thought that I would adapt to the situation and contribute to the development of judicial policy in the countries of Asia.

Just before Christmas 1964 I left home for Tokyo via India and Thailand. 1964 had been a busy and eventful year.

Why was I in such a hurry to take up my post with the U.N.? Could I not have left it until after Christmas? Well no! I had resigned from Strathclyde after less than three months in the post. The offer of a position with the U.N. had to be taken up immediately or I would be deemed to have terminated my 'approved employment' and my pension would be lost. In any event, the Institute in Fuchu needed someone there right away, because the other non-Japanese staff were all departing before Christmas! So the new boy had to go and stand in forthwith. I was quickly fitted out with a United Nations Passport and various other documents, a number provided by the Japanese Embassy.

Indian stopover

I was allowed a few days break en route and Delhi was a possible stopover. I decided to opt for this location in preference to any of the others -- the route via Moscow was not available and the only alternatives were some of the airports in the Near East, or the extremely long flight via Copenhagen and over the pole. Of course, the Near East is nearer (somewhat) than is the Far East, and for some reason does not seem to include India, which is, I suppose neither 'near' or 'far'. When Stafford Beer learned that my travel plans included a free-time stopover in Delhi, he introduced me to an Indian member of his staff who had contact with Indira Ghandi. He had ideas for communications which he thought should be of value to the development of rural communities in India. He hoped that I could use my stopover to visit Mrs Ghandi, if this could be arranged. I liked Stafford's idea which would now be seen as an application of "Intermediate Technology". I thought I could explain the idea to Indian officials and agreed to see what I could do during the short break.

The flight to India was uneventful, until arrival. Despite my United Nations Passport, which entitled me to go to the head of the queue, the red-tape was applied slowly and with quite unnecessary thoroughness, and this included a complete count of all my cash and its careful recording on duplicate (or more) documents. This record was to be checked with my cash balance on departure. It was easy to assume that this was something to do with the financial controls and unofficial rates of exchange. How this

purpose could be achieved by the procedures used was not so obvious. It was more than two hours before I was cleared and on my way to meet the Sigma staff member who was to sort out my residential requirements as well as facilitate the discussion with Mrs Ghandi. She at that time was the Minister for Information.

The journey from the airport to the city was a devastating experience. It was freezing. The road was lined with beggars who were camped out in all kinds of ramshackle bits and pieces which provided scant shelter. It was the juxtaposition of the extremely rich and the extremely poor which really 'hit' me. Wealth was displayed with ostentation and arrogance.

However, I had already found it to be essential to adopt some of the "Raj tactics" myself in order to cope with a situation that could have been disastrous. As I was departing from the customs area, my baggage was seized by a 'porter' who went off at great speed. I was suspicious of the intent; it seemed like an attempted theft. As it proved it clearly was! My intuition took over and mustering my R.A.F. parade-ground style and voice, I barked out a command to put the bag down right now. It worked. I did not like playing these kind of officer/other ranks games.

I duly met my contact and was driven in an old Hillman to my hotel. It was OK. My contact arranged to show me around Delhi in the morning. Seeing Delhi was reasonably safe with my guide who protected me from beggars, bullocks and others, though a snake charmer was rather aggressive -- or his snake wanted a gift before it would relax. My guide was giving nothing away. Perhaps it was just as well. I was moved rather quickly out of range while the various altercations ran their course.

The idea to be presented to Mrs Ghandi concerned the dissemination of information through village groups and the use of television; a sort of Open University for a developing country. I had prepared notes and a short paper explaining. But I think my audience with the lady should be measured in seconds rather than minutes! Mrs Ghandi was just not interested. Furthermore she took the view that our suggestion was somewhat insulting to her and her country. I understood the position she represented to be that (a) India should not be assumed to be satisfied by lower grade technologies than those elsewhere in the west. (b) India did not wish to be told by any foreign technicians how to disseminate information to its own citizens. (c) India has its own technicians. The last point was doubtless true, but irrelevant; they were no more valued by the politicians than foreign ones.

I had no time to stay around and left the matter with the Sigma representative. My money was counted, some sums were done and I was eventually cleared for boarding a flight to Bangkok. My next assignment was a week's briefing by United Nations Technical Assistance H.Q. in that city.

Briefing in Bangkok

Bangkok is hot and sticky at any time of the year, even December. My hotel was probably the best available and had good air conditioning and all mod-cons. However

the view from the window was of people grubbing in the hotel's garbage containers. Poverty, wealth and religion were equally dominant features of the scene. Physically I think I had a relaxed week there. It was clear from my briefings that Tokyo was not going to be like I might have supposed. I had imagined it to be rather threatening because of its 'Eastern Mystery'. I was going to be pleasantly surprised. The contrasts between the old and the new and the apparent westernisation were stressed. I was pleased to learn that there was a very much respected protocol officer in Tokyo who would be able to advise me.

Here I had the opportunity of playing an unfamiliar role. Well! Rather reminiscent of my R.A.F. days, though I pulled this one off much better than the Oslo parade. I was to be an official inspector of prisons. Looking at the press pictures taken at the time, it seems that I did as well as most royalty!

Bangkok would not be a place in which I would choose to live, nor even to make a holiday. True, there is the splendour of the Temples, but squalor is juxtaposed to wealth. Souvenir silver spoons and a Tai Angel cloth are tokens of this stopover. I managed not to be sent to Bangkok again!

Arrival at Tokyo

So to Tokyo. Here I was met with considerable ceremony. I think there were at least two, perhaps even four cars of officials to welcome me. I had not been advised to count! The portents were, however, good. I was soon to learn that the welcoming procedure (and most if not all other such actions) were determined by the role the person was playing at the time, and not any personal attributes of the individual concerned. This was going to be a very important factor in my work for the next two years. The Japanese culture (and indeed language) does not allow one to generalise from one situation to another. When I arrived I was arriving as a "Senior Adviser" of the United Nations and carrying a diplomatic passport. The ceremony was appropriate to my role at the time; it was not to apply to other roles I would have to adopt later.

I was delivered at the United Nations' Asia and Far East Institute (UNAFEI) known locally in brief as "Azhaken" (as heard). My initial experiences of Fuchu were somewhat traumatic. On the U.N. Institute Campus there were three houses for foreign staff known as the "Adviser's House", the "Experts House" and the "Director's House". I was, of course, allocated the "Adviser's House". This was the bureaucratic arrangement. I had a sort-of tied property which I could rent at a peppercorn rental so long as I was a member of the U.N. staff located at the Institute. Staff were not entitled to free food or accommodation.

Alone in a Foreign Country

After two or three days the Institute closed for the vacation and the Management announced that I was on my own. I had a two storey, two bedroomed house, a domestic help, who spoke no English, but little or no furniture, no refrigerator and a

bottled gas driven 'central' heating system which was either super hot, or not functioning at all. It was months before I managed to get it tamed.

There were no supermarkets, so it was not possible to drift along shelves and identify foods. I had to cope with the small shops, jostling with Japanese housewives who shopped twice a day. This was reasonable behaviour because refrigerators were not in general use in Japanese households at that time. I had a grill and what passed as a small oven which was powered from the external gas cylinders connected via flexible tubes to an outside lean-to. This of course, was a provision for earthquakes. I had been warned that vegetables (being somewhat 'organic') were not safe if merely washed, so they were immersed in a solution of permanganate of potash before cooking. (I had taken a supply with me on advice before departure). I survived until the Institute mess reopened after the New Year.

I rapidly purchased a set of lounge furniture and various lighting and other fittings. The lounge armchairs could be totally dismantled by a few butterfly bolts and become a set of cubes. These subsequently went with me around the United States and as of the time of writing are still in use in our lounge at Cambridge, as is also a standard and table lamp (with etchings!). They were very good buys. Various items of decorative art were also acquired and are also still in evidence. I doubt that I would have collected these interesting items had I not been forced to do so by the living conditions. (It's an ill wind ...)

International Courses Begin

In the New Year the participants in the courses started to arrive. The Institute began to function as an educational establishment for adult students. Each participant had a study-bedroom (western style). There were both eastern and western style toilets. Above the western design were a set of cartoon-like pictograms illustrating the approved manner of use of this model! There was no similar instruction for the eastern style, and I never got around to asking which way it was correct to face during use!

Food was provided by a self-service system with a limited menu. Since the total complement was about 24 participants and a dozen or so staff, the meals lacked variety. Food provisioning was a difficult exercise in political correctness. The participants in the seminars came from all 'eastern' countries, extending from Fiji across to and including Iran. Religions tend to go by geographic area, and many of the religions had food taboos. When the mess menu was edited to delete any items which gave offence to any of the twenty or thirty participants, the diet was almost restricted to two forms of rice; sticky and not sticky. So I learned to cook for myself in the house.

Nearby the U.N. Institute was Fuchu Prison. A large establishment with a classification of high security. In any other country it would have been classified as an 'open' prison. A rail track ran through the centre and prisoners worked on production processes linked with the local factory. It seemed that all the castings needed for the 'free' factory were produced in the 'closed' prison and moved along the production line. Inmates kept my

garden area in good shape. The inmates and guards seemed to be on very good terms and worked together -- certainly this was no 'chain-gang' style of incarceration. It seemed far more humanitarian than the Californian State prisons. Prisoners knew the kind of behaviour which was proper to their role and guards knew their place too. The format was, it appeared to me, to be a natural derivative from the culture of the nation.

My duties at the Institute were not unduly demanding. I organised the courses, gave some lectures, wrote a number of articles, and was able to spend time in visits to Tokyo and the parks, temples and other cities. Each course also was treated to several tours and naturally I went along.

Impressions of Tokyo

My first impression of Tokyo was derived from the view I had from the diplomatic car windows. I was impressed by the rail link connecting the city to the airport (not the current one). It was a monorail. As we drive through the city it seemed 'westernised' and the neon lights and advertisements in the downtown area were more like Times Square than Piccadilly. However my briefing agent stressed that 'westernisation is a thin surface'. It was more a matter of appearance than of attitude. But this was 1964. The Olympic Games had just closed, and certain features had been added to most of the street furniture to make it possible for athletes from various countries to cope: railway stations had the name in Latin script as well as the two styles of Japanese hieroglyphics. But road direction signs were only in Kanji.

Out in Fuchu -- on (what was then) the extreme edge of Tokyo metropolitan area -- there was little western influence to be noted. It was most fortunate that the Keo Line railway station was only two blocks distant from the Institute. The station identification boards were marked (in small type at the foot) with the station name in Latin script. Fuchu had been the most distant point of the Olympic Games Marathon. Some concessions to foreigners had been made then and not deleted, though I think I was the only one to benefit! This was more than useful, it was for me absolutely the essential key to going anywhere. Otherwise I would have been restricted to movements within a range which I could walk and remember the way back, without asking for directions!. Later I observed that the trains, which were very frequent, ran to split second timing and that if one could get a timetable translated it was possible to get on the right train by consulting one's (electronic) watch.

Few, very few, Japanese people spoke any English and the railway journey to and from Tokyo required one or two changes of trains (also, indeed, of railway companies!). It was challenging.

I found a foreign food store in Tokyo and was able to purchase foods which I understood. Alcohol was, of course, provided as part of the diplomatic perks, tax free and at rock bottom prices. There was a restricted allowance but even with all the entertaining I did, I never reached anywhere near the ceiling. (Nor was I ever flat on the floor!)

Tokyo 'tube' trains.

Journeys on the train were interesting. The rolling stock and the track operations from Fuchu to Shinjuko were identical with the Metropolitan line from Croxley to Baker Street. There were parallel fast and slow (stopping) tracks. Shinjuko performed a similar function to that of Baker Street in that it provided a considerable number of connections to other parts of Tokyo, on other rail companies, including the central city (Ginza) and the (then) airport. Seating on trains was limited to a divided bench on each side with the large central area supplied with hanging straps for standing passengers, of which there were many. Some seats were reserved for the aged or disabled, and this allocation seemed always to be respected, no matter how crowded the train. I found it interesting to take the first coach of the train and observe the driver and the rails ahead. There was a good view through the driver's cab.

I am not tall, being about 5'8", but I could see over the heads of almost all of the other standing passengers. This would not now be the case; I have shrunk but by a lesser amount than the Japanese are taller. I would doubt that the present distribution of heights for the younger age groups differs between Japan and the Western world.

Meals and earthquakes

I often went to Tokyo to eat. How did I know what to order? It was simple. In the top class hotels, such as the Imperial, English was spoken. It seemed that English was an affectation of the upper class Japanese. Menus, rather than in French, were in English. The Imperial Hotel even laid on English tea with cucumber sandwiches and cakes! Some wealthy Japanese enjoyed the Western style and English was more popular than American.

The Imperial Hotel was the work of Frank Lloyd Wright and was the only major building in the city which survived the 1984 (R = 8.4) earthquake. It was built of pumice stone, very light. (I think it is a pity that it has now been torn down and replaced by a modern building). Below there were catacombs developed as very large linear shopping malls. These tunnels followed the rail tracks. One could walk to different parts of central Tokyo and connect with trains above ground. A similar set-up is to be found in Montreal.

Having mentioned earthquakes I must intercept the serious record with a story of how I was instructed to take precautions for such events. The advice was from a visiting expert from New Zealand. It was 'Robby' (Attorney General Robson), the same as I spoke of earlier in regard to the compensation for victims of crimes of violence. He reminded me that his country too was prone to serious earthquakes. He pointed out that "quake" was not necessarily the best description of what happened. The earth behaved rather like the sea with a wave form which ran away from the epicentre. He asked me which way around I had orientated my bed. Then he advised me to turn it around through 90 degrees so that when an earthquake happened I would not be rolled out -- the bed then being longways to the wave effect instead of broadside on. I rather

took the view that if an earthquake were to occur I would want to get out of the house, and getting out of bed might be the first step, so some assistance from nature might not come amiss. I (almost) fell for that one! I can assure readers that the Imperial Hotel did not survive because it was facing the right way round!

More on Roles.

In the shopping arcades were many eating houses. It was easy to decide here also what to order, not from any menu, but because there were on display plastic replicas of the items available -- very realistic. It was necessary only to remember the name or to point to the item. Smoked eel (pronounced 'oonahghi') was one of my favourite items in the smaller cafes, and with Sapporo beer this made a good meal. I also patronised the Imperial Hotel quite often.

I soon learned that even for a geigen (foreigner) diplomat, what determined expected (or actual!) behaviour was the role at the time rather than any permanent characteristics of the persons concerned. For so long as I was in my official capacity, I rated a driver and car. Escorts into and out of the country were provided and three cars were needed for the 'waving party'. But as soon as 'off-duty' status applied, all associated facilities ceased. If I behaved as a tourist, then I was to be treated as a tourist. If I was acting as a diplomat, technical adviser or instructor, then it was that status at the time and for the duration of the role only which determined what service I could expect. Any attempt to extend facilities to other activities was seen as a form of dishonesty.

The Japanese language encodes this separation of the individual into roles and the associated status. The language does not have a first person singular pronoun -- no general "I". The speaker uses a pronoun appropriate to the role or social standing at the time. The role or status of the person addressed must also be considered, particularly in greeting behaviour. Age demands respect and this has to be accommodated in the means of address. Women speak a different 'language' from men. I never understood the rules for bowing, and I was told that young Japanese have great difficulty with this too. It was understood that Westerners did not show respect in this way, so the best thing to do was to shake hands or greet in as natural manner as possible.

It is the lack of a common "I" which enables what appears to western perspectives to facilitate denial as a means of avoiding embarrassment. I recall requesting the Senior Administrative of the Institute to change the opening of the lecture room doors for fire safety reasons. He agreed readily. Two weeks later, nothing had been done. I had made the mistake of asking him to get this work done while he was a guest in my house. I had not asked him in his role as Senior Administrative Officer. When I went to see him and 'reminded' him of his promise to get the work done, he drew his breath noisily through his teeth and disclaimed all knowledge of the matter. I pointed out that I had asked about this while he was in my house at a small party. His reply was, "I was not in your house". The form of "I" he used was the "I" which applied to his work role. The idea of

self as an integrated individual is a western view of personality. The drawing of breath is saying "I am embarrassed and don't know what to do". It was very useful to learn this!

Defining my own role.

Perhaps I was impressed by the significance of role, status and personal image? For some reason it seemed important that I should be regarded by other 'foreigners' (particularly other British) not according to nationality but by my function. I was an international civil servant and was travelling on a U.N. passport. So that when I got around to signing in at the British Embassy (which I was required to do) I requested that my nationality be not recognised unless in emergency. I did not wish to be seen as just another individual from Britain: nationality was irrelevant. .

The United Nations had their own protocol officer in Tokyo. He was bilingual and a most delightful person, extremely well informed on Japanese culture and history which he also genuinely appreciated. He was willing to take me around the area and introduce me to the shrines and explain the ceremonial. He also had a high quality tape recorder and a supply of tapes of 'classical' music. He recommended that I should invest in an Akai reel-to-reel and some good speakers. He would then copy tapes which he might have and I might like too. I was able to build up quite a large collection not only from his copies but by recording concerts broadcast by NHK in stereo.

Distinguished Visitors and Sundry experiences.

Reflecting on Fuchu gives rise to all sorts of memories, but few structured patterns. The turnover of participants in courses emphasised the discontinuity of experiences. One has now a rather hazy picture of the work, the participants and one's associates and the difficulties of finding something good to eat. Many experiences were quite surprising at the time though most have now dimmed into the background of fuzziness. I was surprised to find that the Tokyo radio broadcast, in stereo, most afternoons first class music (taped and recorded) played by the world's best orchestras and some live broadcasts of local programmes. I suppose much of this was with an eye to the export market. For whatever reason they were most enjoyable. Western music, it seems, was becoming accepted, and among the intellectual classes, much appreciated. It was possible for me to set up the tape recorder to record the afternoon concerts and to listen to them in the evenings. This was before stereo broadcasting was begun in England.

Soon after my arrival a young Dutch lawyer (Antoine Peters) was appointed. With the agreement of the authorities, I offered him accommodation in part of the house. He was glad to accept and we shared the house until he married one of the Japanese interpreters. He did not have the status to enjoy the services of the official car, so he purchased his own. The Adviser's house had a garage. It was a good arrangement because we could then pick up the duty free drinks from the Embassy stores (and of course purchase food in the foreign food stores in Tokyo). We were able to entertain staff or seminar participants, as well as 'externals' such as professors from the universities. We made many good friends.

Among the guests we enjoyed the company of Professor Nikiforov of the Moscow Academy. He spoke English with a Scottish accent -- he had had a Scottish nanny! I was impressed by one feature of Soviet law which Nickiforov described. In a case where an individual from another part of the U.S.S.R. is arrested for a crime in, say, Moscow, which is not an offence in the home area and residence in Moscow short, a claim to "in bar of trial" could be made on grounds of his being unfamiliar with the local law. I recall that Saint Paul was said to have claimed the incompetence of the Jewish courts to try a Roman and requesting his trial to be in Rome. Paul's claim did not challenge the idea that ignorance of the law is not an excuse, whereas the Soviet concept certainly does involve the element of "knowing". How this relates to our legal idea of "mens rea" and "intent" is interesting. I will leave the thought there or this will become a different kind of book from that which I "intended"!

Not that Nikiforov's visit was characterised by a one-way traffic in ideas. Many years later I learned of the impact these meetings had had on criminological theory in the (then) Soviet Union. Some 20 years later at a U.N seminar a presentation by the Soviet delegation reproduced my 'deviation amplification' theory. I asked the speaker how he had come across this. He had been a student of Nikiforov! By this time the transformation of the U.S.S.R. was well under way. Perhaps I should have been less surprised than some that this took place. Nikiforov had views which were "subversive" in the extreme prior to Gorbachov's accession to power. He did not, of course, present his views to the seminars, but in private discussions in my house he felt free to give his uninhibited views. We had discussed my Social Deviance book and clearly he 'took it on board'. The Soviet physical scientists were also well represented at the U.N. Earthquake Research Centre in Japan and worked well with many other nationals without regard to political conflicts.

The story of my times in Fuchu could occupy several volumes (well, it could if I could re member them) but I do not propose to attempt a detailed account. I will say only that my time there, despite sundry hardships and in particular suffering the separation from family, was a very positive experience.

Touring (a different role!) earthquake aftermath

In addition to the work and its related activities I was able to see as much of the country as most tourists, including Hiroshima and Niigata. The former was included on some of the U.N. trips, and of course, there were always prisons which could be visited and these were located throughout the country. My visit to Niigata was different; very different.

I had purchased a movie camera and it had become a habit to send the films home as a record. I had been at the Institute for some months when the Canadian "expert", who spoke some Japanese suggested that we might visit Niigata on the west coast. This would take us through the Japan Alps, a very beautiful mountain range in central Honshu, and we would see how the city was recovering from a serious earthquake. We had heard of how buildings were being lifted upright by hydraulics.

The train journey was, I suppose, typical. No space was available for one additional passenger. People were being posted into the carriages through the open windows and luggage racks were accommodating whoever was small enough to fit. We decided that we could pay a bit more and get some additional air if we were to take a meal. I don't remember what we ate, and I do not recall any questions arising (nor anything else!) at the time. We arrived at Niigata, I think, on time, though having a Japanese speaker with me I was not so attentive to indications of time and space and the relationship between these two dimensions! It was dark and we took a taxi and were dropped off in the foyer of our hotel. It was a western style hotel -- and I suspect the best in the city.

We went to our rooms right away. After a while John called on the internal 'phone to ask whether I felt OK. He was, he said, feeling quite dizzy or disorientated. I also had noticed an unusual feeling, rather like having been spun around several times and then stopped. We noted that we had had a meal on the train, decided that we might 'sleep it off'. I decided to take a bath first. There was a full-sized bath and as I began to run the water, the cause of our malaise was made apparent. The water all flowed to one end! The whole hotel, quite intact, had tilted several degrees out of the perpendicular. All our visual orientation clues had been out of kilter! Shots of the view from my hotel room window are on the home movie and show the buildings opposite leaning against each other, and other buildings with several degrees of tilt. Continuing to work in these buildings resulted in a medically recognised ailment termed Niigata sickness, related I would suppose to seasickness..

Some personal views on Japanese culture

Many books have been written on Japanese culture and traditions and I cannot claim to be an authority on that subject with only two years of residence, True, my role as an international civil servant gave me an unusual perspective, and living 'native' in a suburban area was an experience not gained by tourists or by visiting academics.

The early 1960s were times of rapid transition in Japan. People were unhappy with their past and wanted to 'learn democracy'. If I were to try to make sense of much more of my time in Fuchu and of my impressions of my experiences there, a large percentage would probably be inaccurate. While I will not interpret events I can say what happened in a few cases which may have an interest. Some seemed trivial at the time and less trivial later while others seemed to have considerable portent but later the apparent significance evaporated. I will draw no conclusions.

The Times are changing, rapidly.

In the mid-1960's Japan was trying to retain what was seen as 'good' in their older tradition. For example, the relationship between employer and employee involved the concept of obligation. Indeed 'obligation' was a dominant ethic. In practice this meant that once an employer had engaged a worker, that worker had an expectation of working for life with that organisation; there was mutual obligation. The worker owed a

loyalty to the employer in exchange for this security. Two examples of this may be of interest.

There were superstores as elegant as any in the West End or Fifth Avenue but features of tradition were retained. Even in these stores the ethic of obligation of employer was evident. Work was found which could be carried out satisfactorily by each employee. In the Ginza superstores the handrails of the escalators were wiped continuously by girls dressed in the elegant shop uniform which included white gloves. They stood at the top of each floor and, bowing to each customer, bid them welcome. I pointed out that this was an unnecessary activity. Handrails could be cleaned probably more efficiently and more cheaply, and the welcome could be supplemented with information or advertising. The reply was to the effect that the only thing the girls had going for them was that they were good-looking and it was the duty of the employer to provide work which engaged the employee's qualities, qualifications or even the lack thereof.

The loyalty to the employer which this system engendered was expressed covertly by professors who, when introducing themselves would not give their name until they had told you of their institution. This caused me some embarrassment when at an official function a distinguished and somewhat aged professor introduced himself to me by announcing that he was a member of the Tokyo Institute of Technology. He stressed this by asking me if I was familiar with the MIT, I agreed that the M.I.T. was a world renowned university. Proudly he pointed out that he was a TIT! The first and last time I have spoken with a 'big tit' who was proud of that fact!

Tolerance of ambiguity

The contrasts between east and west in the workplace was also evidenced in social life. Adoption of western patterns of public and private etiquette was not generalised. Home life styles varied from fully 'western' to traditional Japanese, irrespective of status (at least within the academic community). If there was a correlation between preferred styles and any easily observed variables, it was with age. The younger were more orientated towards western styles in cafes and places of public entertainment.

It became clear to me that the Japanese people are extremely tolerant of ambiguities, not only in respect of religions and whatever it is that religious affiliation implies, but in all aspects of living. According to my perspective there was tremendous contrast between those individuals or families who retained the old patterns and those who had adapted to the new. But this did not seem to be noted by the people themselves. There was also a strong correlation between western life styles and western religions, particularly in that those who claimed to be adherents to any of the various denominations within Christianity tended to also adopt the western patterns which went with that belief system. But religion seemed to be held lightly, no matter what faith was assumed. A senior law professor (the Deputy Director of the Institute) claimed to be a Shinto-Budhist-Christian. Often a family would opt for a marriage ceremony in the Shinto tradition while on other life occasions they might elect either Christian or Buddhist.

The Deputy Director was quite 'at home' in western academic environments but his wife walked behind him! On no occasion did he invite me to his home, but entertained in male society in cafes, served by Geisha girls. At Buddhist or Shinto shrines he performed the ritual. Many shrines combined in various ways both the Shinto and Buddhist symbols. (Possibly also the rituals, though I was never sure whether the local Buddhism had any specific 'ritual' of its own). Households did not seem to mix traditions. If the home was westernised, then chairs and dining tables were used, but if it was not, one sat cross-legged on the floor with a low table. The western bed was not as functional as the traditional futon. Space was very limited in Tokyo homes and there were seldom separate bedrooms. The futon was set down at night in a general purpose room. Chopsticks were more generally retained, and many of the staff and foreign guests at the Institute decided to use them. Among these I include myself. In fact, if eating out in other than the Imperial Hotel, or western style home there was no alternative.

Whether a home was or was not westernised was, of course, somewhat related to income and status in that the poor could not afford the western forms of furnishing and space use. But neither occupation nor status gave any firm indication of what style of living one should expect if one were to meet those involved on social occasions. Among the academic community a number referred humorously to themselves as the 'Michigan Mafia'. I knew well the two senior professors of law at Tokyo University. One had adopted a fully western style and was proud of the years he had spent at the University of Michigan. The other law professor whose behaviour in the office was precisely the same as the Michigan trained -- using all western equipment and dealing with his secretary in the same way -- nevertheless his home life was completely Japanese traditional home life. His wife served drinks and food to him and any guests from a kneeling posture in the study and was not introduced! But it was most unusual for him (as for any traditional Japanese men) to entertain at home. He probably did so on the occasion I remember because he knew that his colleague had invited me to the house and he felt 'obligated' to do similarly. As an aside I might add that some 20 years later when Barbara visited the Institute with me, the 'westernised' professor invited us to his home and we had a great time singing madrigals and English folk songs!

Poverty was often obvious, particularly in the countryside, and contrasted with displays of wealth, though by no means as flagrant as in India. Aged women, pulling barrows loaded with produce would be seen crossing road bridges at the same time as a Mercedes. Perhaps the preceding kind of word sketches indicate best something of what I learned in Japan and why I remain grateful for the opportunity I had to be there at such a critical time; before the disappearance of many of their traditions and special perspectives. So I will tell one or two more stories.

After you! A sidelight on obligation.

The Fuchu streets were extremely narrow. If everybody in the roadway moved into the shop doorways a car could pass along. If one came in each direction it required negotiation by one backing into another road or other inlet. Corners were blind. Cars

were, at that time, fairly rare. Cycles were used in their thousands. Among their use was the delivery of workers' lunches, which seemed to consist mainly of soups and raw fish. The meals were piled high on trays and these trays were held high in the manner of good class western hotel waiters while the bicycles were operated with the other hand. Soon after my arrival I was approaching a corner and could see two such delivery cyclists with their trays approaching at right angles and at similar speeds. It was clear to me that there was little chance that they could avoid a collision on the corner. I was right. They collided. Now imagine two London (or New York) taxi drivers in such a situation. But no abuse was expressed, no blame allocated; the two cyclists picked themselves up, bowed deeply to each other and began together to clear up the mess.

I was told that this behaviour was because had one been the first to accuse the other, this would indicate that he was to blame. So both apologised profusely. The situation called for reconciliation.

An apology would often be all that was necessary to resolve even what in western cultures would be deemed a crime. This was particularly so if the individual offended against was willing to forgive the offender. The emphasis was on negotiated settlement. The U.N. protocol officer briefed me on the significance of 'obligation' and there is little doubt that this powerful and wide-ranging tradition featured largely in determining the behaviour of the cyclists and in many other interpersonal situations. Apologies are very significant and have great weight, it seems, both in the giving and receiving.

Estimation of formality -- a gradient?

I had often tried to project the anticipated styles of behaviour I would encounter in forthcoming situations. A good rule-of-thumb seemed to work out well. I knew the level of formality which might attend a situation in the U.S.A. and similarly I knew what would apply in England. Normally if I projected the 'line' I could expect the situation in Japan to be somewhat 'further along the line'. This concept of a gradient in degree of formality worked well generally and was particularly useful in such cases as in the official welcoming of distinguished guest speakers at the Institute. So, when I was invited to attend a trial in a Tokyo court I was expecting the formula to work for the sort of ceremonial and formality I would find there.

The case I was to hear concerned a charge of attempted murder. The facts of the case suggested that in England the charge might have been G.B.H. (grievous bodily harm) which could have been reduced to A.B.H. (actual bodily harm) if the accused had a clever lawyer. A prosecutor in the U.S. would have thrown the book and the charge would have been the same. My expectation of formality was shattered. In fact the Tokyo court was less formal then those of the U.S.A. with the UK being the most formal -- the equation had not worked. The participants were seated at tables. The accused when giving testimony or being examined was not put into the witness box --- no box existed. Even more surprisingly, the prosecuting attorney questioned the accused on the floor of the chamber by a table. In this case he even handed the accused the knife exhibit and

asked him to demonstrate how he used it on the complainant. A rather risky approach, I thought!

The demonstration by the accused was initially somewhat reserved, but he was pressed into greater realism.

The prosecution began the proceedings by apologising to the judge for having to bring the case because he had failed in his primary duty to bring about a resolution acceptable to the victim. The victim was not prepared to accept a simple apology (not surprising to western opinion!).

The importance of 'obligation' and the power of apology are not understood in western society, nor is the fact that each of these concepts is related to the ideas of negotiation and reconciliation. In the end the accused was awarded two years prison, precisely the same as he might have expected in England and considerably less than in most U.S. states!

The underlying explanation for the cyclist/waiters behaviour on (or off) their bicycles illustrated the same point of the significance of the apology. Their behaviour was also typical of cycling etiquette. Schoolchildren cycling in the narrow streets were most courteous to pedestrians, often dismounting before passing if this seemed to assist the walker.

So road users are courteous? Not at all. The cycling courtesy of school children was not echoed in car driving behaviour, which tended to be aggressive, particularly in the city where roads were modernised. I discussed this anachronism with friends and the protocol officer. The view was that because there were no precedents for an etiquette of car driving, and the lack of any explicit rules, behaviour tended to be chaotic. Furthermore a driver is on his/her own (or is in a separate environment from) the pedestrian or cyclist. Thus there is no 'deferential' behaviour to provide the underpinning for this activity. But I will return to this matter a little later when we have looked into some other examples of interpersonal ploys.

Religion's influence

The impact of both Shinto and Buddhism upon Japanese behaviour is especially apparent in the procedures of the search for consensus in decision-making and in the emphasis upon negotiation. In Christianity there is an underlying philosophy of dichotomies: true v. false; right v. wrong; bad v. good; 'he who is not for me is against me'; and so on. Shintoism has much association with animism. Animism is, among other things, less dependent on tight categories. Hence, it seems, the division between 'right' and 'wrong' is often open to negotiation. Christianity would deny this, indeed the idea of negotiating 'truth' is itself to be deplored. I have few problems with negotiating a 'moral' issue. I have an example..

When I had been at the Institute for some eighteen months I had become well accepted in the Ministry of Justice. It transpired that the Chief of Police of Tokyo was attending

one of the special short courses and we had several discussions on police strategy. We got to talking about prohibition and gambling. Most gambling was illegal, but was, nonetheless, prevalent. Normally the police took the view that since the public wanted it and it was run without causing them any trouble, they could treat it with a benign neglect. However, as in other countries, at intervals somebody starts a moral panic and a demand builds up that the police 'do something' about it. Such a demand had reached the point when the police would have to 'do something'. What was to be done? It seemed obvious to the Chief that he should discuss this with the chief 'gangster' who operated the Tokyo area. He was in hospital at the time.

I was invited to go along to meet the gangster who was surrounded in hospital with his keepers. Agreement was apparently reached as to who was to be picked up, and there would be no trouble, because all who were concerned would be notified in advance that it was 'their turn' to do the penance. The police 'did something'. The fact that this was the result of negotiation was probably unknown to those who were involved in the moral panic. This strategy would not be acceptable where right v. wrong was seen in terms of a two-value logic.

When things go wrong.

This explanation seemed to be true, and was supported by another incident in which I was involved. A friend from my statistical days (a Dr. Duckworth) had achieved a senior managerial level in his company which had branches in Japan and a Headquarters in Tokyo. He was making a sort of 'state visit' to the Tokyo unit and was being treated by the Japanese managers as a distinguished guest. He contacted me to know whether I would care to take a day off to accompany him on one of his entertaining jaunts. I was able to do that, and agreed. We were chauffeur driven to some of the major attractions within reach of Tokyo, had a good lunch and were on our way to the next location when it began to rain. And when it rains, it can pour. It did.

Then something clearly had gone awry. It was obvious that our driver was not going to go any further. We were able to deduce that the next point of call on our schedule could not be reached. We found shelter while the driver went to see what could be done . Everything had gone like the proverbial clockwork up to this point when everything went to pieces. The driver obviously under extreme stress was behaving as though he was suffering from a catatonic fit (well, nearly). He tried to telephone H.Q. for instructions. He could not make contact. He therefore, left us sheltering under an inadequate awning and did absolutely nothing. My manager friend's temper was beginning to fray. Clearly our driver had lost his steering mechanism! Time was going by and nothing was going on and nothing seemed likely to. Eventually I realised that the problem was that our chauffeur was not authorised to amend the schedule or route and he could not get authority to do so. Remembering my briefing I pointed out to Duckworth that if anything was to happen he had to countermand the orders under which our driver was operating, because he was unable to carry them out and that his embarrassment at this situation was totally disabling for him.

We decided to order the driver to take us back to Tokyo. He complied, but was extremely uneasy about this, revealing all the signs of great mental stress. We were sorry for him, but we had to have action. The difficulties experienced by our driver in the absence of a framework of rules or a structure appropriate to his role, were exacerbated by the fact that he could not assume an authority he did not have because this would have been assuming a different role or 'personality'. This need for external direction would not apply to persons whose role included decision-making. Nonetheless I should note that most decisions within organisations tended to be made by discussion. This applies even with the decisions of very senior staff, and much time is spent in meetings sorting out a line of action which then binds those involved.

***Good manners or merely risk avoidance*?**

Initially some explanations of etiquette seemed contradictory as with the road users behaviour in case of accident. The rules and precedents were certainly complex. A few different examples may be briefly stated. The tendency to seek compromise solutions and to make decisions collectively seemed not to apply at the top level of government. Perhaps the discussions between staff in decision making were limited to issues which could be framed as issues of how to interpret existing instructions rather than independent actions. This would fit with the explanation given to me as to the behaviour of drivers while IN their cars. When, for any reason, drivers were not actually performing the role of driver, the usual rules of conduct could apply. In an accident the driver who lost his temper or who was the more aggressive was always assumed to be the one at fault. Hence drivers involved in accidents tended to behave well.

I was surprised at the almost open conflict between Ministries as to national policy. Two instances came to my attention and both might have been seen as 'territorial disputes'. The Ministry of Justice (which was the department responsible for the operations of the UNAFEI) was a powerful Ministry and apart from dealing with criminal law it had control over passports. In fact in 1964-6 the Japanese people did not have passports, but had to get clearance for every single journey out of the country from the Justice Ministry. The Ministry of Commerce and Trade were strong opponents of this procedure and wanted passports similar to those of most other democracies. My opinion was sought in another dispute between the Supreme Court and the Justice Ministry. Juvenile delinquency and criminal acts by young offenders were within the province of the Supreme Court as was the probation service. The Ministry of Justice wanted to take over juvenile delinquency if not also the probation service. So, it seemed that when there was no prior situation which might analogously indicate a clear line of conduct, the Japanese operated with the same confrontational style as western cultures.

During the time I was with the U.N. (1964-6) the police force was deployed in a manner which I consider to be superior to the British or American system. I do not know whether this system has been preserved with the changes which have taken place in the country since. There were local boxes (Koban) not much larger than a telephone box, distributed around the built-up areas. These were manned by two officers, one of whom would be present in the box while the other would walk around the area. Houses in

Tokyo (and probably other cities) were numbered in the order in which they are built within the local area (about the size of a parish) The Koban was, therefore an essential first call if one wished to find anyone for the first time. Another method for meeting the difficulty of identification of location was in the almost universal use of 'visiting cards' which would have a map printed on the reverse. A card (or something similar) is also essential if a name is to be 'recognised' because the pronunciation does not define the Kanji script which must be seen. Japanese do not 'sign' documents but have an individualised and registered stamp (carried in a small case with ink pad). This will be seen as the signature on Japanese paintings or prints. Police work was much more widely defined than in the UK or U.S.A. It was, for example, part of their duty to carry out census and various other tasks.

Philosophy of business

Soon after Japan had became 'open' to the rest of the world, it had become successful in business by copying the technology of the west and selling its products cheaper than home products in Europe and the Americas. The image of Japanese imports in these countries was of cheap (and nasty) copies of low quality. This was not completely inaccurate at the time, though it was a strategy which had little impact upon the cultural development of the country. The copies were provided solely for others, if they paid for them! For the Japanese themselves, while they took note of foreign technology and consumer goods, tended to adapt these for their own use rather than merely to copy.

After WW2 Japanese industry changed its orientation. It soon became concerned to design and produce goods of quality and reliability rather than in quantity and cheaply. Quality control methods were welcomed and utilised. New technology was embraced far more readily in Japan than in the United Kingdom. Still more recently (mid '90s) Japan made another industrial strategic policy decision. Its research expenditure will give priority to work which in Britain would be classified as 'blue sky research'. Development has taken a back seat to research.

I have continually to remind myself that my main tour of duty and residence at Fuchu was in the mid '60s -- more than 30 years ago. The rate of change in all departments of life was extremely fast when I was there and the present situation must be very different. I observe Japanese student visitors and they are no longer of shorter stature than western students. Whether time has resulted in a multidimensional westernisation' of Japan or a more fundamental and deep social, political as well as economic similarity I cannot say. Some of my experiences in that country may be only of historic interest.

Bribes, tips and presents.

There was one more feature of Japanese culture which had to be accommodated by any Westerners in social or business situations that could cause difficulties. I doubt that the traditions have vanished. The 'obligation' principle makes the giving of presents or rewards a difficult matter for foreigners if not also for natives. I had to 'hide' my domestic assistant's birthday and Christmas presents as one hides Easter eggs -- not too

difficult to find! It being accepted that if she 'found' the present it did not put her under obligation to reciprocate. A direct gift would have presented such an obligation. The value also has to be carefully considered. It must not be a bribe. And, I almost forgot -- Japanese taxi drivers do not expect a tip unless they do more than the generally expected level of service.

On being "Japanese"

While Japanese culture seemed generally, to me, to be tolerant of ambiguities (as demonstrated with respect to religions) and while moral instruction associated with Shinto made its points by means of examples of moral dilemmas, there was a dichotomy which was present in the undercurrent all the time. This was the distinction between 'being Japanese' or being a geigen. The term geigen has a lot of historical and probably emotional baggage and even experts find it difficult to define. It may be taken to mean, or to be used in much the same way as we might refer to an 'outsider', or as inhabitants of the Isle of Man might use the term "come-overers". But being Japanese has much more content than any similar terms in English would suggest. I experienced this one evening and the story will illustrate this point. I had missed the last Keo Line train to Fuchu which departed just before midnight from Shinjuko. It meant that I had to travel by the main suburban line which was served with rather less modern equipment. Bench seats were provided only along the outer sides of the carriage. No seat was available for me. I stood with a few others in the door sector (which was very similar to the London Underground trains). The seats were unavailable because three or four were all occupied, or should I say, were being used in so far as he found it possible, by a drunk trying to get some shut-eye. Trains in Japan, as elsewhere, often go around bends and each time, he was thrown on to the floor. Each time he tried to retrieve his 'bunk' and he certainly was very persistent in trying to retain a comfortable horizontal posture. I was watching this performance (could not avoid doing so). The behaviour of the celebrant clearly embarrassed some law students, who were disturbed that a 'geigen' who was obviously older than the drunk, was being prevented from having a seat by this behaviour. I noted earlier that age was given considerable respect in Japanese culture at this time.

Eventually one of their number who spoke some English was apparently assigned to come over and apologise to me for this situation. He said something about being sorry and remarked "He is drunk". I treated this comment as rather humorous because it was all too obvious to need pointing out, even to a geigen. But the student pressed his point since clearly I had not understood. "He is not Japanese -- he is drunk". The role of 'being drunk' was the dominant quality. It was inconsistent with the role of 'being Japanese'. Remembering my problems with the Senior Administrator at the Institute about the fire doors, I indicated that I understood the position.

Beauty and whatever

While I am discussing the different perception of the individual I might mention another related concept. It was the protocol officer who in my initiation talks pointed out to me

that the Japanese people have a great sense of beauty, but no sense of ugliness. Certainly they have great regard for art, and particularly art from early times. Most households have a special niche or alcove which will have freshly arranged flowers or the most significant piece of art that they can purchase. Even drinking bars will often have a similar special 'piece'. (I believe that this alcove/shrine has a specific name in Japanese). When visitors are received into the home, they are always seated with their backs to this exhibit. This symbolises that the visitor is of more importance than the most prized possession of the household.

Figure 7. Reception with then Crown Prince of Japan, later to be Emperor.

Royalty and Ale

I learned much from the 2 years of my U.N. assignment based on Fuchu. It was possible to meet some of the most distinguished authorities in jurisprudence who were among the official visitors. But there were other visitors and formalities to be performed by the U.N. representative. One distinguished visitor was the Crown Prince, or whatever is the appropriate title for the son of the Emperor. He had been educated at Cambridge as a biologist and, as I learned much later, as a patron of the Pike and Eel. He was pleased to speak English and discuss scientific issues in an informal atmosphere. Whatever guards there were were not obvious. The formalities which were required by his visit, and these were considerable, were all looked after by the Japanese. It was necessary, among other things, to rent for the day the most valuable Bonsai tree in the country. This was no mean charge on the Institute's funds. But costs of this kind are a natural consequence of a royal household. Such costs are never included in reckonings as to the cost of a monarchy. I am sure that while at Cambridge the Prince was not accorded such

deference, but he seemed to have enjoyed his time there, particularly at the Pike and Eel(?). (So do we!)

The role conferred by the system of monarchy overrides any personal qualities of the individual; and in this Japan is not unique in its subservience to the systemic factors of role and status.

Other duties

I would not wish to give the impression that my time in Japan was a total learning experience. I was the only English individual in the set-up. The other full-time United Nations staff were the chief of administration who was from Sri Lanka and a devout Anglican, and a young Dutch Lawyer (Antonie Peters). Tony became much involved with the Japanese interpreter whom he later married. I should note that I played the role of 'go-between' for the purposes of the Shinto wedding. I had to be a character witness for Tony, but also to give my views as to the desirability of the marriage and whether it was likely to last. We met again in my next location in U.C. Berkeley. Tony is a poor communicator, both in personal and professional terms -- he does not appear in the citation index! He has not contacted me in the last decade. The last I heard he was a professor of law in Holland and was still married to Michiko. So my prognosis was apparently justified.

Another staff member who was also around during the teaching sessions was provided by the Australian government but he lived (with his wife) somewhere provided by their embassy. Their tastes were not mine, and they had other associations through their Embassy, so we did not socialise to any degree.

The U.N. provided relief by giving some assignments in Europe which made it possible to visit home. By this date the 707 passenger jet was operating transpolar flights and the travel was tolerable (unlike my first transatlantic trip by a 4-engined propeller aircraft driven by an in-line power unit which entailed stopovers in Ireland, Iceland and Gander en route for New York).

The European assignments which punctuated the Fuchu/UN experience included the 1965 Quinquennial Congress of the United Nations on Crime and Social Defence which was held in Stockholm. I was given the task of rapporteur. It was hard work. Late nights, often after midnight, were required to prepare an acceptable version of the next day's business. Ed. Galway was the organiser as head of the Criminal Justice Section of the U.N. I owed much to him and his sorting out the position at Fuchu when the Strathclyde post disintegrated. He was a perfectionist in the drafting of U.N. resolutions. There were also off-site issues to consider!

Student protest was just beginning and demonstrations against some delegations caused trouble. Some delegates were sympathetic to the student cause, as was Jackson Toby, the distinguished professor of Sociology at Rutgers University. He was, I believe, representing an NGO (Non-governmental organisation) He was associating with a demonstration of students on the steps of the Parliament building when the Swedish

police decided to act. Jackson was thrown down the steps (indirect evidence only!) but I had to sort things out with the police. I had some sympathy with the police because Jackson was dressed for the part of protester rather than that of an academic. I later learned that this 'grunge' style was the height of fashion on U.S. universities and sported by most sociology types.

Administrative duties

I have said little about the things I did in the course of my official duty at the Institute. The work was not unduly demanding. I did, however, treat it rather more seriously than some members of the foreign (non-Japanese) staff. I was responsible for planning the courses and various ceremonials. I usually gave two or three lectures a week during the course sessions. There was considerable travel in Japan arranged by the Institute staff. Nominally these trips were visits to Japanese prisons or other 'correctional establishments'. I had to go along on these visits with the participants. The opening and closing ceremonies required appropriate speeches. It was, however, the informal contacts with participants from a large number of different countries which was most rewarding.

My assignment with the United Nations ended without any problems. I had been invited (on expiration of my term) to a post as a full tenured professor in the School of Criminology at the University of California at Berkeley. I was to become a real academic! As of August 1966 I was, technically a resident of the United States. This was the beginning of the great commuting period for the family when we lived in two countries, separated not only by a common language.

CHAPTER SEVEN: THE BERKELEY EXPERIENCE

Conflict, Conspiracy and Criminology!

At the end of my period of office with the U.N. I was already appointed to a chair at the University of California at Berkeley. Obtaining the necessary visas and work permits presented some problems. I had been an 'exchange visitor' within two years (the President's Commission attachment and the temporary release from the Home Office was arranged on these terms). The issue of work permits was not allowed in such cases. The reason for this restriction, I was told, was at the request of the British and not due to American restrictive immigration policy. Britain did not want to constrain 'exchange visits' by academics but was perturbed by the 'brain drain'. The two-year prohibition on the issue of work permits after an exchange visit inhibited the use of the exchange process as a means whereby British scholars could be 'looked over' in the head hunting process. The intervention of the Law Department at Berkeley probably facilitated my entry, despite this bar. It was, of course, part of the argument that I was 'entering' from the United Nations and not from the UK

It is perhaps fortunate that I had no real option but to accept the post. It would have been a tragedy if I had been persuaded to return to England. In retrospect, I find it odd that a number of British academic acquaintances tried to persuade me not to emigrate. I believe that they thought that they were giving me good advice. American universities, it was said, were great places to visit, but not good places to join. A Cambridge 'type' told me in a mock Cockney accent, that it was only those who were 'not of the salt' who went down the 'brine drine'.

I cannot remember now how it came about that I was 'landed' in San Francisco while Barbara was 'landed' in New York. I can only assume that I must have flown directly from Tokyo to the West Coast. But there were so many flights! 'Landed' is the U.S. technical term for acceptance into the country as an immigrant alien with rights of residence and employment with the issue of the 'green card' (which to my sight is duck egg blue!) If the facts of entry were not part of the continuous record on my official documents I doubt that I would have remembered that my wife and I entered America from opposite sides. Must be some significance in that -- but I cannot think of any.

Head Hunting and Politics.

The apparently unsolicited invitation to take a chair at Berkeley was not so mysterious as at first it appeared to me to be. It was probably not merely coincidental that Norval Morris, my predecessor at Fuchu, who was then Dean of the University of Chicago Law School, had been a member of the Graduate Program Assessment Council which had recently approved the doctoral programme of the Berkeley School. This procedure is known as 'accreditation'. It is a 'peer review' of proposed programmes and the members spend several days in the departments concerned. The recommendations of this panel

were essential before the School could offer a doctoral degree course and award doctorates to those who qualified. The expansion of the School probably gave rise to additional positions. Proceedings to get me 'on board' had begun several months before I ceased my Fuchu work.

Housing

Initially my family were with me (during the English vacation period) and we lived in a house rented from an anthropologist (Professor George De Vos). It was a residence of typical professorial status on El Camino Real.

Fortunately academic salaries in the U.S were at that time about twice that of the UK We could afford two residences, one in England and one in California and pay for our children to visit us while for the most part they continued their education in England. Berkeley salaries were a percentage point or so lower than similar status universities elsewhere because the environment, both academic and geographic was a great selling point. I certainly used the views through the Golden Gate at sunset as an appeal when trying to attract quality staff! .

Later we could manage with a small apartment and we selected one within a few minutes walk to the campus. We moved into the top floor apartment ('penthouse'!) of a block on Hillegass Avenue which ran parallel with Telegraph Avenue two blocks up the hill. This was certainly a 'central' location. We had a 'deck' (balcony) where we could relax or when the police helicopters were not directly overhead, even sun ourselves! This sun deck had a view over the second block of Telegraph Ave. We did not realise the significance of this at the time, indeed it probably had no significance then. It certainly did later! But I will not anticipate that. It also featured a grand view of Golden Gate bridge, and on a day both in the Spring and Fall the setting sun went down right in the centre span as viewed from our vantage point. The colours featured in many photographs and it was some time before we came to take the view for granted.

Campus Conditions

It is not possible to recount more than a selection of my experiences over the 30 months I was at Berkeley, and those which may be worth telling I cannot assemble into any systematic presentation. Berkeley was at the centre of the student revolts. The Viet Nam war was still on. The Free Speech movement had ended, but the organisers (activists) involved were still highly active. All kinds of protests were organised and then disorganised. The image of 'student unrest' as being organised from Berkeley had even gained international credibility. Even car stickers gave support to this perception. One which Jan, my daughter, gave me read, "I'm from Berkeley but I'm not revolting".

Berkeley had reputations of two kinds. It was internationally renowned for the scholarship but also for its dissidents. These two features were not new. It is also possible that politicians see intellectuals as threatening whether or not they are politically active -- they are unpredictable, and that's 'bad'.

Contrasts and Countryside

I cannot convey the excitement I felt at Berkeley at the challenge of the extremes of experiences of so many kinds. From the moment of my arrival there was always some unexpected happenings to deal with. Some were humorous, others serious, some very serious, some relaxing, some most stressful. There were features of the 'hippie' culture which were not unattractive -- to 'hang loose' was better than to be 'up tight'. There was a wonderful countryside, beaches, mountains and sunshine, and the occasional fog. Retaining a balance between different roles when part of the family were on the other side of the globe was demanding. But that was not the reason for my rather short time of attachment to the University of California. The termination of my period of service was sudden and somewhat dramatic. I doubt that anything I could have done would have resolved the problems and saved both my career there and prevented the eventual closure of the School.

The Telegraph Avenue Culture.

The administrative element of academic life was almost a side-show to the 'student movement' and academic work was totally overshadowed by both. Despite the situation and events I was able to carry out some research, conduct seminars and do some political work of my own.

Ronald Reagan (later President of the U.S.) was Governor of California and Chairman of the Board of Regents of the University. Pot was illegal and plentiful. Tim Leary's message had been taken seriously by many young persons and they had 'dropped out' if not also 'tuned in'. It took me some little time to realise that Berkeley was characterised by its chaos and to cease trying to see any logic in the happenings. Indeed the word 'happening' had a specific folk meaning that eventually I was to appreciate.

The favoured hangout for these students, would-be students and ex-student dropouts (with a few anti-students too) was Telegraph Avenue, proximate to the Student Union building and Sproul Hall, the steps of which made a very suitable grandstand for all kinds of demonstrations, with speeches by Cleaver, Mario Savio and others.

Academic Politics

Remember that this is my first real experience of a university, and now I was at Berkeley; the campus with more Nobel Prize winners per square meter (say, yard!) than any other! The politics of academic life came as a surprise. Here I was to find no 'ivory tower' but rather a mental motte and bailey! It could also become a battlefield. But I was not here to admire or "tut-tut" at any features of my new environment; I had a job to do and part of my task was to define it for myself.

As a civil servant I had been forbidden to take part in politics and when I was with the United Nations in order to stress the international basis of my role, I had sought to avoid national identification. Now, without realising it I was surrounded by politics of all

kinds. My time was to be disproportionately devoted to the political shenanigans of academic life.

U.S. University titles, significant differences.

Readers may be more familiar with the administrative structure of British academic life than that of the United States. There are words which refer to types of duties which imply different activities in the two countries and I think I should explain a few of those which will concern this narrative. A 'School' is an administrative division in a university, usually applied to the larger divisions. Departments (normally more than one) are divisions within a School. Teaching staff are referred to as 'faculty'. A School is headed by a Dean. Unlike in England, Deans are not religious. Indeed American universities are almost all secular -- they have to be if they are to be funded by the taxpayer. There are private universities which may or may not be of religious foundation.

The quality of the output, both of students and research papers, despite the accreditation system, varies widely, though the systems under the control of the major states are fairly comparable. In order to assist assessment of students who apply for higher degree programmes, faculty may refer to a reference book which provides ratings of every accredited type of programme in all the accepted universities. The best departments (within a University) are designated "Star" departments. (Parenthetically, this "star" assessment usually makes it possible for any school or department so rated to claim higher salaries for its faculty members). Universities and colleges which are not accredited are of no academic significance whatsoever. (Note: Schools which are acceptable and accredited for first or second degrees need separate accreditation before they may grant doctorates. Hence my note that Dean Norval Morris was a member of the accreditation team which approved the doctorate in criminology at Berkeley).

The Dean (head of a 'school') is primarily concerned with administration and chairs faculty meetings. A Dean may also do part-time teaching and/or part-time research. The Dean has direct access to the central administration and voting rights in the Deans' meeting -- a sort of controlling council. While a School normally consists of several departments, the School of Criminology was divided into only two parts. The major part was concerned with the usual 'criminology U.S. style', with its roots in sociology. The other part was concerned with 'forensic science' or 'criminalistics'. This was mainly chemistry devoted to identification and evaluation of evidence from the scenes of crimes. It was also concerned with research into various 'prohibited substances'. One of the Dean's duties was to receive from official sources quite large supplies of marihuana and to secure this in the school's safe. The criminalistics sector was carrying out work to test the toxic and other qualities of this product. The 30 years or more research into this substance has merely reinforced the conclusions derived at that time -- but, then and now, rejected by the establishment..

Teaching.

As for my teaching commitments I realised that I had little experience; my only prior practice being limited to my experience of briefing research assistants and field staff gained at the Wartime Survey and the Home Office Research Unit, and my previous evening-class part-time work decades earlier.

I could also claim some credit in that just before the war I had managed to supplement my Ministry of Labour income by teaching Pitman's shorthand and typing. I had passed examinations to qualify as a Pitman teacher and had taught, part time, at Clough College in Southampton. Some decade later all I could remember of the preparation for accreditation as a teacher of secretarial skills was that it included studying a volume of some 20 lectures on the principles of teaching. Though I was examined on this as well as the subject matter (and passed) it was no adequate basis for claiming to be able to 'teach'. But, then, it is not appropriate for university staff to 'teach' by using the same techniques as are deemed appropriate in Commercial or even Secondary schools. Oxford, and probably Cambridge and other British universities seem to assume that no special training in lecturing or instruction methods is required of any person who knows their subject well and has published research.

I opted to teach a course in research methods. I remember opening the first lecture of the series by telling a story, the origin of which is presumably lost in antiquity. Once upon a time there were two Native Americans (Indians). One was old and wise, the other young and inexperienced. They were fishing. They had not spoken: Indians are renowned for their cryptic communication. They had fished all day and had caught nothing, and had said nothing. They were about to go back to their tepee when the elder Indian felt a pull on his line. It was a very big fish. Both could see that was so. He wound in his line very carefully. On the end was a beautiful mermaid. Both looked, bogeyed, at her for a while as the old man gingerly detached the line. Then he gently put her back in the water. After a respectful period of silence the younger Indian asked, "Why?". Then there was an even longer pause until as Indians are reported to do, the elder Indian retorted, "How?". So you see, "How?" is the more mature question.

If 'how' is a serious question, I should ask how well I performed in my academic role. Subsequent feedback from students would suggest that I did not do a bad job. A Handbook was published by students for the information of freshers. This gave assessments of members of the faculty. My evaluation in this booklet included a statement to the effect that my approach was different from most teaching staff; whereas they always assumed that students were ignorant until they had proven otherwise, I began by assuming that students were intelligent until they proved otherwise. I think I can still take some pride in that assessment, which, if true, represents a style I would like myself to experience. I suppose I did not do too badly because I know that quite a number of my students gained recognition in the field for their research work and publications. Two of my doctoral students gained important posts in international bodies and at least two others obtained important research posts.

Students teach me a few things!

Apart from remembering my first lecture -- well an important part of it -- I had two further early experiences of teaching, each of which resulted in a complaint to the Dean. It was required that a mid-term test paper be set and the students given grades. I set a few questions in much the same way as I would in England. The students soon taught me the American 'tradition'. To help the students (or so I thought) I appended to my test paper a list of references which they might find useful. Bearing in mind that the library did not have enough copies of journals and books for every student to have access to a specific volume, I listed many alternative texts and left it to the students to use the resource material according to their taste. On presenting the test paper to the class there was consternation. It was a few moments before I could sort out what was wrong. "It's the reading list" complained a spokesperson. "What's wrong with it?", I asked. "Which of all this lot is required reading?" "Required reading?!" I echoed. It was the first time I had heard that term. But the meaning of the phrase was clear without knowledge of precedent! "There is no required reading" I retorted after a pause, "there is a required problem". I resisted the 'spoon feeding' of literature and tried to continue with the method of providing helpful suggestions rather than 'requirements'.

At the end of my first semester in addition to my difficulties with my objection to 'set reading', a formal objection was made to the Dean. A student complained that I was not doing my job. I was getting students to do what was essentially the professor's duty. It happened thus. I had given a series of lectures on the problem of formulating useful questions. This had dealt with survey interviewing and the correct phrasing of hypotheses. One strongly advised (required?) reading was the little book by Paine, "The Art of Asking Questions". I also had in mind that as Masters degree students a number would teach and would have to set examination questions. It was, of course, necessary for me to set an examination paper for this lecture series. (Students had to have a grade). I admit that the exam was unusual, but I would contend, very relevant. I found an interesting cartoon which showed an ape in academic garb retreating from the platform of an awards ceremony ("commencement") with diploma tucked under its arm. This scene was being watched by two gowned academics with the balloon reading, "So much for the true and false method of examination". I reproduced this as the frontispiece of the exam paper. On turning the page the students were faced with the examination task. "Write five examination-type questions relating to the content of the course. Answer three. Say why you chose to answer these three". The complaint was that it was the job of the professor, not the students, to write examination questions. In view of the "charge" I retained the "evidence" . .

I might add that it was very easy to distinguish the students who had got the message about the importance and difficulty of working up an appropriate question design. I think that the issues of question design are still important and seldom fully appreciated. Errors are widespread and even found from time to time in professional agencies. Too many questions are formulated in a manner which distorts the probability of receiving (or deriving) a worthwhile response. At that time I was particularly persuaded of the desirability of asking 'small' questions and relying upon their number to describe the

'dimension' of concern. The success of the medals and other demand estimates was due to this 'grapeshot' technique. I was also stressing the desirability of substituting 'how' questions for 'why' questions. I was, of course, not alone in this. Harry Henry, the director of Marketing Services, had written a paper entitled, "You can't ask 'why'" which received much attention in the Market Research Society.

I was not reprimanded for either of my attempts to bring a bit of British style to Berkeley! In addition to the doctoral and masters' programme there was a popular undergraduate programme which was mainly taught by final-year doctoral candidates. Teaching Assistant positions provided a means whereby graduates might gain experience and also obtain some payment to assist their studies. An appointment as a teaching assistant meant that tuition fees were waived. No relaxation of normal degree requirements were allowed and T.A.s attended seminars and prepared their plans for a dissertation. The School offered a Masters' degree but this was not separately tailored; It was an M.Crim, which I was told was a sort of consolation prize for those adjudged not likely to complete the doctoral degree.

Dean Lohman came quickly to the conclusion that my style was not suitable for teaching the basic courses. I was to concentrate on supervision of doctoral projects and the management of my own research.

I have to report that the indelible impressions left by Berkeley are not so much concerned with the academic aspects of life, as with the social and political involvements. Despite this state of affairs, a number of research projects were completed during my time there. I also, jointly with the Assistant Dean (Bob Carter), published two readers which sold well.

Violence -- Politics in undesirable form.

While I knew of Joe Lohman's background in politics and his practice of law as a 'public prosecutor' I did not make any general assumption that being a Dean involved party politics. Politics, I assumed, might have been tolerated but kept in their place: a hobby and not part of the job. I was wrong.

If times had been reasonably normal I might have been able to make some sense of the situation, but times were not normal on the Berkeley Campus. The 'police riot' at the Democrat's Convention in Chicago and the killings of student demonstrators at Kent State were fuel to smouldering fires in many parts of the country, but particularly at Berkeley. One of the professors at the School of Criminology (whose special area was policing) had some official duty at the Convention in Chicago in connection with the security arrangements. While walking in the city he saw a police officer beating an individual who was already cringing in the gutter, and he remonstrated with him. He was then himself badly beaten up. This experience had the effect of 'radicalising' many in the School. Several events, not only at Berkeley, resulted in a hardening into extreme positions: a dichotomy where the few who tried to remain in 'the center' were hit by both sides!

I doubt that even an experienced and trained historian will ever be able to make much sense of the situation; there was really no 'situation', but many independent and quasi-independent situations, and all situations were running in confused parallel.

The Administration of the School.

The School of Criminology reflected in many ways the personal views of Dean Joe Lohman, and this was particularly true of the graduate programme. The D.Crim (Doctor of Criminology) was very much Joe Lohman's invention. Lohman defended the School' s D.Crim by stating that in his opinion a Ph.D. would commend the graduate neither in the environment of police work nor in politics where their special expertise would be most valuable. The term 'philosophy' would have negative overtones to many in the criminal law business. No other Schools of Criminology or Criminal Justice followed Lohman's lead with this designation. This was because within an academic setting, professional identification in degrees (such as in pharmacy and some chemistry) would normally relate only to the Master's level. The other doctorate which identified the profession was the Doctorate of Divinity, and this, particularly in the United States, is not a subject for state universities. I suppose too, that a similar attitude was reflected in the rejection by London University of an Institute of Criminology.

I can accept that there were problems of 'territory'. Most of the course content would have been covered by Sociology departments in the sectors dealing with law and society. Indeed, Berkeley already had a Department of Law and Society which was a consortium of the Law and Sociology Departments, and there was a strong interest in the sociology of deviance in the main sociology department. Why then criminology? In addition t o the quality of Lohman's arguments, emphasising the crime prevention and police ethics, forensics and penal research Lohman's political strength doubtless added much weight to his case. Thus a Doctorate of Criminology (rather than the Doctorate of Philosophy -- PhD) was a unique degree and it identified both the School and the Dean.

Selling the credentials.

I soon became aware that Lohman and the School were not popular with a fair number of Deans of other schools and departments of the university. But this situation may have been no more significant than the attitude of 'hard scientists' to many other 'soft' disciplines including history, anthropology and certainly social welfare! I was myself no convert to the idea of criminology as a proper academic field. I do not think that criminology can stand alone; it is not well defined as a 'discipline' but is rather a collective of knowledge. It is better seen as an area of application of many disciplines and expertise and even of technology. Hence the idea of an 'Institute'. This implies that those who are involved in the area of application ("criminology") should have qualified in a base discipline such as sociology, law, statistics, certain areas of psychology and if forensic science was included, analytic chemistry.

Despite these comments, which are later interpretations, at the time I was extremely pleased to try to fit in and to contribute to the School. However, my underlying ideas on administration of criminology did not help with my teaching.

Observations on the School.

Lohman was probably more a politician than an academic, but he was very tolerant of intellectual variety. He knew that I was not really convinced that 'criminology' was a legitimate category, though the subject matter covered was fitting. The different lines of teaching and research might, I thought, be better packaged. Most could find an appropriate home in existing departments. Rather than a separate organisation I would have seen the school as a co-ordinating institute. It was known that my wish was to bring the School closer to the received disciplines of law and sociology, and to link these with the teaching of statistics and research design. As I learned more of the situation I came to consider that good relations with the Institute of Law and Society, headed by Selsnick, was essential. But Selsnick and Lohman did not 'get along', though I never knew why.

Lohman had difficulties on two (perhaps more) fronts. He was an important politician and had been Sheriff of Cooke County, Chicago. He was a very high level cog in the Democratic Party machine and it was widely believed that he was in line for Vice-President should Adlai Stevenson (a Democrat) be elected though his own likely ambitions would have extended to being Secretary of HEW. He knew the political climate well enough to know of the importance of “geographic balance”. . But that was not all. He was always thought to be in danger of his life because he had been responsible for prosecuting a number of powerful gangsters in Chicago.

I was first aware of this when at a party (it was probably my welcome party) an agitated member of Faculty came up to me and asked if I had seen Lohman and had I any idea where he might be. I was most surprised at his concern and asked whether Lohman had any medical condition which needed monitoring. He explained that he was seriously worried about the possibility of kidnapping or worse.

Britishers and Brooms.

While my colleagues and particularly members of the faculty were a powerful influence on the bread and butter operations with which I became involved, it was the students who provided the intellectual challenge. Several are still in touch. Among the doctoral candidates were four English 'characters'. These took me at my word and often challenged my thinking and made sure that being a professor did not 'go to my head'. With these four were an equal number of American and one Chinese who, by reason of their standing in the programme were provided with office accommodation (well, cubicles) in a large room (Room 114). This room was a refuge for me. I could go there at any time and be sure to be insulted! I visited frequently. The Britishers were Burnham, Kingsnorth and Carr-Hill and another who might prefer not to be identified. Burnham was an Oxford classics man who had been an Assistant Governor of a Borstal,

and could not be separated from his cricket. He became an international civil servant with the United Nations until retirement. Rodney Kingsnorth, was a graduate of LSE who later took some time out from teaching criminology to sing German Lieder (in Germany) then returned to the U.S.A. to a professorship in Sacramento. The third, Roy Carr-Hill, has subsequently appeared from time to time -- recently as a statistician in the O.E.C.D. His academic record before coming to Berkeley was the most distinguished. However, it was not the academic climate which attracted him to Berkeley, but the 'movement'. He was responsible for my close encounter with the Berkeley Police.

When Roy first applied to the School he came near to being rejected by my graduate secretary without reference to me because she was so sure that he was unsuitable. He certainly looked unsuitable. Just to check she called me and gave a brief account of her interview with Roy and clearly expected me to endorse her rejection, but added at the end, "He has a peculiar accent, and says he knows your work at the Home Office and wants you to be his supervisor". I said I would see him.

Enter the stereotypical hippie! Full father-Christmas length red beard and shock of hair slightly lighter in colour. No socks, sandals? -- I think so. Torn jeans and half a shirt. He looked like a tramp. However when he spoke, I was, I thought, able to explain why my secretary had said that he had a peculiar accent. It was an accent peculiar to British public schools. "Wellington or Marlborough?", I ventured. "Marlborough actually" he responded. It later transpired that he had a first in Maths at Oxford followed by a first in Philosophy at Cambridge (or the other way around) -- both being I understood on full scholarships. Why had he come to Berkeley? He was interested in the student movement and hearing of my role in the School had considered working for a doctorate. He was already well qualified as a statistician.

Roy became interested in the anti-Viet Nam war activities which led him quickly into direct involvement. But he had his own agenda. This was illustrated when he inaugurated a 'broom dance' in Telegraph Avenue. This had the latent purpose of suggesting that it should be kept somewhat cleaner than the street people were in the habit of doing.

I Become Acting Dean.

I had been at Berkeley for just under two years when Lohman suddenly died. The academic staff met and asked me to permit them to petition the Vice-Chancellor that I become Dean in his place. The reason for their decision was that they considered that my experience in the Home Office gave me a background in administration and that I would lack political bias. Foolishly, I agreed. It was a great honour to have the support of the entire staff, and I admit to some pride in such an association with a distinguished university. The Vice-Chancellor approved my appointment as Acting Dean forthwith.

The fact that Lohman had been such an important member of the Democratic machine was a positive feature while the Democrats were in power. The School prospered with research funds and had influence in the Committee Rooms in Sacramento and in Washington. Lohman flew the 'red-eye special' to and from D.C. far too often than was

good for his health. He had died and the Democrats were defeated. There was a Republican President, and more significantly for me (and the University of California!) we had a Republican Governor of the State; the one-time B-movie actor, Ronald Reagan.

My time as Dean was the most dramatic period of my academic career; it was also of very short duration. Before I write more about this period I must note one most disturbing event which took place while I was there. I refer to Berkeley's most tragic student, Gene Carte. As all members of the American Criminological Society are aware, Berkeley proved to be perhaps the only campus from where a doctoral candidate was murdered in the actual course of carrying out interviewing for his dissertation. The Gene Carte Award, which is given annually, commemorates this. Gene was interviewing in San Francisco. The most likely explanation of his death was that he was a 'mistaken identity'. So far as I know the case was never 'solved'.

An Official Home Office visitor.

I had not been more than a week or so in the Dean's office when the British Council (or Consul?) [16]contacted me because they were hosting a visit by a Home Office Minister. Since I cannot be absolutely sure of this individual's name, perhaps he might represent a British viewpoint from anonymity. His programme had been fixed but, at the last minute he had expressed a particular wish to see the method of execution (I think he believed that this would be the electric chair) which had been 'mothballed' while the death penalty was in abeyance. They wanted to know if I could arrange a visit for him to a nearby prison where there was a 'death row' -- if I knew where there was one. I told them that the Minister could not see an actual execution (!) but if he was visiting San Francisco he could probably go to San Quentin where there was a "death row" and all the necessary equipment. I was asked to see if I could obtain permission for the inspection by the Minister. I knew McGee (Director of Corrections) quite well and he enabled the visit. The Minister did not visit the School nor, apparently, ask anything further about the work of Californian Correctional Services. My friends in the Department were rather disappointed that he was so easily satisfied. I know that the art teacher in San Quentin was hoping to sell him a painting by an inmate!.

I confess that Home Secretaries (apart from Butler) have not impressed me with their abilities or applications. It would be hoped that persons appointed to this office should reveal a sound basis of reasoning for their views and determinations. I can think of no examples of any brilliant analysis but I do have two examples of an interesting if idiosyncratic logic. The British propensity to secrecy was defended (most ably!) by Merlyn Rees who is quoted as saying, "Unauthorised disclosure is wrong because it is unauthorised"[17] I think that must be true. Rather more doubtful is Jack Straw's objection

16 I know that the status of these two offices differs significantly and that I had previously insulted a Consul by assuming he was representing the British Council. But I cannot now say which office it was that passed on the Minister's request.

17 Evening Standard (p 1) closing edition, 21st February 1977

to a demand for a Royal Commission to consider drug laws and his defence of the illegal status of marihuana. His argument by analogy lacked something (say 'logic'!) : "Marihuana is illegal for the same reason as speeding is illegal". [18] (He was not referring to taking 'speed'!). It does not take a Mensa graduate to see that driving faster than permitted is not analogous to smoking pot.

Intolerance, Escalation, Violence

I was continuously lobbied by the left members of Faculty, and in particular, Professor Tony Platt. He was a graduate of Oxford. Tony's dissertation had thrown considerable light on the child welfare field. It was widely acclaimed as an excellent piece of research. It was published in book form under the title "The Child Savers". But Tony was 'radical'. Tony accepted the philosophy of direct action, which included breaking the law. He was arrested on several occasions and when things settled down was awarded compensation for being beaten by the University Police. His theme was that 'we liberals' should "stand up and be counted".

While there was, I thought, a strong case against much of the national policy, particularly in the operation of the war, the situation was muddied somewhat by the fact that students who were protesting about the war were exempt from military service while they remained in full time education. Many who could not avoid conscription by being students sought to avoid it by going to Canada rather than declare themselves to be conscientious objectors.

To the student movement the university administration was 'the enemy'. This was due to the fact that the university had used first its own campus police and then the Berkeley police department to clear demonstrators from Sproul steps. In the situation which developed no one could be accepted as neutral because there was no official neutral policy or ideology. The police mirroring the student view saw the campus as 'enemy territory'. My car had an official 'sticker' to permit entry to the campus and to my 'slot' by the office. On several occasions this fact caused some problems with the local police. When stopped I could point out that my 'sticker' was not a general campus parking permit, but one reserved for the Dean of Criminology. An apology was sometimes forthcoming, with an explanation that they had a broadcast order to "stop all cars with the usual stickers".

Diverted by Pancakes

However there were lighter moments involving the local police force. There was the occasion organised by Roy Carr-Hill (who else). The small British group and a number of Anglophiles were not all of one mind about the student cause, but when it was pointed out by Roy that the Americans do not celebrate pancake day, and that pancake

18 T.V., news interview (reference lost)

day was coming quite shortly, a fair number thought that a pancake party was called for. I was invited and saw no reason why I should not attend.

Assembling a collective of barbecue stoves and getting these going in adjacent student housing enabled sufficient production of the necessary symbols. I do not remember whether any turned out to be edible. But that was not the point. We were to have a pancake race. That also seemed reasonable enough. But then somebody claimed that in England the pancake race was headed by the Mayor (I don't think this is correct!). We had to follow tradition. "We don't have a Mayor, BUT, we've got a Dean". Well, why not accept the honour? I duly set forth around the block with my frying pan and its contents. Not many yards into my stride I heard the familiar wail of police sirens. Nothing to do with me, I thought, and pressed on regardless. I was wrong. "Stop!". "What's going on here?" "What's the meaning of all this? As though there was any meaning; a highly coded message in how high the pancake was tossed? "It's pancake day" I explained. "What's that Buddy?" (or words to that effect). An explanation was eventually agreed as satisfactory, but this was behaviour which was only permitted in private by consenting adults. We retreated rapidly to the confines of the yard. It was fortunate that we were 'dealt with' by the Berkeley force and not the Marin County force ("Blue Meanies").

Reagan and the Regents

The School had few friends when the Democratic Party lost power about half way through my stint. Reagan was only one of those who created problems for the School and for the university. The District Attorney of Alameda County, Edwin Meese, giving evidence before a Senate Committee made it clear that he was no friend of the School. He saw no point in having a School of Criminology which was “on the side of the criminal". Later, when during the Watergate affair he was a member of the White House staff he might have been pleased to find such an outfit!

The shadow of the Viet Nam war was ever present and had a major influence on almost everything one could think or do. It impacted on all universities with some unusual consequences across the country. Kent State with the shooting of student demonstrators by the National Guard was the most devastating incident and Berkeley also had its fatalities. Many years later one of my students told me that while he was working for his doctorate with me he was a paid agent for the FBI. How else can a poor American boy get through college?

Into this environment of agents and hippies and tear-gas I was thrust by my colleagues into the role of Acting Dean. I should, perhaps, have refused this honour and remained in my role as an 'intellectual' foreigner -- a mere professor who just got on with the business of teaching and research. I might have survived and the School might also have kept afloat. But though I had administrative experience, it was an entirely different matter to be projected into running the School and trying to remain aloof from politics. It was, perhaps, impossible.

Academic Concepts? 'Get Lost'

I soon came to realise that by accepting the honour my colleagues had wished upon me I had taken a lot more than honour: the task was not one which I could carry out and remain true to my interpretation of an independent academia. But I had accepted. It had seemed to be an ideal chance to put into effect some of the ideas I had about the development of the School and increasing its academic standing. I thought that it might be possible to find an arrangement which would link it with the Institute for Law and Society and put more distance between it and police interests. But it was not to be. I think that it is possible that my ideas would have gained acceptance with many of my academic colleagues and with those of related disciplines. I had managed to 'polish the skids' under one or two deadbeats on the staff and another I wanted to depart assisted in this by plagiarising (in a book which he claimed to have authored) a large section of a doctoral dissertation! Not surprisingly the student complained to me as Dean. The case was made more damning because the new book was submitted as evidence to support an application for promotion. He left rather quickly. More action might have been taken had conditions been more normal.

The School was moving in the direction I wished and further progress seemed likely. Academically things were going along well enough, and I was surprising myself in my role as academic administrator. The sudden termination of my appointment had nothing to do with academic life. I mention this now because I do not want the other incidents I shall note to be interpreted with an incorrectly assumed background.

Relations with dissidents

The faculty and students were divided on the Viet Nam War issues, though the majority of the students were not supportive of the official policy. Many did not get involved (voluntarily) in the situations which arose. The radicals, however, made their presence felt. There seemed to be a natural tendency for situations to escalate on their own, and some of the activists certainly consciously ratcheted up every incident. We would discuss the dissidents' strategy in my office at intervals, though I made it clear that my interest was like that of an anthropologist and I was not biddable by any faction. My function, as I saw it, was solely to defend and preserve the academic environment and the philosophy of inquiry.

I had several situations to cope with, and I was directly involved in one which may have resulted in some 'mud' sticking. The assistant professor (somewhat aged and tenured!) in the criminalistics department invited the local Chief of Police to speak to his students in a seminar. He was entitled to invite a few external speakers provided that the topic discussed was relevant to the course content. No one from criminalistics complained -- neither students nor faculty. However, I received a complaint from another assistant professor (the ex-Oxford UK, Tony Platt). I explained the entitlement to invite external authorities. He was quick to take advantage. If the police speaker was 'in order' then I must agree that it would be equally in order for him to invite Eldridge Cleaver! Cleaver was one of the leaders of the dissidents. In all fairness I had to accept. Imagine my

indignation when I found that instead of his using my permission to invite Cleaver to discuss in his small intimate seminar, he arranged a meeting to which he seemed to have invited everyone in sight! While the 'right wing' action had a low profile, this left wing radical action attracted far too much attention for the good of the School. To the authorities Cleaver was worse than a common and dangerous criminal. This sort of situation fertilised the dirt in which grew the urban myths which, unfortunately, tend to be far too robust.

"Call in the Army" Tear gas the lot!

After pancake day events moved quickly. The situation of warfare between the 'students' and the authorities escalated. Demonstrations were broken up by ever increasing violence and demonstrators similarly escalated their responses. Demonstrators were killed. Tear-gas was widely deployed. Usually it was possible to avoid the areas of greatest confrontation and get on with the business of education. But the tear gas was not selective, particular when sprayed from helicopters operated by the military. I received a fairly heavy dose while walking to meet my class for a seminar. So did hundreds of others who were equally unconcerned and just doing their usual thing! One or two retreated with me to my apartment on Hillegasse to try to obtain relief. But the military had suddenly and without warning changed the gas type from CN to CS. The remedies for the one did not work for the other, indeed they increased its effect!

Barbara was on a visit at the time. The next day I was due to do something at Stanford University so it seemed safer for her to accompany me, though there was not much to commend the trip except what seemed to be a better chance of avoiding involvement in the conflict. That day was the only day the authorities decided to gas the few demonstrators at Stanford! Berkeley had a day off.

But while all this was going on, research and writing was also going on. I have mentioned the two 'readers' which sold very well. Except for the encouragement and indeed managerial ability of Bob Carter, the Associate Dean at that time, I would not have contemplated undertaking the major work of editing and preparation of connective commentary that an acceptable “reader” would demand. Though we shared the title, I confess that he did most of the work. I merely wrote some of the connective material and an odd chapter or two! Bob later became a General in the U.S. Army and taught at the staff training college at West Point. We remain good friends and we have entertained Bob when he came to London to run in the Marathon; of all things!

With Bob and others, also around this time, I was director of a project (at least nominally) which studied the work of probation officers in relation to their training. I think that one most important finding resulted from this work. It was the importance of a phenomena which we termed the ‘culture of the office’. A term which has since become almost common usage. By this we meant the informal socialisation of workers in an environment. It seems that this quickly blurs the results of training which are not concordant therewith.

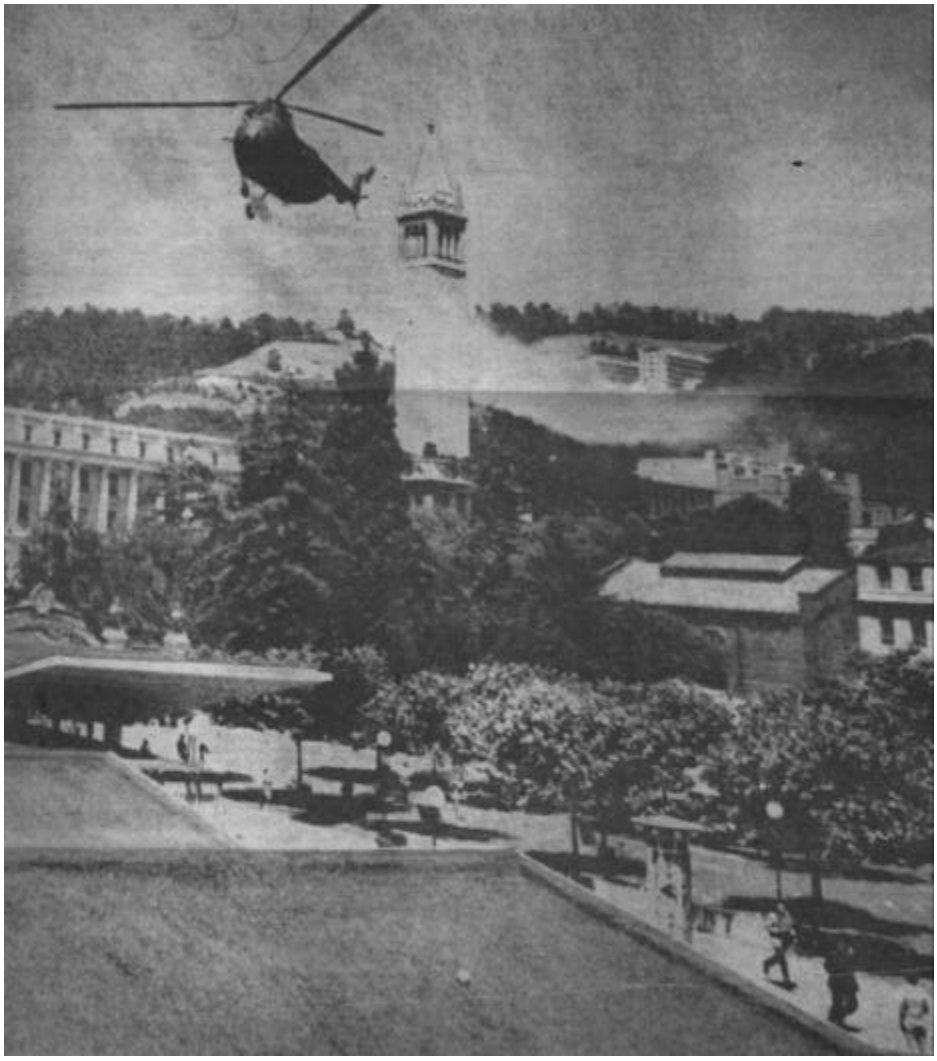


Figure 8. Tear gas is sprayed over the Berkeley campus by helicopter.

The Sausalito Conference -- an important beginning.

In addition to research by the members of the School, I was associated with Don Gottfredson who at that time was Director of Research for the National Council on Crime and Delinquency (NCCD) located at Davis. Don and I noted that there were quite a number of research units beginning to get started on funds provided by the Department of Justice and the Law Enforcement Assistance Agency. I was concerned with the issues of managerial techniques in research (as my comments on my time at the Social Survey have illustrated). It seemed to be a good idea to call together a meeting of directors of research in the field to discuss funding, management and staffing. We also had in mind inviting the commissioning agencies. Accordingly Don and I put up a project for a few days conference in Sausalito on the north side of the Bay. The success of the conference may be gauged by the fact that the conference was repeated each year thereafter and is still going. I am not sure whether the original organisational plans are

still operational, but we intended that the conference should be hosted by different agencies each year in much the same way as the joint NCCD/School project. It was pleasant to have professional associations outside the School and university, and the NCCD was just sufficiently distant to secure some quiet and do some thinking.

The end of my Deanship was precipitated by the student movement conflict. However before I record the simple facts of the case I have to take note of the ways in which conflict situations, such as those at Berkeley at this time generate all kinds of absurdities. Almost anything can be believed by either side once the dichotomy has deepened. Rumour and propaganda are powerful weapons and often capitalise on dramatic incidents or information lacking in clarity with unfortunate results. These conditions often spawn the 'urban myth' phenomenon, and many of these are extremely robust and enjoy extraordinary long lives. I have an example!

Urban Myths and "Investigative Leads"

It was not until 13 years later that I learned that a myth which originated at this time (1968) had not only survived but was apparently being further exploited. Late in 1981 I 'obtained' (and still have) a copy of a publication titled "Investigative Leads" which purported to tell of events at the School of Criminology at the time of my departure for Albany. This document contains the following: " ... Rand Corporation ... and the Berkeley School of Criminology that (are?!) directly responsible for creating riots...". The suggestion that the School was in any way 'responsible' for 'riots' is absurd. I am sure that the Rand Corporation would be amused to learn that they were connected either with the School or its "conspiracies"! However, it is not the outrageous comment which is the main feature of interest but the date of publication, namely August 1981. Riots on any campus were not then an issue. Why raise this myth at this time? Perhaps it was a republication, but if so, this was not disclosed and the reason for its republication is equally peculiar.

Another quote from the same 'report' is more explicit. "In the United States during 1969-73 Berkeley Criminal School Dean (sic) Wilkins recruited a group of radicalised students into a project paralleling the British Deviancy Conference." And again, "Wilkins' principal protege during the Berkeley period was Tony Platt. ... he became involved under Wilkins tutelage as a behind-the-scenes controller of the Oakland Black Panther Party." How absurd can "intelligence services" become!

But, of course, the document also contains some true materials. That is what gives it any dangerous aspect it might have. Readers knowing some statements to be true may assume truth of other statements which they do not know to be incorrect. Some quotes in the "Leads" I could accept as true. For example it claims to quote Tony Platt verbatim, and I think it is probably correct, though they do not quote their source. "It is the obligation of intellectuals to both develop theories of oppression and exploitation and to participate in the process of transforming society. Criminals have written better books on crime then criminologists. Take for example the work of Eldridge Cleaver, Angela Davis, Sam Melville and George Jackson". Marx had said the same about

criminologists and criminals much earlier! I will leave this as a mystery because I was not aware of any interest by "Investigative" anything until 1981! I will say a little more about that when I get to 1981 -- the date of publication of the defamatory document.

Strike and Strike-breaking

My term as Acting Dean ended somewhat abruptly. A strike had been called and many of the faculty, and most of the students, participated. There were, of course, picket lines. It was my view that academic activity should continue, but there was no need to make this confrontational. Faculty, unless they too had opted to strike, should, I thought, continue to offer to meet students, but that the location, if mutually agreed, was irrelevant. I could see some analogy in that classes often met outside the confines of the university on 'site visits'. I could not, however, see any educational principal which would dictate specified locations and the consequent crossing of picket lines. Such a demand would be putting students at unnecessary risk.

Educational need v. political power.

I was shocked when I received a memo from the Vice Chancellor (Berkeley Campus) requesting me to inform him of the name of any member of faculty who had "not met their classes at the designated times and locations". It was made clear that any such persons would have salaries deducted and probably be liable for other penalties due to 'breach of contract'. I was told that I must find out what had happened.

To meet this demand from the Administration I put out a notice asking any students who had wished to continue classes during the strike but who had been unable to do so to let me know. I received no complaints about difficulty of class attendance where the student so wished, with or without crossing picket lines. I was satisfied that the academic activity was proceeding as normally as possible. My own apartment was situated as near as the usual lecture rooms and some seminars were held there. Another member of the faculty who wished to respect the pickets conducted his classes in a church hall in the same block as the university. This seemed satisfactory to me.

But it was not sufficient for the Regents that I and the students were satisfied, and that work had continued. They insisted that the 'usual location' was the dominant concern. I could not agree. I found this morally unacceptable. It was obvious that the Regents were not concerned with education but with politics -- they had wanted to force faculty and students to cross picket lines. It was a strike-breaking move. No prior warning of the intended punishments for making other arrangements for meeting students requirements was given.

I objected to this treatment and to the demand that I act as an informer. So I responded to the request for information with a carefully worded and respectful statement saying that I had "taken action" and was able to assure the Vice- Chancellor that all educational requirements had been met. I had made thorough inquiries and I had received complaints from neither faculty nor students, as to any failures on this score. I made no

reference to locations. This as I had rather anticipated was not acceptable to the Administration and I was pressed to carry out inquiries and to reply in full to the request for information. I was advised that my refusal to comply was a 'disciplinary matter' which would be referred to the Council of Deans. I persisted in resisting giving information on the location of any student/faculty meetings. My duty as Dean, I claimed, was met if I was assured that the location was satisfactory to both parties. Moreover I thought it reasonable to protect students from the risk of violence from pickets. In any event had not the authorities recently subjected the 'designated' lecture locations to tear gas raids from the air?

I can only assume that the Chancellor was being pressured by the Regents and probably in particular, one Ronald Reagan. There was at least one further telephone call, this time from University President and a final warning to me that if I did not comply, action would be taken. The matter of my refusal to respond to the demand for information on location was placed on the agenda for the Deans' meeting; with no reference to my name or means of identification. I was entitled to attend, and did so. The meeting was asked to suggest the action that should be taken to 'deal with' the dissident dean. It is probable that few, if any, of the deans suspected me -- there were at least two others who h ad made their radicalism more obvious. I remained anonymous when the vote was taken. So far as I remember only two deans were in favour of taking no action, but these had both been labelled 'fellow-travellers'. I was not myself an advocate of any political position, or so I thought, but merely a defender of academic freedom. I was not happy to be associated with my supporters.

Before the Administration could take any further action I published my resignation. I pointed out that I was not concerned with politics and that being Dean of Criminology did not include among the duties the function demanded. The Oakland Tribune ran an article headed 'Dissident Dean', and gave the story a reasonable coverage. They did not suggest that I was subversive; and it seems that the word "dissident" was carefully chosen and probably the most appropriate. My resignation and reasons were public knowledge at the time and have been discussed in society journals.

Dissident Dean

I find it interesting at this distance in time and place to re-evaluate my action. Was I right? I still think so. I think I rationalised my position by the logic I used in Japan. I was not a citizen of the United States. I was a foreign academic and my concern was not the country's politics but its educational activities. The Viet Nam war was not my war. I did not approve, but I disapproved from a foreigner's viewpoint. Since I was not permitted to vote in the U.S. I had no political identity in that country. If I was not entitled to vote in the country I should not interfere in the country's politics by other means. This position I saw as somewhat analogous to being (as in Japan) a United Nations international civil servant. Then it was inappropriate for me to seek to influence Japanese politics from the perspective of an Englishman but appropriate only in the role of an international civil servant. The concept of role had obviously had an impact. Here I was not in an international role, but an academic role and I should move within the

constraints of that role. Clearly if I was to act otherwise I would be confounding political with academic independence.

I might deplore the confounding of politics (particularly the politics of the right) with the function of universities, but the situation did not allow me to take an independent position. There were only two sides -- I was not disposed to join either the students' revolt nor the opposing forces. The middle position was bombarded by both sides and it was a lonely and totally ineffectual position. I could only duck out from under.

I was no longer in a position to defend the School from the Regents and the derision of Meese and his ilk. By refusing to 'rat' on my staff I had, according to their belief, shown that I was on the side of the strikers -- criminals all! As with most conflict situations, escalation takes place as it were, 'naturally'. The middle ground had become eroded and with Reagan the simplistic Bible ethics ("He who is not for me is against me") defined the situation. I could not avoid the ethical conflict without changing my situation. I considered whether it would suffice if I were merely to resign as Dean. This option lost any significance when it became clear that I was to be deposed anyway! Merely resigning my post was then that which was desired and if I volunteered, this could not express my moral disapproval of the demand nor provide a basis for my claim for academic freedom. I would have to resign both as Dean and relinquish my chair.

Then what? It looked grim.

It did not seem likely that I could obtain a reasonable post in the UK. The establishment still took a poor view of my departure from the civil service. I should, according to Treasury expectations have accepted transfer to another department until a different Home Secretary had been appointed. I do not know how much this was a general view, but I know I was then seen as 'disloyal' (and perhaps as likely to prove disloyal again). Moreover, my identification with the student movement would be assumed, and that did not commend me to any university administration in England. Fortunately, however, there was the State University of New York, and my Berkeley 'dissidence' was not counted against me.

Postscript -- some odd items

The foregoing is, I suppose, a sort of bare bones record of my time at Berkeley. I fear that it fails to give any indication of the tensions and dramatic situations. The few papers I have retained from that time and the tapes which I have still not erased are reminders to me of the intensity of the experiences. Perhaps a few documents which sample some different perspectives are worth enclosing with a short commentary.

I have not, so far, mentioned the "Peoples' Park" drama which featured very significantly, and symbolically, in the student unrest. At least one whole book could be written on that off-campus feature alone. In essence it transpired that the University demolished an old building and proposed turning it into a car park. However, when the ground had been flattened there was some delay in starting the buildings. In this interim, the 'street

people' (no doubt aided and abetted by students) decided to treat it as a kind of 'squat' and to cultivate it as a park. I walked past it on the first morning of this activity. Nothing seemed unreasonable. Overnight a few flowers had been planted and work was going on. (That, I thought, was a 'good thing' for the 'street people' in any event!) But there was to be no accommodation. The police decided to 'protect' the private property and 'cleared' the site of the squatters, then erected a high razor wire fence around the whole area. This, of course, was the key for further escalation. I was not involved in this matter in any way, but I could not help being a close observer because of the location -- nearby my apartment.

One of my doctoral students who produced one of the best theses of the School became involved. He later fell, or was driven from, acceptable society and was, at one time (probably correctly) reported to be growing marihuana in a sector of the National Forest! During his transitional period he tried using humour to de-escalate the conflict which surrounded the issue of the "Peoples' Park". Clashes with the police were frequent and he was arrested several times for peaceful protest. I was careful to make no pronouncements on this case. I was not on either side: the 'street people' were not of my constituency! Despite my attempts to remain and be seen as uninvolved I must have been seen as somewhat friendly to the Park case, because I was designated a Citizen of the Free State of Berkeley, and presented with a certificate. The presentation of the certificate was made by the unnamed student.

As Dean of the School I was responsible for research in the criminalistics sector as well as the more general criminology. One of the duties was to receive and secure in the school's safe deposit large quantities of marihuana -- the police having seized these 'substances' passed some over to us for research. It is interesting to note that the results of the research have only now been fully justified and become the expressed view of the majority of the public of the effects of using 'pot'. Now, (1998) California has legalised the use of marihuana provided it is dispensed on a doctor's prescription! I was convinced of the rigour of the research design and the soundness of the results some 30 years earlier.

Another retained document certifies that I am an “ordained minister of the Universal Life Church Inc." This was an anti-Viet Nam War ploy. Ministers of religion were exempted from military service and the idea was to 'ordain' anybody who might otherwise be conscripted. I was not in any such danger, but, my ordination was intended as a 'token'; though precisely what it was a token of, I still do not know!

The riots led to my being tear-gassed (see Figure 8) and eventually to the strike and picket lines. Few people could now envisage the scene without pictorial assistance.

Testimony as to the political intervention in the academic affairs of the School was also preserved in an original document of the report of Senate proceedings. This is a mere sample of the political pressures which were aimed directly at my work. The enclosed document has some special interest because it contains a note of the views of Senator Meese Another indication of the state of play between the School and the Senate is

illustrated in the more genial reporting of the San Francisco Chronicle of 14th September 1967, but this dates to a time before I became Dean.

In writing up these experiences I have probably given the impression that the critical division was between the 'political' and the 'educational' perspectives. Perhaps this was so if I were to identify the 'political' with the definition of the role of the School as seen by Meese -- as 'training' rather than 'learning'. The School certainly was divided on the 'training' v. 'learning' perspective and Lohman's emphasis upon the former with its 'credentialing' function was at odds with the general academic image the University had of itself. The interesting point here is that the 'political' perspective on the proper function of universities was agreed across party lines! Lohman was a Democrat, Ronald Reagan, Chairman of the Board of Regents, a Republican, and they probably did not agree on any other issue. My orientation moved rapidly from an original attempt to seek an integration to identification with the academic. The expectations of the students of the School were divided, a proportion doubtless wanted to obtain credentials (and with as little effort as possible), others came seeking educational objectives. The School catered for both. I taught research methods (? vocational ?), and the Criminalistics sector while carrying out basic research might have been seen as providing training in forensic procedures.

The last thing I would wish to do is to leave the impression that my time at Berkeley was 'unproductive'. It certainly had 'redeeming features' and these were so significant as to negate the effects of all the undesirable situations and events. Among my students at that time I can name five who have become recognised internationally for their scholarship. Not that this had anything to do with my behaviour. I am sure that the key factor was in the qualities of the 'input material'. These students came to Berkeley, even during the '60s because they were attracted by the image of learning which the name Berkeley suggested. It was quantity and quality of published research across the spectrum of knowledge and the recognised standing of the faculties that were the appeal. Those students who subsequently have 'made their mark' were not in search of 'credentials', but attracted by the scientific standing of the total environment.

Unfortunately as things began to settle down in the 70s, throughout the whole university system, the emphasis came to be placed more and more upon 'credentialling'. The political perspective also began to extend its damaging influence to the direction of research demanding that emphasis be placed on funding "near market" projects (none of this "blue sky stuff"). This battle continues with academics (notably in England, the Oxford Defence of Science Group) fighting a rearguard action against short term funding perspectives.

And so to Albany

To continue the history -- it was 1969. I was going to become unemployed for the first time in my life unless ... A year earlier I had been approached by the newly formed School of Criminal Justice at Albany. Hans Toch, with whom I had co-operated in research many years earlier had joined the staff under the deanship of Dick Myren. I

telephoned Myren. There was no vacancy. The available posts had been filled, but I could reckon on having the first to arise. Within a week, Albany had dug a special hole for me and, taking my tenure with me, I resigned on ethical grounds and moved across the country to a chair in the graduate programme in criminal justice, now known as the Rockefeller School of Public Administration.

CHAPTER EIGHT: UPSTATE NEW YORK.

In the time between ceasing my duties at Berkeley and moving to Albany I was able to spend almost three months back in England. In my family, I think there was some slight measure of satisfaction in my forthcoming move to the East Coast. It was several hours nearer to England, and Laker Airways made regular daily crossings (the flights were referred to as the Atlantic Train). Tickets were bought on the day of flight or one or two days before. It was a 'stripped down' service with DC10s. For the first year or so Laker provided no in-flight meals; one could buy sandwiches before boarding. Later it was possible to buy sandwiches on flight. The fare in 1968/9 was about £50 each way.

The Dispersed University

Albany is, of course, the capital city of New York State and accommodates the Senate and much of the civil service as well as the university. It is also one of three major towns, which share the airport -- full title "Albany-Schenectady-Troy. Troy was an old town on the Hudson; Schenectady was the base of the national research laboratories of G.E.C.

The university comprised two sets of buildings downtown and the main campus, a new development, some five miles to the west. This development not only accommodated the increased number of students but provided an opportunity for some of the more distinguished older 'downtown' campus buildings to be repaired and adapted. The new 'uptown campus' was built next to the main State Government Offices and Police Training College on what had been a municipal golf course. The buildings comprised a central area - a large enclosing block of lecture rooms, dining and student union facilities, library, art gallery and concert hall. These buildings surrounded a square with a bell tower (hiding the water system) and large fountains. The central complex was some distance from four towers which were originally planned as student accommodation, but were soon taken over as offices for various faculties.

The whole complex was of white stone and certainly an elegant architectural design, but it seems that it was designed for a location which did not experience severe winters and the occasional heavy fall of snow. There were no undercover walkways between units, nor between accommodation towers and offices, except within the main block where there were in-service tunnels which were supposed to be off limits. But when stair wells were full of snow, there was no other means of access. The lighting was diffused by large circular bowls with many suspended between pillars. But a proportion of these were not under cover. With the first snows they filled up, the weight proved too much for the supports and the elaborate illumination crashed to the ground. It was rumoured that the design had been purchased by the state financial office who got it cheaply because it was not taken up by those who originally commissioned it. The design was planned for a tropical area, rumoured to be Saudi Arabia!

I could believe at least part of this explanation. I had only to assume that the architects and administrators of the University at Albany were as thick as the British Medical Military who were responsible for Netley Hospital. This had been designed for India when the British Raj was running things there. So if a hospital can be misplaced by a distance of a continent, why should I doubt that a university could be similarly misplaced and for somewhat similar reasons?

Accommodation with beer cans.

If our housing position had any impact at all on my research this derives from the fact that we always had a 'base' in England -- we never completely uprooted ourselves from the UK and the family did not develop any identity with the United States. I am sure that these physical circumstances were influenced by prior events and had a considerable impact upon many aspects of our lives. I leave readers to make their own inferences as they read on. The emotional pull of 'roots' was not for me a significant factor but it certainly was very important for Arnold who never adjusted to 'foreign ways', though he owes his research career to events arising from the American educational system. I think that other members of my family may have hidden their feelings from me, but I sense that they possess a sort of British patriotism which I do not share.

In the Albany area a large proportion of employment was in the civil service, electrical and electronic research and medicine. This meant that the population was unusually well-educated and the state and local government provision for education and cultural opportunities in the district were much greater than might be expected in an urban area of about one million population. The city had its own symphony orchestra and, of course, the university had both an art department, art gallery and a music and drama department which provided entertainment for local residents. The radio programs too catered to an intelligent audience. There was available both PBS (T.V and Radio) and two university-run radio stations. There was always available a choice of good music. Non-music programmes were also quite substantial, such as, for example, the broadcasting of the luncheon guest's speech at the Washington Press Club, live, daily on Albany Medical programme. I would have to say that from my perspective the quality of life at Albany was pleasant.

Albany is significant also for the fact that this was where I bought and set up my own home-based computer: before the first IBM PC clones. The operating system was CPM, which lost out to MS DOS a few years later. It was, however, possible to programme in something like simple Basic.

The University had reserved an apartment for me in a block which they owned and where sundry faculty were housed. I had the 'furniture' and car shipped from California. I put the word 'furniture' in quotes because it consisted only of the packaged chairs from Japan, an odd lamp, the tape deck and radio. Bookshelves were planks supported on beer cans (empty), and I used an air bed and some camping equipment. It was a Spartan existence. However there were several eating places within a few meters,

including a Howard Johnson, Tom Sawyer, and Dunkin Donuts. There was also the university's dining commons.

The floor above me was occupied by the University bassoonist, who unfortunately practised regularly and at peculiar hours. Sound insulation between floors is difficult because of structure borne noise. It was, then, with some urgency that I sought to purchase a house.

House purchase - Albany as a family location.

I was fortunate in that one of the doctoral candidates at the School was a practising lawyer (he made slightly less noise than the practising bassoonist) and he knew another attorney who was moving from a house very near to the Campus. (What lawyer does not know another lawyer!). I was able to purchase 14 Brookwood Avenue. A one-floor wood-built (shingles) dwelling with a basement and attic. The basement ran under the whole house and was large enough for a workshop (a full-sized bench had been left by the prior occupier with a lathe and vice), a table tennis table, a billiard/pool table and laundry area. There was also a large old furnace, now oil-fired, but adapted from solid fuel. It was so inefficient that it never went wrong. The attic was not heated but it was a useful store. By US standards it was an 'old' house, being built about 30 or 40 years ago. The central heating radiators were the main clue to the age -- large cast iron. But everything worked, and when it did not, access to all 'services' was very easy. There was a spare building plot adjoining, covered with about two dozen pine trees. There was also a garage at the end of a drive long enough to park four (American) cars. In winter, when necessary, this long drive had to be cleared of snow.

Brookwood Avenue was a dead end, branching off one of the main arteries leading out of the city of Albany. Traffic on the main road was speed restricted. On one corner was the local firehouse. (Eng: fire station). This proximity reduced my fire insurance premiums quite considerably.

In addition to an excellent city 'bus service, the university 'bus circled the university campus buildings and also connected with the downtown units every ten minutes and at peak times, almost continuously. This was free to students and staff. The old downtown section was mainly student accommodation, additional sports grounds and a few departments, including at that time the School of Criminal Justice. Thus while I had my home near to the main campus, I was some five or six miles from the office. This situation was to last for about a year when the School moved to the main campus, only to move downtown again later. These moves were not the 'productive' moves designed by Moss of the Survey. More on that topic later.

Life at Albany was peaceful and even the university's difficulties did not disturb me in any personal way, nor influence my status. The area had available as much culture as we could accommodate. In addition to the museums, concert halls, a local symphony orchestra, there were all kinds of ethnic foods available, including excellent Japanese and, of course, Chinese as well as Italian, Swiss and plenty of Jewish restaurants. There

were folk music and folk dance societies for Armenian, Jewish and even Morris, and probably more. There was plenty to do when one was off duty. In winter we tried some cross-country skiing. We had friends who lived in the mountains on an (almost) self-sustaining basis.

Albany was a very suitable location. There was a very good air service to Washington, New York and Chicago. The airport was accessible within 15 minutes from our house and the university, even at rush hours.

Working environments: the zoo, and others.

After rather less than a year the School was, as I have said, moved to the new campus. The offices were located in the lower floor of the library, and below the general level of the walkways. Access was by external stairway, except in winter when the stairwell filled with snow. The access was then through the service tunnels at basement level. While that may seem somewhat odd, it was also odd that all the offices of the senior faculty were glass fronted on to the stairwell. Students used to gesture and offer peanuts! Fred Cohen kept his curtains drawn continuously and worked by artificial light.

Later we moved to the top of one of the four tower blocks which had been designed as student accommodation. The one feature which gave evidence of this was the windows. -- it was impossible for a body to get out! Design precautions against suicide meant that the rooms were not well suited as faculty offices. During a gale it was impossible to hear telephone speech because of the howling of the wind in the decorative louvres of the windows. Each floor was comprised of four accommodation units. For housing purposes the segregation of small units was probably a desirable feature, but segregation of faculty of the same discipline, not only by separate floors, but also within floors was most unsatisfactory. The main social contact was while waiting for lifts! But it was not expected that lifts would be used for distances of one floor. There was also an adverse factor in the symbolism of height and the direction of view provided by the location. The administration were, of course, on the top floor. However, there proved to be a natural justice. They were repaid for allocating to themselves the best view; when it rained water seldom penetrated as far down as my floor -- just one below!

Naturally we moved again. This time to specially designed refitted old buildings in downtown Albany. The architects consulted with faculty before finalising the design of the internal layout. The exterior was covered with ivy and protected as a listed building. Car parking was at the rear and on a detached lot. Faculty offices and some services were located on the first floor (second floor in U.S. terms). The floor layout was of the piazza style with offices around the periphery. This meant that the central area was crossed whenever one visited another member of the staff. Information was displayed in this area. There was a water fountain and comfortable seating. All faculty were housed with the administration on this one floor. The lower floor housed the ancillary services and dining commons. Libraries were in adjacent linked buildings. This seemed to me to be the kind of environment which was desirable to stimulate interaction between faculty and students. The lounge facilities were appreciated by the students. A large notice was

printed one day with the words and question, "Still Life?", and placed over a sleeping graduate on one of the benches..

Academic environment

The academic environment in the School was congenial. I did a year as Chairman of the Faculty and this went quietly except for a slight difficulty. A resolution was passed banning smoking in faculty meetings, but Hans Toch would not attend unless he was allowed to smoke (and chew -- which he denied) his Havana (?) cigars. We compromised by taking a break when he could retreat and add one further health risk to his score. But Hans never really took probability seriously!

It was also pleasant that our house was near the main campus. Students were a great pleasure to work with. There were many informal sessions in the basement of 14, and the table tennis table proved a useful adjunct to the discussions in doctoral seminars.

Cross-disciplinary contacts in dining commons.

Other stimulating and pleasurable experiences were the lunch meetings with members of other faculties. In particular I became most friendly with the professor of Astronomy --- a theoretical physicist who was much concerned with the development of the University's computer systems. He was also a fine flute player.

Stephen Temesvary was a German. He had been trapped into carrying out some calculations which resulted in the development of the V2 rocket -- one of which came close when we lived in Hampstead and I was on a course in Regents Park; it dropped on Primrose Hill. Stephen endorsed the idea of collective guilt and was willing to accept that Germany was collectively guilty of war crimes. He was also willing to discuss how it came about that scientists were deluded into thinking that their work was 'pure research' and independent of the war effort. The tasks assigned were broken up into small problems which might have had almost any or no utility. This procedure amplified the common idea among scientists that they are not concerned with the ways in which their work is deployed. He would tell students how it was some time before he came to realise that his highly theoretical work was being linked with other work of which he had been kept in ignorance. When he came to realise what was happening he became associated with the German underground movement. He was also a good singer.

One series of seminars was arranged on the philosophy of science in which Stephen and the professor of philosophy as well as myself were all three jointly involved. I cannot speak for the students, but we enjoyed the experience. I doubt that few Deans would allow such expenditure of faculty time. Stephen and I saw most academic issues in similar ways. He kept me up to date with developments in subatomic physics and quantum theory. I learned much from him.

Unfortunately during a period of financial cuts his department was closed and he returned to Germany and a chair at Freiburg. He died quite suddenly. He tried to make

contact with me urgently when he knew he had only a short time to live. He had a cancer of the type which develops extremely fast, and he had only about two weeks notice. I booked a flight to Munich as soon as was possible. I did not panic as I should have done -- I could have made it a day earlier or even perhaps a little more than that. But I missed this last appointment with him. I very much regret this. He could, I am sure, have told me much more and I would have liked him to tell me.

There were also members of other disciplines with whom I found it most pleasant to associate. In addition to philosophy there was the head of biology and the resident composer. We all had common ground and overlapping interests in the computer. It was these opportunities for cross-disciplinary discussions which had considerable influence in stimulating me to take information theory seriously and to adapt some of its concepts to my research. It may be remembered that I thought that it was the 'mix' of disciplines which was a major factor in the success of the Social Survey research. It may be so in general.

Many of the faculty at Albany were more than mere co-workers; we were friends. The department was newly formed and perhaps recruits tended to be attracted to each other as well as to the idea upon which the department had been established by the Dean, Richard Myren, a non-traditional lawyer. Myren saw his task, much as Moss had done, as finding people who had ideas and providing for them an environment in which they could be productive. He protected his staff from interference from the University administration and political buffeting. Even when he disagreed with policy passed by a faculty meeting, he would, as his profession of lawyer enabled him to do, present the case to the administration and fight without reserve for it.

Research: Money and Management.

Because the School of Criminal Justice was concerned only with doctoral candidates, and there were around a hundred at any time, it had to have a large research facility. Members of the faculty could apply to private foundations (such as Ford, The American, Mellon &c) and/or to government departments (such as National Institute of Mental Health, National Science Foundation, Department of Justice), for support for research projects. Research students could then work on these as assistants, receiving payment and also often obtaining their degrees from secondary analysis of the data which became available. When I first arrived, the University's Administration, through its Research Foundation held and administered all research funds, supervised the costing and allocated moneys for 'appropriate' expenditure such as equipment or travel and student/assistant rewards.

I shall now have to separate into two stories two sets of events which over time were coincident with each other. Research projects, irrespective of the type or area of inquiry have to be 'managed' and an important part of the management concerns the ways in which the funds (usually provided by an external body) are controlled. It is usual for the managerial features to be separate from the operational aspects of the research project. Individual or groups of research fund applicants may be organised through an

administrative structure which may look after many different projects. Often there are research sectors within larger organisations, such as the National Council on Crime and Delinquency where the research element was proportionally quite large. Research carried out by members of a university staff will usually be administered by a department within the university. Such was the case at Albany when I arrived. This feature gave rise to considerable controversy. For the purposes of reporting, I will first follow through the managerial and administrative issues which were, of course, independent of the topics of my research projects. In other words I will first follow the 'money line' and then return to follow the 'ideas' line over similar periods of chronological time.

The "Research Money" Line.

I objected to the whole operation of the University Research Foundation and particularly to the taxing of my projects with overheads which bore no relationship to the actual costs, but were supposed to pay for supervision and to cover all research 'common services'. Thus, when making a research application the proposer and designer of the project had to include an 'on-cost' and to justify it to the funding agency. Each project, therefore, not only had to satisfy the foundation or agency in terms of its scientific content, but it would obviously not be funded if the total cost was unreasonable. Often the 'overheads' figure (percentage taken for on-cost) had to be 'negotiated'. This procedure of percentage 'on-costing' might have been reasonable, but as interpreted by the University Research Foundation the rate was quite unreasonable. All research on the Foundation's books was taken together but the on-cost was based on direct labour costs only. The proportion of total cost for 'hard science' research which was attributable to labour was typically a mere fraction of the proportion for labour in social sciences where in fact almost all the total cost was direct labour. Thus this method of calculation of the overheads resulted in the social sciences subsidising the 'hard' sciences. In addition to the bias towards the 'hard sciences' (i.e. apparatus dominated) in the costing of projects there was a levy which provided a pool of money to be disbursed as 'merit awards' (of which more anon).

Overheads so calculated resulted in a percentage considerably over 100% -- typical of accountants logic! It was difficult for me to justify this level of on-cost charges to funding agencies. It should have been obvious that the kinds of research which we and other social science departments carried out made few demands upon equipment, except computer time, and our travel was limited to a few states, usually within the east coast region. Physics, on the other hand, involved travel to Geneva (CERN) and Hawaii (for the telescope) and weather research included visits to polar stations and use of aircraft. I objected to my research funds being taxed at such a high rate, and since some projects were continuing from my time at Berkeley, I had no intention of asking the funding agencies for additional money. After some argument the Foundation permitted my existing arrangements to stand and proposed that I allow them to negotiate special rates in future. I had other ideas.

Accountancy madness.

In addition to what I regarded as the inequity of the costing provisions I had other grounds for dissatisfaction with the administration by the Research Foundation. It was inefficient even at doing the wrong thing! The accountants set up rules for the control of expenditure which might have been reasonable for a fish and chip shop management. As an example of the total lack of understanding of the politics and management of research, the control of travel costs exercised by the Foundation was ridiculous. It was required that project directors should apply for a travel warrant, giving reasons, if they needed to visit any location out-of-state and that they give at least 48 hours notice. For astronomers' trips to Hawaii or physicists' to Geneva, this may not have been unreasonable. But for my projects which were "out-of-state" (being mostly Federal funded) it would not work.

Criminological research has more public interest than the Cyclotron and High Energy Particle Accelerators! Unfortunately political problems which could seriously damage a research do not give 48 hours notice before they occur. There was, I recall, such a situation in my study of judicial sentencing discretion. Sentencing is a sensitive subject! The press can show an unhealthy interest in the actions of the courts and the "structuring of judicial discretion" was a politically hot topic. Not even within the criminal justice system could we expect unstinting support for this research.

One of the sites where we had permission to work with the judiciary in the trial of sentencing 'guidelines' (or structured discretion) was in Denver. It so happened that a Colorado Senator took exception to something in the project and began action which would have completely wrecked the project because we would have been refused access to data. Before the press began to run with this issue, it had to be dealt with. I took the first available flight to Denver because I had only inexperienced coding staff on location. It was several months and only after much paper work and the intervention of the University President that I was able to recover the cost of this journey! If a project director can convince the national government to make available several hundred thousand dollars on the basis of a proposed research, surely that director should be able to spend the funds as seems necessary for the success of the project? But, of course, the reason for the Foundation's wish to control travel was financial rather than scientific! Travel agencies grant 10% discount, which I could not claim when I bought tickets! The Foundation had its own travel agency. I did not inquire further into this.

Then a dramatic incident gave me the necessary clout to demand change. This was the incident when the University Account's Department wrote to Norval Morris (Head of the Chicago Law School) accusing him of attempting fraudulent double charging for his travel in relation to one of my projects. Moreover, since I was the director of the project they wrote as though on my authorisation. Fortunately Norval was not only a most distinguished scholar, but a personal friend. No permanent damage was done to our relationship when I explained and apologised on behalf of the University. Unfortunately Norval was only one of about five or six consultants (including two judges!) who were so accused on my assumed authority. This mess provided the

'dramatic incident' which added point to the general difficulties of operating within the restrictions of the Foundation.

Avoiding the Unhelpful and the Obstructive.

I responded by placing the funding for whatever projects I could beyond the range of the University's control. This was possible if the project was not funded to the University but to the National Council on Crime and Delinquency or, perhaps, some other external charitable research body. It so happened that the Headquarters of the National Council on Crime and Delinquency (N.C.C.D.) was located in New York. It will be recalled that I had worked previously with the N.C.C.D. Director of Research (Don Gottfredson) and though he remained located in California, our co-operation again seemed possible. Don's research status was equal to mine in the eyes of the funding agencies and, of course, we had worked on projects conjointly previously in California. We could design a project together, then he could apply for funds and the NCCD could employ me as a consultant. So for my research I was not a university employee. It was also possible for the NCCD to employ graduate students, and to pay them reasonable sums for their work rather than the low rates set by the University Foundation which were tied to bursary levels. The accounting would then be in accord with the rules of the Council, not of the University. The government agencies which provided funds for the research were prepared to accept the NCCD's accounting and auditing. This arrangement was possible and legal because the N.C.C.D. had the necessary machinery and the overheads were realistic. So Don and I operated as a partnership on several projects, but it was a series of studies of decision-making which were to become of major significance in both managerial and research originality. At the start of this series of projects the University Administration had no more control than to see that I did not spend more than my permitted time on external consulting.

The objection to my arrangement with Don was muted because we were engaged in collaborative research and I could claim that my role was secondary and that I was not the originator of the projects. (That is, at least, what the paper work showed!). In fact it was always difficult to know who originated the projects because Don and I were jointly involved from, or indeed, before the start. For accounting purposes I was a consultant to the N.C.C.D. and entitled to be compensated at the approved rates. The time allowed for consultation was reasonable. Academic lawyers were almost as addicted to external consulting as were the medicals (!) and Criminal Justice was a law-related field.

It was unfortunate that this facility was not available to my colleagues. They lacked external links with whom they could work and since they were sole proposers of projects the collaborative argument was not open to them. They had to place their research through the university's Research Foundation and add the excessive on-costs to their research budgets. I began to explore ways whereby the advantage I had through the good offices of the N.C.C.D. might be extended to my colleagues in the School.

The Albany Research Center is conceived. [19]

My colleagues agreed with me that we had to find some way whereby we could control our own research funding. The problem was, of course, that projects would have to be submitted in the name of a charity and not in the name of the originator of the project. Only collaborative research involving at least one bona fide applicant who was a member of the research staff of the external organisation would be likely to be valid.

Most members of faculty who attracted research support wanted to direct their own projects and not to share them with others, particularly others employed by an external organisation. Then someone had an idea (unfortunately I cannot give the credit to any individual, and it is possible that it was sui generis). Why should the faculty not set up its own educational charity and resolve the problem of collaboration with 'externals' by themselves being the Trustees of the educational charity? We could then employ our own accountants and lawyers and ensure that our administrators were not going to dictate policy or procedures of which we did not approve. Thus we could handle the submission of proposals, and administer the funds (when granted!) to the satisfaction of statutory auditors without involving the university administration in any way.

The University Research Foundation was not pleased with my arrangements with N.C.C.D and any attempt to extend this kind of operation would be obstructed. But several of the faculty were interested to explore this approach; a meeting was arranged and most attended. By unanimous vote it was decided to set up a Charitable Trust of our own to operate in a similar way to the N.C.C.D. and to be known as the Albany Criminal Justice Research Center. We were fortunate in that in addition to the Dean, the Associate Dean was also a lawyer, moreover the latter had previously worked in the state offices and knew the necessary procedures. We calculated that we could pay reasonable wages to research students, rent our own building and purchase our own computer equipment and charge about 30% oncost and still have some surplus which we could use to support impoverished students. Needless to say those private foundations and government departments who had supported our research were delighted. So a proportion of the faculty became Trustees of the Albany Criminal Justice Research Center (later renamed as the Hindelang Research Center). The University Administration and most of the other departments engaged in research were less delighted. A 'war' resulted.

The Trustees of the new Albany Criminal Justice Research Center obtained a suitable building within a mile or so of the University (on a 'bus route). They engaged legal and accounting staff. The Center was soundly and legally well established and most of the Trustees were members of the School. The University could not attack the Center in terms of its structure, so they tried to make it impossible for the Trustees to function. It was suggested that being a Trustee of the Center was not compatible with the

19 It is interesting to note that at the end of 1997 this Research Center had 21 graduates and 8 staff conducting 13 different research projects.

academic duties of the faculty and School. This was clearly absurd because it provided facilities for graduates to gain experience. Moreover the medical departments of universities conventionally operated in similar ways with their associated hospitals.

Almost from its inception, the Center was receiving very considerable funding from various foundations and the Department of Justice. It would be foolish to kill a goose which was laying golden eggs. The Center was also gaining credit in the field at an international level because authors were noting its function in publications.

Conflict over credits

There was a state of 'cold war' for some time while the Albany Criminal Justice Research Center expanded. Then, an attempted coup! the University tried to use the State auditors in an attempt to pull off a 'take-over' and force all research funding to be placed through the University Research Foundation. On this occasion the university nearly won. The auditors were despatched to examine our accounts. It is probable that they might have found some points which were problematic from the N.Y. State regulation perspective and they might have acted quickly to close the office. But most of our research projects were funded by the national government in Washington and we were responsible to them, not the State of New York. Fortunately the New York State auditors went first to the wrong address! A few days earlier the Research Center had moved to larger premises some way out of town. However, the cleaners who were clearing up the old offices were smart enough to telephone to the new location and give us notice that the 'spies' were on their way! This gave us just sufficient time to get in touch with Washington and obtain authority to refuse to disclose our accounts to an 'unauthorised' inquirer. We had one other safeguard in that part of the research team were at the time working in Washington with the Department of Justice.

Pressures mounted and we were forced into negotiation in relation to the balance of interest between the University and the Research Center. We agreed that teaching staff should have strictly bounded commitments in, and limited income from, their activities at the Center. By this means we achieved, for the time being, a 'hands off' understanding with the Administration.

But that was not the end. There were many developments. We were, of course. popular with our own students because they could get financial support while they worked their way through their degree courses by being employed in research directed by faculty members and administered by our Research Center. The research environment was more congenial than most money-raising ventures available to students. When qualified they could claim that they had actual experience in a research organisation. We continued to pay 'trades' union' rates (as I had been able to do when the projects were funded to the NCCD) and not the allowances usually available to research students on bursaries.

The Administration were clearly embarrassed by the fact that a large part of "their territory" was outside their control, and more particularly, was operating with

overheads which were a mere fraction of those they exacted. Then we began to have difficulties because of our success. We were approached by other departments of the University which did not make heavy demands upon equipment (psychology and sociology) asking to be permitted to use the good offices of the Center. This was stopped before it was started.

I do not know how the Foundation was able to do this, but I was told that a memorandum was presented to all departments by the Administration which made it imperative for other departments to use the Research Foundation of the University; the use of non-university facilities was barred.

Democracy as the enemy of Meritocracy

But it was not a straightforward fight. There was no clear 'us' v. 'them', because linked with the overheads issue were two other concerns, namely, the merit awards and the status of departments. Together with the School, the Mathematics department and Earth Sciences were classified as "star" departments, and with this rating came some privileges. The grading of departments and award of the 'star' category was carried out by an independent team of assessors comprising academics from other (out-of-state) universities. (Coincidentally Norval Morris -- remember him -- was a member of the assessment panel for Albany!). The 'star' grading provided an indication of peer group evaluation for the guidance of research funding agencies and was usually thought to influence the allocation of merit awards (special salary negotiations) and one-time payments. There was ongoing argument as to the desirability or ethics of this (so called) 'elitism'.

The merit awards were, at least in part, supported by the overheads charged by the University Research Foundation. While the Research Center remained independent of the University Administration it was not contributing to this 'pool', but had its own charitable fund for fundamental research or research bursaries. It was argued that our Research Center was 'robbing' the University of its allocation and we had to admit that there was less in the 'merit award' kitty than might be if we had been included. For this reason the School (with the Center) was somewhat unpopular with other departments, and especially those who might have benefited by sharing in the spoils.

Furthermore this was the time of 'radical' politics in universities in the United States, and the issue of 'power' was much debated. While this is not the place for developing a philosophical argument, I will recall the related events and my reactions without moralising. Certainly at Albany, at this time, there were specific motions set before the Senate aimed to 'reduce elitism' and these (naturally!) were carried by a large majority. There were more votes for democratic levelling than for the 'star' system because the most highly qualified were not the majority! Indeed all matters referred to a vote of the Faculty Senate were similarly settled to the advantage of the majority who certainly were not the 'elite'. Perhaps that was desirable? The rights of minorities within a democracy are difficult to safeguard: but it is hard to distinguish the simple majority form of democracy from a tyranny of the majority.

I have considerable doubts as to whether a voting procedure which gives equal weight to the votes of all academic grades is in the best interests of the corporate body of the university. I do not remember whether the voting equality included teaching assistants, but there certainly was a requirement to consult with students.

A Conflict of interests

My interest in this issue was linked with the Research Center on the one hand and as Chairman of the Faculty, with the granting of 'merit awards', on the other. The impact of the democratising of governance influenced (almost determined) the allocation of merit awards by demanding the distribution across all faculty. The base of our unpopularity as non-contributors to the funds was now increased. Initially only a few were allocated merit awards, but now the whole community were beneficiaries of the distribution of the 'surplus' and hence received a reduced sum because our research made no contribution to the general kitty. It is not difficult to see that our immunity was not popular! Some claimed that the position was worse; it was unethical -- say undemocratic, and probably illegal.

The University Foundation gained credibility from our opting out of the general fund and using our surplus to grant scholarships within our own School. There was no objection from our students, but we were under pressure to become more integrated with the whole academic community. The Research Center was, by this argument subject to attack from all sides.

Direct attack on the legal grounds had proved impossible because our charitable status was well founded. If we were unwilling to relinquish our independence voluntarily the Administration would find other means. So tactics moved to a form of harassment.

There was a gradual erosion of the independence of the Center. It was represented that if faculty were concerned with the Center they were not "full-time" members of the University staff. This assertion had to be treated with care because there were one or two members of the faculty who were certainly increasing their income quite considerably from consultation fees through their base at the Center. The next move (or harassment!) was to forbid us to pay doctoral candidates who worked at the Center more than the rates paid by the University Research Foundation. Thus our students ceased to be privileged.

A change of President and 'financial stringency' resulted in the University working on another plan to take over the Center and make it part of their own Research Foundation. Various compromises and transition states went by and at the time I retired, the control had moved to the university. But then the President was also a member of the Faculty of the School -- so the conflict ceased by the prerogative of power and the means of merger! From origin to osmosis had taken about ten years!

The Deficiencies of Central Management.

I would still find it difficult to accept the argument that moneys generated by a few should be freely disbursed among the many as a necessary feature of either democracy or of university governance. I would argue that since research projects are not funded at random, the 'surplus' funds they might generate should not be distributed randomly. The system which was railroaded through the Senate could only ensure the triumph of mediocrity. I would see no ethical objection to the procedures which distributed funds which became surplus to research activity among those who played some part in generating them and to those others, such as colleagues in the same discipline, who might have facilitated the work. (The on-cost charged included fees for use of 'common services' such as libraries and computation). I could also agree that it is reasonable to recognise 'merit' in any department or indeed in the whole organisation. In the latter case the argument of equality of opportunity for all employees could be sustained since cooks, typists and accountants as well as teaching staff are capable of meritorious service. But to distribute the surplus among only vote-qualified faculty is not 'democratic', it's just plain selfish and provides no incentive to the support of research.

I recognise that I am legitimating my two roles in these two arguments: as Chairman of the Faculty I had an interest in merit awards and I was also a member of the Board of the Research Center as well as running my own research through that body. But, of course, I do not think that this makes my logic any the less acceptable!

It was, of course, not possible for the faculty of the School to set up their own research charitable organisation until there were available research projects and moneys. Several professors had research in progress when I arrived with my NCCD administered projects; theirs were handled by the University Research Foundation. It was only when new money became available from a sufficient number of projects and was large enough in total to sustain an income to meet the costs of a separate administrative structure, that we could risk the purchase of a property and 'set out our store'.

Clearly the organisation and financing of research has an impact on certain aspects of research activity, but within any set of managerial or accountancy constraints it is possible to have different styles of research organisation. Moreover I think that the important components which result in the failure or success of research projects are primarily determined by the quality of the basic ideas. Unfortunately ideas are infertile in adverse financial or political climates. The idea of research into judicial decision-making which was the basis of a series of interrelated research projects, had three different 'homes'. It was, first with the NCCD Research Center, then with my move to Albany, the University Research Foundation laid claim to the phase current at the time, with the disastrous results I have noted earlier. It was, then, this project which was one of the major factors leading to the setting up of our Criminal Justice Research Center under faculty management.

A Natural History of Decision-making.

Research orientated readers will probably have found the previous section boring and want to know something about the actual research ideas in the decision-making projects and how the projects themselves were directed. In this particular series of projects, in a rather strange way, the organisational aspects provided not only the structure within which the research could be carried out, but also made a major contribution to the origin of the ideas underlying the research. It may sound like chaos: it often seemed chaotic at the time. Now it is possible to take a more detached view.

There are many publications covering the different phases of this work, and still more arguing the merits of its results, but none have told of how it began nor of many of its important personnel managerial features. There were some events which caused 'waves' in the U.S. Senate and even in the White House! These background dramas were not told in the research write-ups. It is important to note that around the time of the application of some of the research findings, the "Just Deserts" movement arrived on the scene and became confounded with the prediction and 'seriousness' features of the decision approach. I shall not repeat here any of the actual research findings, the consequences of which were far more extensive than I can ever know. Volumes have been written on and around the 'deserts' and 'guidelines' concepts.

In the beginning was the LEAA and the NCCD

There is little doubt that the series of judicial decision-making projects became one of the major operational research activities in the field of criminal justice at that time. But the idea did not emerge from a vacuum, nor by contemplation or introspection. Rather the problems came to be specified in the course of interactions with the operational agencies. The climate in which this kind of research could flourish (as it certainly did) needs some elaboration if the story is to be clear. I will be brief, but I must provide some notes on the background, both in particular and general.

At the general level, would-be research workers in the United States had (indeed still have) many sources for funds. The tax laws have favoured the establishment of charitable organisations, and many have multimillion dollar capital resources (e.g.: Ford, American and Melon Foundations). Also several governmental agencies provide support for social, medical and legal research. The Department of Justice (like the Home Office) has its own research units and sets research topic priorities and sends out 'requests for proposals' (FPS). At the level of the particular background, the Law Enforcement Assistance Administration was, at this time, a new official research funding agency of the Department of Justice. A major sector of this organisation was then headed by John Conrad who had been in Sacramento, in the Department of Corrections during my first visit to California and had been a working colleague of Don Gottfredson. The concept of law enforcement was widely interpreted and the organisation was later renamed as the National Institute of Justice. Presidential Commissions had provided a climate in which research was in demand. Professional bodies had to take research seriously. Research was a valued activity!

It is not possible to start the decision studies story at its absolute beginning, because like all research its antecedents attenuate into past decades. The work rested on two 'legs' -- the methodological and the operational needs. In some major respects I think I may say that in this case the primary move was methodological. Showing that a kind of problem might be studied by research methods (Boolean intercept information search) led to the research being requested -- though, in the first place, probably not for the most logical of reasons! The 'decision research' series depended upon a complex of situations and a number of chance events. Though the beginnings of this work were not distinguished from most other projects in their involvement with political figures, they were destined to attract far too much drama and political involvement for my liking. I do not know what happened behind closed doors in such places as the White House!

The Appeal of Computers.

During my time at Berkeley, it may be remembered that Don Gottfredson was the director of research at the National Council on Crime and Delinquency's Research Office, then located nearby at Davis. I have told of how the N.C.C.D. and the Berkeley School had co-operated in organising a meeting of 'directors of research' at Sausalito. The American Correctional Association became linked with the Research Directors' Annual Conferences which grew out of the initial meeting in Sausalito. Don and I continued our association in one project or another when I moved to Albany. (He later moved to Rutgers in Newark). Our association was mutually beneficial. I have now 'set the stage' and named my main cast of actors! Now for the action story! It transpired that an annual conference of the American Correctional Association was scheduled to take place in Boston Mass. John Conrad, who had been at Sacramento, was then administrator of the Law Enforcement Assistance Agency. He had been asked by the administrators of the American Correctional Association to arrange and chair a meeting on research topics at this forthcoming annual ACA conference. John asked Don if he could prepare a paper on any project or ideas of interest to him for presentation at the ACA Research Section meetings. Don called me with the proposition that we might do a joint act (the Mutt and Jeff thing for which we were known). Don had obtained some excellent data on parole cases from the Federal Parole Board and was interested to analyse these. I was interested in the development of computer-based information systems. Don had the data, I had some know-how and an interest in computers, and Conrad had need of a paper: things took off from there.

An aside of explanation about computers may assist because it will be difficult for readers today to appreciate the meaning of the term "computer" as it applied at the time I moved to Albany. The speed of development in the technology means that the word "computer" used in 1969 had to be given a completely different operational definition with each generation -- where a generation was around three years. The university had a Univac housed in the basement of the main building; it occupied several hundred square feet. Today I have more power in the machine I am now using while sitting in an armchair in the lounge than was available in that basement. The inversion of a 10 x 10 matrix was about the limit of its capacity! At that time the future was seen in mainframes with work-stations. The PC, even if it had been thought of, was not

considered to be viable and even when they emerged they were initially scorned by mainframe enthusiasts. (Anybody who worked on mainframes was an enthusiast).

I do not know (or do not remember) how the next steps came to be taken. I had read about the development of a data-base and search programme (DIALOG) that Lockheed had set up. This seemed just what was needed to analyse Don's data and there might be a more general use for it in the Parole Board or other governmental departments.

A miracle with a modem!

It struck either Don or I that if Lockheed would cooperate with us, the analysis of his data would make a good presentation and demonstration at Conrad's meeting. Could we persuade Lockheed that the publicity value of such a demonstration would be of benefit to them? The deal we had in mind was that Lockheed should turn Don's data into a usable data base and then Don and I with the Lockheed technicians could put on a show at the Boston meeting. We represented that the A.S.C. through the good offices of John Conrad would provide a platform for a demonstration to a highly specialised audience. This would represent an advertising opportunity which our contacts (Lockheed's technical people) might be able to get their public relations people to pay for. Everybody would be happy! John would have an interesting presentation at his Section, Don would get his data coded and accessible, I would have an opportunity of trying out some analytical methods and Lockheed would get the valuable 'exposure'. The deal was struck. The 'stage' was set, the first act could begin --- but there was no plot. So I will not continue with that analogue.

I will just say that it was a risk-accepting venture to attempt such a presentation. The data base could only be housed and operated upon by the computer located in California. The connection was primitive -- a sonic pad placed under the hotel telephone in the Boston hotel. But it worked. The audience was certainly 'high level'. John, of course, was powerful in that he was most influential in funding research and had a large budget. Also present was the Chairman of the Federal Parole Board. He was encouraged to operate the system and to 'ask questions' of the outcomes of parole decisions using DIALOG's Boolean intercept programme.

The Chairmanship of the Federal Board was a Presidential appointment. Parole has since been devalued but at that time the Chairman's status placed him with the senior judiciary. His name was George Reed. George was interested in the proceedings for two reasons. The Parole Board's decisions had been subjected to serious criticism by a some scholars (including Kenneth Culp Davis) and by the Washington Post and as though criticism from these two sectors was not enough, computers among Washington bureau chiefs had become tokens of prestige, and the F.B.I. had just been allocated one. Why should J. Edgar Hoover be favoured?.

It is recognised by bureau chiefs in D.C. that as a last resort in time of trouble one can always turn to research and hope to divert some adverse criticism by saying that you are doing so. It is a way of admitting the problem without loss of face! I fear that politics

and status factors outweighed the scientific in driving the interest shown. The idea of "computer assisted decisions" certainly seemed to have an appeal! The presentation gave Chairman George Reed sufficiently good arguments to approach John Conrad requesting support from L.E.A.A. funds to provide a computer for the Board.

Collaborative Decision Research

Conrad agreed to fund a computer for the Board, but only as part of a larger deal. Its use would have to be evaluated. To this end it was necessary for the Board to permit research to be carried out without constraint. Who should do the research? Who else? It was not long before Don and I had prepared a research package -- "Study of Parole Decision Making". We asked only for six month's funding in the first instance, but it was understood that if we reported that all was going well at the end of the first quarter, the Board could retain the computer and plans for a major project would be submitted with the expectation of extended support. It was also agreed that both parties, the Board and the research team had equal rights to call for the cancellation of the project if at any time it became clear that it would not succeed. Success was defined as the satisfaction of the parties involved. I think it was made quite explicit that we would do research with the Board, but not for, on, or to them. It had to be a joint project between equals, namely the research team and the Board. This concept may not, even then, have been new, but it was rarely applied; usually the research teams were consultants and saw their role as advising their clients, rather than being partners in operations and policy.

Bonding session: getting to know you ...

George Reed did much more than accept these conditions, he developed the idea in his own way. At the first meeting to discuss procedures he remarked that Parole Board members and social research people tend to think differently and use a different 'language'. Something had to be done to facilitate communication and understanding before we began working together. He then proposed what today, in modern management jargon, would be called a 'bonding session'. The research staff and members of the Board would take an 'unstructured' weekend in a resort hotel and 'get to know each other as persons'. It was a great weekend in a Vermont country hostelry. How he got this out of his budget I cannot imagine. Later I came to suspect that he may have paid for it himself, but it was impossible to ask. It did not come from the research budget. It is, of course, not possible to prove that this bonding session facilitated the research; on the other hand it may have been the main ingredient in its success. My subjective assessment is that it was such a major ingredient; and more than just a sweetener.

Formalising relationships

The next move was to establish a "Steering Committee" for the project. This body should be advisory and independent of both parties. The composition of this group was extremely important, and it was certainly original. The Board had two representatives, the research team two, two places were allocated to chairmen of state parole boards

who would be observers and there would be an independent chairman and a technical consultant. The idea of inviting observers was to save time should the project commend itself to other parole boards -- the initial problems of communication might be served if the chairmen were involved. In fact these representatives proved valuable in "keeping the Federal Board honest". For example if the Board should resist providing some data on the grounds of time, their colleagues might challenge or support their claim. Indeed this happened on at least one occasion when the Federal representative claimed that it would take too much time to provide an item of data; the state chairmen members called their bluff. No one else could have done this.

The external consultant was proposed by analogy with judicial practice. If one side has a lawyer the other side will also! The research team had statistical know-how but the Board did not. The Board were not happy with leaving this expertise as it were on one side of the table: they wanted their own statistician. Of course we had no objection. In fact the Board secured the services of Professor Herbert Solomon who was head of the department at Stanford. Don and I were pleased to have such a heavy weight on board and the Board felt protected from being misled by statistical procedures they did not understand. It seemed reasonable to us to propose that he be asked to be also the group's chairman. It was a great group!

Solving the wrong problems the wrong way.

For our first stage of research with the Board we accepted their definitions of the tasks. We did not know at that time that the specification was not correct -- that it would not map on to the tasks as actually performed. In fact we adopted two basically incorrect assumptions which led to inappropriate methods being deployed. However, the methods provided the means for revealing the errors and progress was made.

I am faced now with a problem of how to tell the rest of the story. As the research gradually worked its way towards better specification of the problem there were many false moves and no small amount of confusion! That is the way research is. If the story follows the research line over time, the chaos will be reflected in the story-line and the reader will become as confused as were the research team prior to the two breakthroughs. This is why research reports give a totally incorrect image of the procedures of research! The presentation of the findings and methods has to be clear. The clarity is supplied retrospectively -- the report is written at the end. I will try to preserve something of the sequence by concentrating my report on the nodes in the network of ideas as the work progressed.

Information Overload.

Decision makers in all fields tend to take the view that their problems would be solved if they only had more and better information. It is not easy to convince them that this is not so, and it is unwise to begin by challenging those with whom one must work collaboratively. The Board took this common view. They thought that the problem of

disparity would be solved by faster provision of more information, so we set up a scheme along these lines.

I have not yet met a single decision-maker who, intuitively, accepts research findings as to how soon information overload may erode the quality of decisions. But this is a research topic which I have dealt with in booklets and articles in plenty. There is no point in repeating the findings here. It seems that decision makers have to experience for themselves the problem of information overload and for this to relate specifically to the kinds of data and kinds of decisions they are personally concerned with. Other decision makers experiences are not believed to apply.

We devised various laboratory-type experiments in information search and decision making so that Board members became aware of the overload problems. Their results surprised them! This laboratory type research was carried out at the same time as the Board's field trials of computerised data. It is weird now to record that for our specific research projects on information retrieval we had to simulate the computer of the (then) future by the use of a random-access slide projector! The data available to the Board were often not as clear as they might have been and often certain specific facts were not recorded. It should be noted that Board members, like members of the Appeals Courts, worked only with case papers. The Board generally did not interview applicants for parole themselves. The case papers included a very significant report prepared by probation officers who had carried out interviews and discussed the case with the prison governors and other staff. There were difficulties with missing information. This might have been due to the fact that Hearing Representatives or other officials who recorded information did not regard certain items as of importance or because of oversight or error. In research terms, the data-base was incomplete, and both 'noisy' and redundant.

My prior research into decision-making by probation officers had led me to believe that the information search strategy adopted by probation officers was a major feature in their determinations. I had also verified earlier decision research findings which had shown that information overload was not given sufficient attention. However we began by eliciting from the Board their views as to the items of information they believed they used in granting or refusing parole. They provided a list of over 100 items. We knew that this was unrealistic but we had to design means whereby the Board came to realise this for themselves. This work took place as a series of small, specific studies while the main work of setting up the computer data base was under way in Washington.

The initial plan was to use the computer to store information on all decisions made by the Board and to build up an 'experience' data base to which the decision-maker could refer to see how prior cases similar to the case in hand had been decided. It was a way of making quickly accessible relevant precedents, a procedure commending itself to lawyers and administrators. The programme was such that any question beginning with the words "How many?" could be answered almost instantly. Such as "How many murder cases have been paroled?" "How long were they detained?", "How many of these reoffended?", "In how many cases were the victims members of the accused's

family?" and so on. Any particular case could also be called up as a pattern. The Chairman liked to refer to this as finding out what was 'par for the course'.

The case papers for current and prior cases decided by the Board were not on computer file nor were they 'computer ready'. If we were to test the procedure within six months (the pilot study's funding limit) we had to use Don's data. This was the same material as had been used in the demonstration in the Boston hotel. Though these data were not a sample of the true population of "Board decisions" which we were to need, the processes could be tested because Don's data were not significantly different as to outcome and offence profiles. The Board agreed, for test purposes, to use this data base as though it were derived from case histories of their own prior decisions.

Thus we had three kinds of projects running at the same time, namely the small diverse experimental lab-type studies, the pilot project with the Board and the routine of establishing a data base on Federal Parole decisions.

The small pilot using Don's data provided the first report on the basis of which it was agreed that the work should continue. It seemed that the Board could see how things would develop. (In fact no party involved could do this!). However, the funds were made available because the Board agreed with us that we could proclaim the study to be successful. The same terms for the grant were to continue for the major project.

There were several 'balls in the air' at the same time, but the largest was the data coding and transfer operation of the Board's cases. This required a large contingent of keyboard operatives to be located in Washington and these staff had to have security clearance for access to confidential information. The coding team in Washington was supervised by my chief research assistant, Peter Hoffman. Peter was awarded one of the first three doctorates to be given by the School.. It took me longer than it should have done to appreciate Pete's qualities. Pete's parents had just died and I thought he needed a challenge as therapy. The Parole Decision Project and a post in Washington would keep his mind engaged. I put him in charge of the Washington base (the same as noted in relation to the attempted Research Foundation coup!). It was to prove very significant that during this time Pete was in close contact with members of the Board. He certainly had the right image. He knew "what it was like out there". Pete had been a parole officer in the Bronx (New York City). Moreover he was a champion shot with a .45, as well as being our third doctoral graduate. He had become well-known to Board members and staff and played tennis with one or more!

When we moved to the 'production' full-scale operation with the actual real time data, things did not quite work out as planned. We had expected Board members to operate the simple data selection procedures directly. In fact after considerable effort and instruction, the members decided that they were not cut out for a 'hands on' experience. They wanted an intermediary who spoke their language and who could then 'talk' to the computer and report back. Of course, we had just the person. He was Peter Hoffman.

The scheme should then have been operational. But is was soon obvious that it was not being used in the way it was intended. The mere provision of a computer with rapid access to data and various automated analytical procedures was not satisfactory even with Peter's 'interpretation'. The fundamental issue of disparity still remained. We were being driven by the circumstances towards a decision theoretic approach, though we did not then realise this. I was concerned with the problem of information overload, and as I saw the task at that time it was one of finding the 'powerful' items of information which decision-makers should take into account. Linked with this was the idea of providing a 'prediction table' because the risk of recidivism was thought to be a major element in granting parole.

Disparity.

It was the accusations of disparity in Board decisions which justified the project in the first place. We needed to do some fundamental thinking. How could computerising, or prediction equations help to reduce the problem of disparity? What, in any case, was meant by 'disparity'. It had become clear that whatever disparity was, it was not going to be reduced by providing more information, more quickly nor even more accurately. Could 'disparity' be given a statistical definition? We were getting into deep water. We did not realise how deep -- indeed the tide still sweeps on! But then we were more or less alone in this field.

Disparity had to become our main focus -- the criterion. This led to discovery that the decision task of the Board had been inappropriately specified.

A Reassessment Begins.

It seemed desirable to discuss with Pete how he saw the problem from his close association with the Board. Were they content with his role? Did he think a development along the line then being followed would work out eventually to reduce disparity? So Pete came up to Albany to talk about 'progress' with me. The date is unknown but one document remained as a record of the meeting.

Though Pete was working collaboratively with the Board, I was not happy, in principle, with this essentially subjective individualised approach to decisions. It seemed that it would be sounder practice if we could deduce the principles (dimensions) upon which the decisions were based. We were told that there were two major concerns: the seriousness of the offence and the likelihood of recidivism. The Board had heard of 'prediction' and were not averse to its consideration. There was, however, the question of whether a generalised prediction equation was of more use to the decision makers than the data they could obtain via DIALOG, where they could examine patterns of prior similarities according to their choice.

I remembered my studies of the setting of 'establishments' for police divisions and the equations used for the Carlisle inquiry. These had worked well for the Metropolitan Police and the Home Office. We could use a similar method to look at the Board's

decisions. I was willing to begin with the assumption that the decisions made by the Board were, on the whole, reasonable. The first step would be to see if we could find a pattern of data items and weights which correlated with their subjective determinations. I had every confidence that we could. We could then move to the next step and use a similar approach to that of investigative quality control. We would first examine the variance while making no judgement as to the appropriateness of the mean. I began to try various models looking for equations which would fit the decision patterns observed. If a pattern of information use (vector) proved predictive of the decision then the variation in determinations which was the cause of criticism could be examined and perhaps explained. So I started to work up a similar model. In the Carlisle case I had been seeking to find a fit to the expected profit which I then compared with the observed profits. Those inns which did not fit the equation ('outliers') were suspect. A similar approach had been useful in finding fraudulent interviewers. There the assumption was that most of the innkeepers and interviewers were (sufficiently) honest for me to use data based on their behaviour to identify those who were very dishonest. I had to be careful at this point to avoid any suggestion that I was looking for aberrant Board members! It was 'outlier decisions' which we were to examine, not 'decision makers'. Variance between decision makers was to be unidentified variance. (But that was no reason why our methodology should not aim to reduce it)

There was no suggestion that such an equation would explain the decision maker's logic, but only that if I were to use the equation to guess the outcome of decisions, I would get a fair number right -- far more than by chance (or extra sensory perception!).

Continuous or dichotomous model?

As we began to work with these concepts it became clear that we had been persuaded to research the wrong problem! (This kind of discovery should not be as rare as it probably is!). We were fortunate that this was not a disabling matter. We were not bound by any prior specifications to carry out any particular form of analysis. Too often the conditions of research funding (e.g. contracts, rather than grants) inhibit this first important step towards a useful outcome.

The Federal Parole Board's decision task was specified as the hearing of petitions by prisoners who were eligible for release on parole supervision to complete the remainder of their sentence in the community. This seemed to suggest that we should model the problem as a dichotomy: to grant or to refuse the petition. This was indeed the legislature's charge to the Board. However, it was not until a rather late stage that we took note of the fact that the decision was not so simple. If the petition was refused at first hearing, it might be granted at the second or third. It was sometimes said when refusing a first application that "He's not done enough time", or the decision was 'set back' for a later hearing. Thus we might assume that the criterion was not dichotomous, but one regarding the fixing of time in incarceration.

Rational, but illegal decisions by Board.

Setting time-to-be-served is de facto setting the length of sentence. This was not what the Board was authorised to do; the sentence was the judge's prerogative. If the Board were 'fixing time' they were exceeding their powers. But perhaps it only looked like that? So we decided to test the model before raising the legal/philosophical point. The result was clear -- the model using the 'time setting' continuous criterion fitted the decisions of the Board far better than a petition model using dichotomous criterion.

With this specification we had two problems, one involving legal and policy issues, and the other research procedures. But, in support of these embarrassing situations there was nothing more than a few equations. In addition to the political/judicial problem of the Board's powers, we also had a technical and procedural research problem of the data base. Intuitively one would not think that it would be the research difficulty which would prove to be the most traumatic.

When we thought we had been working appropriately with a dichotomous petition model, we did not need data on offenders who 'did time' without making a petition. Obviously, if we were concerned with granting or refusing petitions, these cases were not in the sampled population. There were two classes in this category. A few offenders opted to serve their full time rather than apply for parole because of the conditions entailed and there were cases where the Board virtually decided not to decide. A decision not to hear a petition was a 'decision' and should have been within our sampling frame.

Ethics v. politics: a threat to the research.

It was my view that ethics demanded that we face the Board with our findings as soon as possible. The consequences might be the termination of the project if they should decide that the result was too embarrassing to acknowledge and prefer that it remained obscured in the complexities of procedures as heretofore. In addition we had to obtain the missing case materials.

It will be remembered that we had agreed with Conrad that we would terminate our work if at any time and for any reason its integrity could not be sustained. What could we tell the Board? We had to face the possibility that we might have to declare the project closed, discharge our dozen or so research assistants and make peace with the Board as best we could. I was not willing to accept compromise and produce an unsound product. We had to face the facts. Hastily I called a meeting of the research personnel involved; or as many as could be contacted at extremely short notice. I outlined the position. They were naturally disturbed to learn that there was a chance that they might be 'on the bread line' but agreed that the only way was the ethical way and for me to get in touch with the Board as soon as possible and explain the position.

The Mutt and Jeff routine is helpful.

The fact that Don and I worked together certainly helped to smooth the way. My relationship with the Board was 'professional' whereas Don's was more towards being friendly. Pete Hoffman also had personal qualities which appealed to the 'realism' of members. Our united front was convincing. The Chairman accepted that they were concerned with setting an appropriate time in relation to the seriousness of the crime rather than simply with granting or refusing petitions.

It seemed surprising that the replacement of 'time served' as the criterion (instead of 'granting or refusing a petition') was accepted not only without objection, but with some enthusiasm. However, paroling decisions, looked at this way, were probably seen to be more complex and hence to reflect a higher status than a dichotomous determination on the 'facts'. Perhaps the ready acceptance also owed something to the fact that the equations which provided the 'fit' with 'time served' had very good face validity. But I would stress that this fact did not mean that we had identified any 'mental process' of decision-making or explained anything. It merely meant that we could replicate the Boards' decisions quite closely, and quite adequately for our purposes. Of course, we were fortunate that, unlike in the case of the drink trade profit equations of earlier years, no 'suppressor' variables were involved. The equations were not only useful but they also seemed reasonable to the layman. The items of information used in our equations to 'substitute for' the Board's decisions could be reduced to three principles (scales). The main variable was the seriousness of the crime, the second was the probability of recidivism (mainly prior record) and a small added weight could be attributable to performance in prison.

On average, over all Board members, the equations we had derived enabled us to reproduce the decisions with 80% of cases giving 'time' to within 3 months of the actual time served. We had, then, a useful basis for discussion as well as for further statistical work. Ignoring the outliers at this stage we presented the findings. The confidence of the Board may have been due to the wrong reasons! But we were grateful for it. The equations (fortunately) looked reasonable and the logic (if not the statistics which quantified these vectors) appealed to the Chairman. Even the weights (the numbers) looked sensible! This was interesting research material, but what did it mean for the operations of the Board? This, it must be admitted was, at this time, not at all clear!

The equations provided what seemed rather like an independent 'second opinion' and this encouraged the Chairman to reformulate the Board's function. It need not serve as the nominal decision maker of the first instance. Instead it could openly function as a reviewing body of determinations initiated by Hearing Representatives. The recommendations received could be assessed in the light of the equations which Chairman Reed named "Guidelines". But it was not all that straightforward.

Drama as power politics get involved.

While the acceptance of the basic concept of time-to-be-served made the further development possible, there remained one major difficulty. The 'population sampled' had been restricted to petitioners -- now we had to include all whose period of incarceration was determined by the Board. We had to explain that the 'time setting' criterion meant that we needed the additional cases. Then we received extremely bad news. The data base within the control of the Board was limited to petitioners. It did not include cases which the Board decided not to see, nor those who did not apply for parole. Data in respect of these additional cases was held by the Federal Bureau of Investigation. Head of the F.B.I. was Edgar Hoover whose reputation was not then sullied -- he was, in the public image, Mr Righteousness himself. He was very antagonistic to all forms of social research and the likelihood of our obtaining the additional data looked remote indeed. It was clear to me that we had to have these data or we could not sustain the integrity of the research. We reminded the Board of the conditions of the grant, namely that if we could not carry out our work with the rigour our standards demanded, we had to advise Conrad and close the project.

Chairman Reed asked whether we would hold over advising the funding agency until he had a chance to see whether he could persuade Hoover to release the information. He said, much to the surprise of all, that he thought that this might be possible. He needed two weeks delay. We agreed. We received the data. I cannot imagine that this was a simple matter for the Chairman to organise. Certainly the official record is clear that Chairman Reed received a letter from Edgar Hoover to the effect that he had misunderstood the nature of the request and the FBI would be pleased to co-operate (Pete has told me that he saw this letter). I cannot think that this did not involve some kind of “deal”. However, no matter what the explanation, release of the data was a change of FBI policy (or a most notable exception). The credit must be given to Chairman Reed: he saved the project.

I understand that in his autobiography George Reed makes no mention of this incident. If the story was ever to be told, it was for him to decide whether to tell it -- perhaps there was nothing special to tell. The results were, of course, extremely special for us and indeed for this research. Furthermore, if this project had failed it is doubtful whether there would have been any development of sentencing guidelines.

Friends of research may be unexpected

For my part I find salutary lessons in this situation. George Reed was a Presidential (political) appointee and was saddled with the image which goes with such appointments. It is not the image of a person who would accept personal responsibility on behalf of social research and certainly not on a point of sampling rigour! Again, who would have expected him to set up the 'bonding sessions' when the show was first put on the road? A rigid control of expenditure by the agency providing the funding would not have been likely to have permitted this 'boondoggle' which, as I now see it, was the very operation which provided the corner stones upon which success was built.

The Decision Approach Emerges.

The question was now transformed from how to provide information (without constraints as to how decisions should relate to it) to the question of what kinds of constraints might be placed on unbridled discretion and what procedures would be needed to safeguard the procedures. This implicit specification was not made on the basis of any theory -- it was a structure which grew out of necessity. It came to be seen as the only way of resolving the problem of disparity. We had to specify a system which would be seen as ensuring fairness.

Our thinking became less constrained by what we saw as political realities. To put the matter rather crudely, we saw no point in the human decision makers struggling with information which could be more efficiently recovered or processed by machine. The legal viewpoint was dominated by a view of 'precedent' which led us to believe that 'artificial memories' could be exploited. What was needed was a system which would make as much use as possible of 'machine intelligence' (not only machine memory), thus enabling human decision-makers to concentrate upon the non-rule based and unique features of any determinations.

Our objective was to devise a system analogous to having a slave work force make an initial rule-based decision, and for this to be a first step in a decision process. The human decision-makers (Board members) would review the 'mechanical' decision and accept it unless they had good reasons to think otherwise. If the initial rule-based decision was thought not to be applicable to a particular case, there were procedures which were designed to utilise the special features of the human decision-makers, both as individuals and as a collective intelligence.

We recognised that there was a danger of 'rigidity' if we were to attempt to 'structure' parole decisions. When laws are enacted they are tailored to a specified purpose, but over time they tend to take on a life of their own. For this reason we have "law reform commissions" and similar bodies to maintain the relevance of laws -- but these procedures do not always work or laws come to be disregarded. We wanted to create a 'learning system' (a 'self-homing' missile).

It was necessary, we thought, to distinguish between rules for decisions and procedures for any desired challenge, then to make provision for modification as a continuous process. If we had a 'learning system' we could begin with a set of decision rules which assumed that the present practice was reasonably good. The major question was whether, and if so, how that practice could be 'modelled'.

The Breakthrough. A specific model is devised. .

We needed a re-think of the task and how best to work with the Board. So Pete came up to Albany to discuss. It had been my habit to encourage relaxed lunchtime discussions with whoever was around at the time. Around at the time was Jo Shin, Jo had just completed his dissertation under my supervision -- with some problems with other faculty! Jo was not only qualified in law (Harvard) but was a good mathematician

and linguist. A 'smart cookie'! Pete joined me for lunch with Jo Shin. Discussion turned to the information problem and, since Jo's dissertation had related to decision theory, this topic came in too. Our joint thinking began to focus on the relevance of decision theory to our work with the computerising of information for the Board. What was required was a managerial tool. However, I thought that the framework (constraints) which such a model would impose on the Board would be difficult for them to accept. As with Judges, Parole Board members' freedom of discretion in decisions was jealously guarded.

At lunch, no ideas were ruled out on grounds of probable acceptability, and we played around trying to fit a decision theoretic approach to the parole problem. Pete has always assured me that our ideas were first put down on a table napkin during lunch, but that may be a dramatic gloss! Perhaps such was the first note of the decision approach, but if so, it was not preserved for posterity. However, it is possible to identify the precise situation because, by pure coincidence, instead of a blackboard, we were in the habit of noting discussions on 'flip charts'. On return from lunch to my office, the three of us set to work sketching the logic on a chart. The significance in this roughing-out of the idea of decision theory was, of course, not fully appreciated at the time. By chance the flip-chart sheet on which the sketch was made was not destroyed, see Figure 9.

It was clear (as the chart illustrates) that we had enough data to fit models to the decisions of the Board. But how might this be used to assist the Board? The decision approach would mean substituting "structured discretion" (the selected model) for the discretion of the individual decision maker. The equations could provide the Board with a 'presumptive decision'. We sought Pete's views as to the likely room for manoeuvre we had with the Board. Put more crudely, how much would they stand for! I admit that I was much surprised when he said that he thought he could 'sell' the idea of 'presumptive disposition'. If anybody could, he would be that person.

Packaging the idea.

If a presumptive time was derived from the decision model, then there had to be procedures for these to be challenged and over-ridden when this was reasonable. Such challenges were tantamount to setting aside precedent (the basis for the presumptive determination). By what means, how often and by how much was such revision to be acceptable? We had also a problem of presentation which had to accommodate the Board's discretion. The term 'presumptive' only came to be applied later and initially we talked of 'structured discretion'. The benefit to the Board by adopting this approach was clear. It was a brief exercise in marketing! In fact it so turned out that, after Pete's presentation to the Board, the idea of "guidelines" with the review procedure proposed appealed to the Chairman.

The technical write-ups (over several volumes) explain the rules and procedures which were developed. Procedures were designed whereby rules could be modified. The amount of work required (in terms of consultations and such) was related to decision

difficulty. The most difficult cases (e.g.: highly politically sensitive crimes) required the whole Board sitting *en banc* to make or verify the decision. These procedures enabled the decision process to be seen as two-stage --- there were rules for making the decision in the first place, and there were procedures when this process suggested dispositions which were regarded as unacceptable. The inmate could know the expected 'time to be served' by calculation of the 'guideline', and, if there were unusual circumstances which the guideline model did not include, these could be presented in an appeal. The grounds for appeal were thus restricted to factors not already taken care of in the model. The new situation (structured discretion) replaced their unfettered discretion and probably helped to improve the image of the Board.

The sundry procedures developed around the decision model were complex and this is not the place to go into the substantive material of the research, save to say that initially we established a scale for seriousness of crimes which we compressed into six categories. The system became more and more complex as time went by!

A Relic is Preserved by Chance.

At the time that the original flip chart was re-discovered guidelines were in use in many states. These used a matrix, breaking seriousness of crime against probability of recidivism with the intercepts (confusedly called cells!) showing the presumptive dispositions. The parole guidelines which led directly to the establishment of the Federal Sentencing Commission began with scribblings on a table napkin followed by further argument represented on a scruffy flip chart (Figure 9) that was treated as wrapping paper for items for storage when I went on sabbatical to Australia!

Research management, again a factor

If this story has any meaning it must surely underline the fact that research cannot be managed in the same way as any other collective human activity. I know I may be belabouring the point unnecessarily, but the 'guidelines' project is another and dramatic illustration of the techniques of research management for which I commended Moss of the Social Survey. Serendipity, which is more than just simple chance, plays an important role in research, and it is necessary to create and maintain an environment where it can happen. Too close an organisation is unlikely to be a good breeding ground for spontaneity, imagination and happenstance; all necessary ingredients of a productive research management.

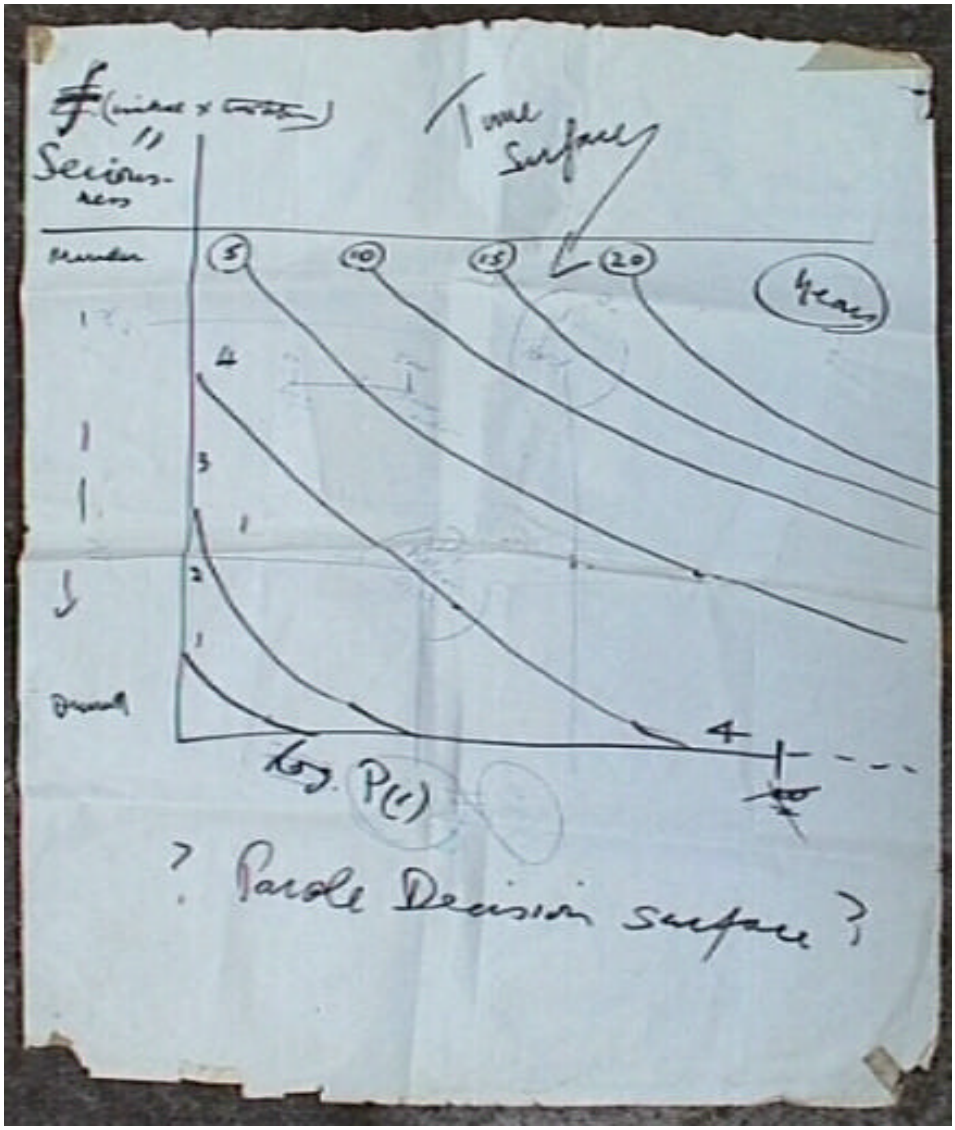


Figure 9. The flip chart on which the guidelines for sentencing were first described.

Few judges seemed to be concerned about the change in the function of the Parole Board. The work extended to other states and similar systems were adopted. In some areas the basic equations needed a different logic to achieve the maximum explanatory power. We took the view that if we could predict a Board's decisions with 80% accuracy we had an adequate foundation to proceed. Several states were assisted to produce their own versions of the 'guidelines' to reduce disparity.

The System Expands: the Idea of Guidelines for Sentencing.

Federal judges were, of course, the first to become aware of the use of 'guidelines' (presumptive time to be served) and became interested. Some were known to refer to

the matrix (seriousness x risk) table used by the Board before pronouncing ` other than incarceration.

Extension becomes Transformation.

The next events in relation to guidelines and judicial decision making was more by way of transformation than development. Up to this time the whole system in the various states as well as at the Federal level remained within a managerial framework and a managerial philosophy informed all perspectives of the work. Senator Edward Kennedy's research assistant by some means became aware of the 'guidelines' and sought to prepare a Bill to give the system legal support. (see Kennedy/Rodino Bill).

The extension of guidelines to sentencing became a separate project beginning in Denver, New Jersey and Vermont. The climate is perhaps best indicated by a letter to the New York Times by two judges of the N.J. Supreme Court. Judge Yanoff and Judge *Marzul*li wrote advocating the idea to the New York Times. which they published in full. (see Figure 10).

Attempt to Involve UK: The Isle of Man Proposal. [20]

As Denver and Vermont progressed and became somewhat routine, I began to look for 'something different'. For reasons which I cannot now imagine, I apparently became interested in spreading the ideas of guidelines for sentencing to England. I knew enough (I thought) about the Home Office to be extremely cautious and to move slowly. It was clear that nothing could be achieved without the agreement of that department and perhaps also the Lord Chancellor's Division. If either of these departments disapproved, that would have to be the end of the matter.

I had to have a well-formulated proposal that would not involve any charge on the budget and first to get its acceptance in principle by the Home Office. I set about preparing the ground. The basic idea was related to the Sausalito conference which Don and I had set up when I was at Berkeley and which proved so successful. We could arrange for a seminar on sentencing guidelines at some pleasant location in the UK. Two judges who had been members of our Steering Committee, Yanoff and Marzuli (the authors of the letter in the New York Times) agreed to my suggestion that they address a conference of British judges, magistrates and policy makers at a suitable location. They even agreed to pay their own costs. (The U.S. income tax laws would have helped!). I suggested the Isle of Man as a possible location for a two-day meeting, and this appealed to them. Accordingly I set about sorting out the situation in the Isle of Man during a

20 This was an attempt to set up a judges training scheme with the assistance of the U.S. and particularly those judges who had adopted sentencing guidelines. The Home Office raised 'constitutional' objections.

vacation. I had in mind that if we began with a seminar that it might develop into a

Letters

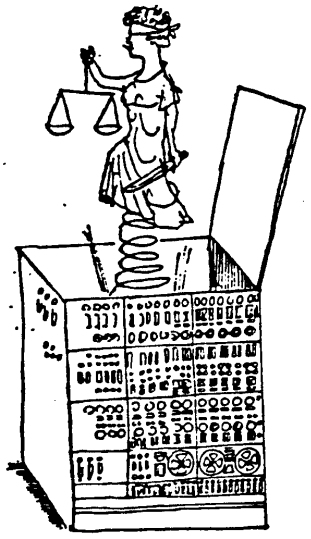
Toward 'Pragmatic' Sentencing Guidelines

To the Editor:

This letter is prompted by Senator Kennedy's July 29 Op-Ed article about sentencing guidelines to be established by a commission. We hope that careful consideration will be given to a study financed by the National Institute of Law Enforcement and Criminal Justice of the Law Enforcement Assistance Administration, entitled "Sentencing Guidelines: Structuring Judicial Discretion." This resulted in the "Final Report of the Feasibility Study" of October 1976. The committee which participated in the study consisted of a statistician, technical experts and judges from various parts of the country, among whom we were included.

The report sets forth a technique making use of well-established mathematical and statistical methods, with computer assistance, for determining the central tendencies of sentencing decisions of judges in a given jurisdiction. In the language of the final report, it is "descriptive" of what the judges have done; it does not purport to prescribe what judges shall do in their sentencing decisions.

The report describes a system which takes into account both the relative gravity of the offense and the personal characteristics of the offender, including his criminal record and his social stability, for the purpose of constructing relatively simple matrices which describe the median sentences imposed for offenses of certain gravity and offenders having characteristics similar to the case under consideration. These results can be used by the judge as an aid to his sentencing decision.



Mark Podwal

One merit of this technique is that it makes available for the sentencing judge an overview of the collective experience of judges in his vicinage. It shows what has been done in real life with real defendants.

It is pragmatic, rather than theoretical. It has the advantage of reflecting what was done by those with direct responsibility for imposing sentences. Guidelines formulated by experts, however well qualified, who have not had the practical experience of sentencing cannot have the same significance. Sentencing is not something which can be reduced to scientific certainty in our present state of knowledge. The collective sentencing conduct of judges is a solid factor which should be used as a basis for sentencing decisions.

The sentencing disparities to which Senator Kennedy refers, documented by many studies, is the result in substantial part of the differences among judges themselves. It has been shown that the severity of a judge's sentence often depends upon his social background. Additionally, a judge may not be consistent in his sentencing because of variations in his psychological state from day to day. It is our conviction that use of the technique formulated by the study to which we have referred will tend to diminish these differences and should help to remedy the evil of sentencing disparity.

Sentencing guidelines formulated in the manner described are already in use in the State of Colorado and Cook County, Illinois, to the satisfaction of the judges there. We sit in Essex County, New Jersey, where sentencing guidelines have been implemented with the assistance of a grant from L.E.A.A. The administrative office of the Courts of the State of New Jersey is conducting a study for the purpose of creating guidelines for possible use statewide.

It is our hope that proponents of the Kennedy-Rodino Bill will pay serious attention to this method.

(Judge) LEO YANOFF
(Judge) JOHN A. MARZULLI
Newark, July 29, 1977

Figure 10. A letter from Yanoff and Marzulli in the New York Times for Friday August 12, 1972

continuing operation. I even visualised an independent judicial training and research institute! In the U.S. there was considerable positive interest at the 'highest level' not only for a short conference but the possibility of establishing a centre.

We were ready to go ahead and advertise an international seminar at no cost to the Home Office or the Isle of Man, though the latter offered their support in other ways. I accordingly wrote to the Home Office on the 31st August 1977 enclosing letters of support from the two judges and emphasising that they were members of the Supreme Court of New Jersey. I made it clear that no financial commitment was requested but that we needed official 'blessing'.

This inquiry was met with silence which, perhaps, I should have seen as dumb insolence! I pressed for a reply. The reply (see Figure 11) was eventually given over three months later on the 5th December 1977.

It was a 'put down' which could have been more diplomatically phrased. Or could it? The phrase "seen unfavourably" is a highly coded one, and this followed by the words ".. at least of the sort that appears to have been successful in the United States". (Note also the "appears to have been ...").

I had to write several letters of explanation for the failure of the project to take off. They were difficult to phrase because I did not think it wise to quote the official response I had received. Perhaps I should have been more persistent and gone through party political rather than official channels? But I decided that discretion was better than valour, and I did not want to make enemies for when I retired back to the 'old country' in three years' time.

The official view of the relevance of experience of sentencing in the United States (as represented in Croft's letter, see Figure 11) was more harsh than that in the Report of the Advisory Council on the Penal System [21] which was briefed on the guidelines method by Jack Kress on his visit to London in 1977. This read, in part "The system of sentencing guidelines, now making headway in the United States as a compromise between indeterminate sentencing and a system of more or less fixed penalties, was of special interest to us, both because the philosophy of steering a middle course between a wide and narrow discretion in sentencing was the one which most appealed to us, and because the practical solution of adopting a penalty system based on the existing practice of the courts was that which we ourselves ultimately decided to recommend" and again they are even more specific "Sentencing guidelines would have, of course, a more direct impact upon sentencing than our own proposals". Nonetheless they concluded that it was doubtful that such a system would be acceptable "in the English context".

21 see Review of Maximum Penalties HMSO Feb 1978

HOME OFFICE
Romney House, Marsham Street, LONDON, SW1P 3DY
Direct line: 01-212 6826
Switchboard: 01-212 7676
Telex: 24986

Our reference: RES 503/6/88
Your reference:

5 December 1977

Professor L T Wilkins
School of Criminal Justice
Mohawk Tower, 19th Floor
The University at Albany
Albany,
New York, 12222
USA

Dear Leslie

I apologise for taking so long to reply to your letter of 31 August and I am sorry that you were put to the trouble of writing to me again - your letter of 20 November awaited my return from ten days abroad. Your suggestion, however, raised a number of questions about which I felt it was necessary to solicit the opinions of colleagues,who have, as it turns out, judged the matter to be even more complicated than it first appeared.

Generally speaking, the Home Office view is unfavourable to any suggestion to introduce a system of judicial guidelines in this country - at least, of the sort that appears to have been successful in the United States. There are also some quite serious arguments against holding a seminar on the Isle of Man (there are constitutional and other difficulties), although the subject of sentencing disparities is regarded as an interesting and important one, which the Bridge Working Party on Judicial Training and Information is currently looking into.

The Lord Chancellor's Office is now considering what value there might be in an interchange between American and British judges. Whatever they may decide, I can say that a seminar of the sort you propose is not practicable at the moment. If there are any other suggestions I will, of course, let you know.

I regret that I am unlikely to be in the Office over the Christmas period.

Yours sincerely
John Croft

I J CROFT

Figure 11. Cooperation rejected.

Though the conclusion of the Advisory Council was not a recommendation for guidelines to be adopted, they did not express any hostility. It is interesting to note that this opinion was given at about the same time as the highly negative (indeed, hostile) 'official opinion' as expressed by Croft. Someone was out-of-step. Apparently it was the Advisory Council, because it was abolished shortly after presenting this report.

I now think that I should have had nothing to do with my old department either at this time or later when we returned in retirement some four years later. But I tried! How many mistakes can one make in a lifetime!

Meanwhile, back on the ranch.

Meanwhile, back in the U.S.A. things were moving too fast and becoming too widely dispersed to remain a well controlled experiment. Most of the activity was now beyond my control. What had been a management tool was fast becoming a set of complex laws. Instead of the flexibility of a managerial model the rigidity of legal definitions was taking over. The 'power structure' was ousting the research structure.

I regret, now, that I did not realise the enormity of the transformation taking place. I had, indeed, taken the view that change as such was desirable -- the model should grow and adapt. There seemed, at the time, no major objection to the idea of guidelines being extended from parole to sentencing decisions. It proved, however, to be a major and eventually disastrous change which first affected the image of guidelines and later their operation. This transformation of the simple basic idea of mapping and 'notional' decisions attracted all kinds of legal appendages.

Sabbaticals.

I have already mentioned that I decided that the development of guidelines might be left in charge of others and that I could take sabbatical leave. While based at Albany I had two sabbatical leaves. As subsequent events proved, it was probably unwise of me to take my first sabbatical when I did, but I had to fit in with others. Barbara and I discussed and we agreed that New Zealand and Australia seemed 'good places to go'. I had, of course, contacts in New Zealand and Australia -- the Attorney general in the former and the Director of the National Research Centre in the latter. Though the distance meant that I would not be in frequent touch with the research back in the U.S., it seemed reasonable to go because I wanted my research associates on the sentencing project to develop their own approaches. I thought seriously that I might inhibit progress because I was not closely associated with the legislative structure and I was not a lawyer. Furthermore, I was, after all, still a foreigner and judges and prosecutors tend to be political.

My 'hands off' sabbatical did result in change, indeed change in the basic philosophy. In my absence the managerial tools were converted by lawyers into rules and regulations. The guidelines became seen as constraining rather than as facilitating decisions. Then, having shifted the model into the judicial and legislative gears, the whole thing became

political, even where the system had been applied appropriately. Too many different interests were seeking a place in the system. To have retained the original model would have required me to make a move into the political world and to retain control. This was not my role. I was not, in any event, prepared to become an American citizen, which would have been a requirement of any politically powerful appointment.

At the same time as the transformation of the model was going on, other problems developed and all coincided with my departure for Australia. I had left a team operation, but immediately after my departure it seems that a power struggle developed. I was too far away to know precisely what took place, but it emerged that Jack Kress (a lawyer and junior member of the faculty) took over. He was not, I regret to say, popular with staff, and so far as I know, soon dropped out of the academic community. Whether my advocacy would have been more or less successful will never be known.

Other Features of Australian Sabbatical.

The Australian visit was productive but I had my first real problem with health while there. With Bill Clifford, Director of the National Research Centre, I developed a model f or guidelines for application to bail decisions. A publication and a presentation to a governmental committee were involved, but this fizzled out.

I had been having internal pains and various attempts at diagnosis had been made. The least serious suggestion was stomach ulcers. The Australian visit might, I thought, provide conditions which would encourage spontaneous remission. But on a side-trip to Perth and Adelaide I collapsed. I also went yellow! This made diagnosis somewhat easier. The Australian clinic suspected that my trouble was gall stone. This was not a popular diagnosis because the susceptible individuals were said to be "fair, fat, female and 40!". I fitted none of these categories. I was certainly not fat!. I was also more than a decade past 40, and only fair if grey counts..

I was offered an operation in Canberra hospital, and I have no doubt that it would have been well done. My consultant was a typical Aussie rather than a typical medico, and had the language to go with the image. We discussed the probabilities and he agreed that, provided the airline did not suspect my condition I could make it back to the UK. Dark glasses were called for! We packed instantly and a few days later I was having an interview with a diagnostic specialist in Harley Street. He confirmed the diagnosis and I was forthwith consigned to St Thomas's Hospital. The confirmed diagnosis was a gall stone which had created a complete blockage -- obstructive hepatitis. Fortunately my U.S. health insurance would cover the costs of a private room on the top floor overlooking the Palace of Westminster. Not that I cared much for the view. Removal of the obstruction was effected urgently within a mere two days from leaving Canberra (ignoring adjustment for the time difference) Recovery began.

Bill Clifford of the National Institute in Canberra had an apartment in Malta and he made this available to us so that I had the benefit of pleasant weather -- it was November in England; and, of course, in Malta too. But that was different.

A Home Office Visit

On convalescence I decided to pay a friendly visit to the Home Office Research Unit and socialise with some of my earlier colleagues. The office was within a few meters of the hospital. Nothing seemed to have changed. The same messenger was on the door, supported by the Manx cat (well a Manx cat -- I never got to know the original well). I was recognised by the messenger, and I remembered one or two of his interests. We talked for five minutes or thereabouts. Then I started to walk up stairs.

"Where are you going, Sir?

"I'm just going to have a chat with some of the Unit"

"You can't do that"

"Why not? You know me!"

His response was classical,

"I used to know you, Sir. You must complete the visitor's pass application".

While the gate keeper knew me very well, he was not now authorised to 'recognise' me. Unofficially he knew me, officially he did not recognise me. (Where had I experienced something like that before?) Some fifteen minutes later I was under escort with a 'pass' to the same office which a few years previously had been my base. But I was no longer 'recognised'. This was the first time I realised how I had been insulated when I was 'up stairs' and the Manx cat was below. I found it somewhat horrifying.

After recovery I was able to once again visit Fuchu. On the course was a Signalise participant, known to his friends as "Karu" (the first two syllables of his name). He later took his doctorate with me at Albany and settled in the U.S. academic world. When I last had news of him he was a full professor at the University of Nevada, a state university which is quite wealthy because it is supported to a significant degree by the local gambling industry. Who can complain? We now have the National Lottery.

Simon Fraser, British Columbia.

Prior to retirement I was entitled to a further short sabbatical and I opted for a semester at Simon Fraser. I have no idea why we/I selected Simon Fraser. It was an interesting place, literally a university situated on top of a mountain! There was nothing other than the university within some miles and a few hundred feet below. Driving from our residence in Port Moody to classes often took me through layers of fog and up from a warm plain to snow and ice. The views were fantastic -- much like the fjords of Norway. But there was no faculty club or dining facilities. It was democracy gone mad! Parking was on a priority basis determined by how long one had been at the university, irrespective of role or rank. Third year students took all the places anywhere near the buildings, while visiting staff and new appointees had to drive almost half a mile to a

location and then walk to the Campus. On occasions I was able to make a deal with another member of the faculty who had qualified (by time!) for a nearer place.

Dining in the commons was rather like trying to eat at an overcrowded McDonalds. The alternative was a drive down the mountain and up again. It was not an environment to encourage faculty discussion outside the formal provisions in the department. I understand that recently a faculty club has been provided.

Research at Simon Fraser

I can remember some of the events on the Simon Fraser sabbatical from the research I was able to do while there. Later publications provide a reminder. At this time I was interested to sort out a theory for the rational disposal of offenders or to explain disparities in sentencing. We had earlier shown that given the same case materials judges gave, on average, the same penalties as a sample of students in non-specialist subjects. This may sound surprising, but the key is in the word 'average'. The average does not reveal the extreme variation, so, for example, we had the same case disposed to probation and to 10 years prison. It seemed necessary to try to find some logic (or common prejudices) behind the variation. The variation in sentencing had been dealt with for the U. S. Federal system by the use of guidelines, though the rigour of this approach was already beginning to be eroded by politicians.

One idea seemed worth testing. I postulated that the variation in degree of punishment awarded for precisely similar offences might be multidimensional. In other words, we might find consistency in some of the components of the thinking which matched the punishment to the crime, but not in some others. I postulated that the ranking of the seriousness of crimes (as crimes -- without reference to who had committed them) would be fairly consistent. Variation might enter more strongly when one considered 'who had done it, and 'to whom,' rather than in 'what had been done'.

I was asked to give a talk to a prestigious group of judges and probation officers. At this meeting I was able to recruit volunteers to carry out an experiment. The full write-up appears in the book which I wrote in part at the time and finished later. Laymen, students and judges all tended to rate the seriousness of crimes similarly. The next step was to try to sort out features which were regarded as either mitigating or aggravating the gravity of the crime -- these features could include facts about offenders as well as added data regarding the victim or the situation.

What I needed was a glossary of terms for all factors which could be either mitigating or aggravating of the (constant) crime. The volunteers were able to give me a list of items in these two categories. One list, mitigating factors, was much longer than the other. I had previously used a method for sampling concepts , but on this occasion no sampling was necessary: the trawl yielded fewer items than I expected and I used all items I could get. Some interesting differences in ratings of these modifying factors emerged from the different groups.

Sentencing! Truth in Jest?

However, the outcome of this work was mainly in providing more ammunition for cynical views of the operation of a criminal justice system. An extreme viewpoint which clearly I do not endorse, has the merit of humour and the jest is worth repeating. The advice on sentencing for magistrates is made up like a cook's recipe, thus, "take equal quantities of politics, public opinion and personal subjectivity, stir thoroughly, spice with your favourite religion, then look at the newspapers and if there is no comment, go ahead and dish it out. Otherwise hold over disposition until the comments of the taxi driver become known on the way home".

But seriously, we have not been able to get much further than that! Political considerations which are loosely connected with public attitudes seem to be the main determinants of both the definition of crime and of the extent of punishment for acts so defined. Furthermore, because society persists in selecting as judges individuals believed to be good, experienced decision makers rather than in the construction of effective systems, all we can know about sentencing decisions will vary according to whoever makes the decision.

The guidelines idea, unfortunately, became too popular with politicians. The Federal Sentencing Commission began with good promise, but despite the devotion of Pete Hoffman it began more and more to reflect political perspectives. The managerial model had died years before. I cannot now recognise the original idea (or ideals).

Sentencing variation, I am told, is often explained by the concept of 'mercy'! If so, then a question which must be settled is that of the relationship between just desert and the exercise of mercy. Can the amount (?) of mercy shown be decided in terms of 'justice off-set'? Surely it can only be a balance based on sentiment? This symbolism has little or nothing to do with techniques of managing a decent society. I give up -- lawyers' logic is clearly of a different order from that of a social scientist. There are more useful things to do. So let's take a break!

Images of Old England

Vancouver Island, on which the capital of British Columbia (Victoria) was located was approached by ferry which weaved its way through countless islands and rocks. Victoria was much more "English" than England had become. Afternoon tea, with cucumber sandwiches was served at the hotel and double-decker red 'buses plied around the small city. High level civil servants sporting umbrellas and even the occasional bowler stepped from their seaplane taxis on to the quay near the Government Building. This old England was seasoned with the culture and traditions of the local Indian people.

Driving north from Vancouver one was soon on rough tracks -- the roads became logging trails, not highways. We ventured on one occasion as far as we thought reasonable. We were in Indian country and the Mounties were also much in evidence in the small town. There was only one eating place, and it was 'rough'. It was controlled by the 'waitress' who knew the locals and their ways -- she could handle those drunks! She

asked us to wait for our meal because she had to humour a bunch of rowdies who had come in from some sort of 'sports' event and were already well oiled. They wanted more, but our 'waitress' saw to it that they did not get it! This fact owed nothing to the two police cars parked outside! In the toilet I was approached by a large burly native with a whispered, "Want some salmon, cheap?". I tactfully declined. Salmon in the upper reaches might be fished by natives, but only for their own use. Selling (and certainly purchasing poached salmon) is a crime -- and the cars were waiting! We beat a hasty retreat towards the south.

Port Moody reminded me of a point made by the protocol officer in Tokyo about the Japanese. "They have a great sense of beauty, but no sense of ugliness". Port Moody was in a beautiful location between mountains at the head of a fjord. Our house looked west down the fjord towards the city of Vancouver. However, within sight on the opposing shores and backed by mountains were two polluting factories; one an electrical generating plant and the other off-loaded sulphur, did some processing of chemicals and belched dark smoke at frequent intervals. But the unloading ships, at our distance, looked pretty!

But, still, we enjoyed the environment around our residence and in the Vancouver area. We were also within easy reach of the mountains and forests of Washington state and indeed walked on the slopes of Mount St Helens. On our return drive back to Albany, we spent a night at a motel in the village which was soon afterward destroyed by ash.

Handing over -- Retirement looms.

Back in Albany the fact that I was nearing 65 dawned on me. No one else seemed to notice! I began to think of retirement, not so much as ceasing work as making way for younger faculty to take up my 'line'. The School, my colleagues and the university pointed out that I was not required to retire at any age, and they certainly did not want me to depart. I knew that many of my colleagues did not plan to retire at 65, but I was inclined to take the view that to stay on beyond that age was not a good thing for the long term interests of the School. While we stayed, young professors could not be promoted and we were blocking 'lines'. Eventually I agreed to stay just one further year and come hell and high water I would depart in 1981. This, my family saw as an irrevocable commitment. It was a pity that at the end of this year's extension the School was faced with a loss of two very significant members of the faculty. Mike Gottfredson and Travis Herchi left to take up appointments in the sun belt at the University of Arizona in Tucson. My departure made three major vacancies. But I had to resist entreaties to stay. We had to return to England.

Barbara pointed out to me that many years before I had made a commitment to do so. This was an interpretation of a humorous comment I had made some fourteen years earlier. Ronald Reagan who at the time, it will be recalled, was Chairman of the Board of Regents at Berkeley had expressed ambitions to higher office. His chances were assessed as zero. His statements were the material of various forms of ridicule. On an occasion I was asked by my family when I planned to return to England. Without further

thought I said, “When Reagan becomes President” This had the linguistic equivalent in the Victorian phrase “When pigs fly!”. I had to be as good as my word!

My Farewell Party (Albany)

My send-off party was a disaster for the cook. We had had our first two courses and then there was a very long unexplained delay. Clearly something had gone wrong and the junior administration were 'stalling' or covering up. The dining commons had just been fitted out with a brand new kitchen and the chef had prepared a special dessert which was set out awaiting its delivery to the tables. It was at this most inconvenient time that the new fire alarm system was triggered. The result? A fine powder descended over everything in the kitchen. There was no fire; it was a malfunction of new equipment. The delay in the serving was occasioned by the total spoilage of the chef's special tribute and students were scouring Albany to try to find a substitute which would preserve the dignity of the occasion. I was very sorry for the chef's embarrassment. The occasion was indeed memorable though not quite in the manner and style intended.

Texas is better (not bigger) than its image.

In England there are cities which are the common butt of jokes. In the U.S. where everything is bigger, the same kind of humour attaches to Texas and to Texans. President Johnson survived this image and even popularised it with many democrats. I thought that before settling down in England I should do a stint in Texas. I had earlier attended a United Nations sub-committee which had met at Sam Houston University in Huntsville. A new School of criminology had been founded there and I was invited to become a Beto Distinguished Professor for a year. The idea of a full year in Huntsville was not attractive and we came to a compromise -- one semester, no more.

The Sam Houston Huntsville campus was extremely well designed and luxuriously furnished: even doctoral students had office space and access to various personal computers. Part of the campus was a motel and it also had its own court room where the occasional trials could be held. It also served as a moot court and special lecture room. I gave the inaugural lecture there. Living accommodation was provided in a furnished house on (actually on) a full-sized golf course with its own lake. Access was restricted to golf club members and residents and monitored by 24-hour armed guards. It was located a short distance off the main N-S highway between Houston and Dallas. Cedric, our second son, on a business mission for Scottish Enterprises (his employer) dropped in for one week-end while we were there. For me this arrangement was good enough -- I had allocated an official car, but Barbara was stranded with a few golfing ladies in the Club House! It was not quite her thing.

We remember the beauty of the 'blue-bonnets' which lined the highways and the rich displays of other flowers. Fall and winter in Texas was not at all bad! We decided that we would miss the hot summer season, by leaving for Japan in the spring. En route to Los Angeles airport, by car, we crossed three deserts. It had recently rained and the

deserts were blooming. Our photographic collection records the deserts flowering with all kinds of plants. We remember too the snakes, cockroaches, snapping turtles and mud. But I will not take time to fill in.

No note which mentions Huntsville can fail to say something about the prison. Huntsville IS the prison with the university campus as a leavening of its dominance. It is BIG. To an English visitor it seems extremely open while being depressingly secure and threatening. The perimeter is covered by gun turrets, but within there is open space. There is a shop open to the public during normal shopping hours which sells items made by the inmates. But most incongruous of all is the annual rodeo. The sports stadium within the secure area is as large as the average British football stadium. Here a most professional rodeo is staged which is the equal of any. This event is advertised to the public by all kinds of publicity including 'bumper stickers'. I was fortunate to be in Huntsville during the rodeo show and was taken by a member of staff who explained the various routines and roles. Despite the punitive politics of Texas, it seemed that the ambience in this maximum security prison was much less oppressive than the general climate in British prisons. The large spatial area was an important difference. Within the walls there was variety enough for a medium sized town.

The flight to Tokyo was uneventful, with an interesting stopover in Fiji, where we were taken for a ride by the local taxi cowboy! But we saw a bit of the island before continuing our journey.

Few of the staff who were at Fuchu during my first stay were still there when Barbara and I visited en route home from Huntsville. Things had changed all around, but the staff had delayed the demolition of the house in which I lived on my mission. It was much as I had left it!

I think Barbara did more useful work (unpaid, unofficial) than I did on this visit. Several afternoons each week she ran a conversation training time in the house. The Japanese enjoyed these lessons, and one of her 'students' from that time still corresponds most regularly -- at least once every month. What loyalty - - over fifteen years. One or two others have paid us visits in Cambridge. The day we left the old house was torn down to be completely rebuilt to fit in with the new and enlarged Institute buildings. The Institute is now operated by the Japanese Ministry of Justice.

CHAPTER NINE: POST RETIREMENT.

My knowledge of some aspects of social medicine gave me to believe that retirement was a danger time. So many civil servants did not survive for long enough to enjoy their pensions. The secret of survival in retirement is not to retire. "Spending more time with my family" though it sounds a worthy objective may not be as desirable as politicians often try to make it sound. .

Prior to retirement I had thought it desirable to minimise the interaction between my domestic life commitments and my business activities. It had been impossible to turn off one's thinking with the clock. However, the separation of locations had facilitated a partial mental separation of leisure from work, and to some degree facilitated also a partial mental separation. Retirement would destroy this separation. Domestic and research activities post retirement were much more interrelated. The complexities of personal management were increased by the degrees of freedom!

My academic retirement was a change of activity and coincident with a change of perspective. I had tried, with parole, sentencing and such research to work on problems of offenders and offending and to look at problems of those concerned with such individuals. The frustrations and obstructions experienced in this work were now to be left behind and I was to take a different track. I began to concern myself less and less with sinners and the saints who were hoping to save them, and to concentrate on ways of reducing temptations; to think less of justice and more of management techniques which might facilitate the development or maintenance of a decent society. The key concept was the quality of life. I did not, however, get down to the new line of thinking or work for some time. The social, political and academic resettlements were far from straightforward.

Selecting a location: the Old Police Cottage.

In our own resettlement we made some use of decision theory, in that we realised that we should begin by nominating constraints -- undesirable features, rather than the more common approach of trying to think "what one really likes". Of course, it is not practical to search a whole country, even a small one. So we searched mainly in areas in the southern half of the country, but included parts of Yorkshire where we found the coast most attractive (in summer). We did not like big cities. This left us with a strong preference for the rural areas. However, there was one class of constraint we did not consider. We had not realised that our desire to avoid towns was incompatible with our need for acceptance in the community. Rural England, we found, had strong views about strangers. It was too late when we realised that we had not given the social acceptance factor sufficient weight.

I do not know whether we consciously were satisfying a desire to visit our old haunts, but at one time we found ourselves in the Hullavington, Badminton area. I noted that the likely postal address of Old Sodbury would appeal to my students in Albany as

sounding quite suitable! We settled on a property which seemed to have most of the characteristics we wanted. It was remote and apart from its pair on the adjoining plot and a farm dwelling across the road; it was quite isolated. It was not only poorly designed but badly neglected.

The location of the dwelling meant that we had to have two cars in order to manage the essentials of living. The building had at one time been a police 'station' comprising two houses and an office. The old office was included within the property for sale. It was freehold, though as we later learned, it was the only freehold for miles around. The whole area was within the bailiwick of the Duke of Beaufort who owned the manor house at Badminton and hunted foxes throughout the area. He also ran the world-famous Badminton Horse Trials.

The plumbing was even more primitive than in most British dwellings, which we had come to regard both as quaint and totally unsatisfactory. The water and some central heating was (supposedly) provided by surplus heat from an open fire in the lounge. The original water piping for both hot and cold supplies had been fitted between the two layers of brickwork (in the wall cavities). This might have been reasonable if the house heating had been maintained. It had not. The pipes had burst and could not be replaced without rebuilding the house. So a new set of pipes had been fitted somewhat loosely on the inside walls of the rooms and office.

We had ideas as to what to do to make the place suitable for us. We had time; we had health, we had assistance and we had fun. A local builder/undertaker who had lived in the village all his life was a great asset. He enjoyed building a Cotswold stone fireplace which Barbara had designed. This replaced the odd apparatus in the lounge. We had oil-fired central heating fitted throughout (there was no gas available). Barbara worked far too hard in the garden and came up with discoveries (mostly undesirable) on most days for the first year. Eventually the house was looking like a home.

Our first winter in Badminton was an experience! It was one of those which England suffers rarely, and for which all services, public and private, are totally unprepared. It snowed. It was an unusual snow storm and the main road was blocked for more than a week. The Queen, who was visiting in the area, became snowbound and the event is still celebrated in the local inn at Old Sodbury. No rescue was made before the next day and the landlord had a distinguished bed-and-breakfast guest. The village was cut off, but fortunately we had brought skis and snow shoes from the U.S. and the main road made ideal cross-country ski-running. We later found that we had been skiing over the roofs of abandoned cars. Though the weather in subsequent years was kinder, the location, on a ridge overlooking the Severn valley was bleak in the extreme. But for a while it was fun.

The neighbours in the other police house were natives of the area. They were a retired couple who were excellent gardeners. They 'kept themselves to themselves'. We had no common interests but this position of neighbourliness when in need suited our book too. Opposite was a farm, and there was access to many lanes and fields. A negative feature was the fact that the foxhounds ran past the house on their exercises every

morning and the hunt came around in full regalia in its season. We often saw foxes idling time away in the farmyard. The view from the lounge was confined to the farmyard and its buildings, so we added a sun room with a view to the rear over our garden.

The old police office made up well into a study, semi-detached from the dwelling. The central heating never really coped with the distance, but supplementary electric bar fires helped. I had brought my computer, a TR 80 with CPS operating system, from the U.S. and it worked reasonably well on transformer supply. Indeed we needed transformers of various designs to provide the 110 v. we needed to be able to use the equipment we had brought back with us: saws, drills, mixers, shavers and even a carving knife, indeed most of our electrical apparatus. (Some 15 years later much of this equipment still functions in the same way).

The local electric power supply was not particularly reliable due to the overhead distribution wiring and the apparent shortage of separators. When the wind blew a gale the wires flapped and could touch with flashing results, but only for a brief period and then darkness! Because the power outages were almost predictable they were not a serious problem with the computer, but static certainly was.

There was much that was good and pleasant about the time we spent in Badminton. There was also much that was not so good. Social life began (and ended) when we were invited to a party given by the local magistrate. (His residence was the next dwelling to our north, about a quarter-of-a-mile distant). Perhaps we were too closely associated with "America" to be seen as genuinely English. The magistrate clearly found my views dangerously liberal or even worse. The possibility of local friendships was zero. For any entertainment we had to look elsewhere in the nearest city. Barbara was able to find some entertainment in Bath from time to time. At first, for me, the Sodbury Rotary Club seemed an acceptable group. Indeed I remained almost on friendly terms with the village solicitor (attorney!) for some while, even after we left the district. Later I learned the possible reason for his more accepting attitude. His brother, with whom he kept in touch, was a Cambridge academic; so he was not party to the general anti-intellectualism of the gentry. But no integration with other locals persons seemed possible.

We were too healthy to get to know the local doctor or welfare workers so we had no like-minded local contacts. An inn in Old Sodbury provided an excellent menu. indeed the food was its forte. We visited there from time to time, particularly when entertaining friends from overseas.

Country ... Ah! ... chemicals!

The social isolation might have been remedied, but there were other undesirable qualities to the living. The noise from high speed traffic on the A 46 (about 200 yards away) was a continuous annoyance though not quite as deafening as the low-flying jets which often were at less than 500 feet overhead and flying at near the speed of sound. This noise was added to by farm machinery. The grinding of silage is not only noisy: it

stinks. Each morning, including Sundays, and far too early for respectable non-workers like us, the milk lorry would pull into the gate opposite. The loading was not a silent operation. Then, the noisy objections of the cows when their calves were removed was a mournful and frequently performed cacophony. We also suspected that fertiliser sprays were probably even more unhealthy than they smelled. If we began with an image of an idyllic countryside with wild flowers and pleasant lanes, most of this was all too quickly shattered. Living in the countryside began to seem little different from living in close proximity to a noisy chemical factory.

Gradually the stinks, noise and social isolation began to seem more and more significant. After two or three years we began to think of relocating. I will not detail our research processes, but one change in our thinking of the location problem is worth mention. We had normally been concerned with features of a district but we began to see the significance of the immediate locality, and this aspect of search became of much greater importance.

Our elder son had been located in Cambridge for several years and through our visits we came to know that area well. There were dozens of villages clustered around the city, but with our experience of Badminton we decided against any rural location. We were most fortunate in finding a three storey 'town house' under construction in a court of nine units in the central area, but strangely insulated from the noise of roads. We found the builder and purchased immediately in its incomplete state. This enabled us to add a few modifications, though building codes and the covenants with the prior land owners (Peterhouse) meant that these were limited. I may venture one piece of advice if ageing retirees should find themselves in a similar position -- it is essential to have a toilet facility on each floor! It is now ten years since we moved in and the structure is as new, apart from some of the vacuum glass which failed soon after the expiration of its guarantee!

This town house was in a very different setting from our first purchase. We were now old enough to appreciate the freedom from hassle that a new building ensured. Given that I have to be in England this is probably by far the best home base we could find. There are good and pleasant neighbours . In fact our next-door neighbour had worked for the Social Survey, We had found a suitable micro locale (ghetto?).

Readjusting my orientation

Retiring from the U.S.A. meant leaving my intellectual home, and initially I made quite frequent return visits. Over the years the frequency diminished. After seven or eight years the frequency had dropped to about once a year and then after fifteen years to less than once. The earlier visits were to continue various aspects of my research and to participate in academic events at Albany and to receive various honours as well as continuing regular attendance at the American Criminological Society's Annual Conferences.

In 1992 the School of Criminal Justice celebrated its quarter century. It was also then incorporated administratively within a new "super" School of Public Administration. I was invited to attend and was awarded with an academic rank of Research Professor. This involved being paraded around the town of Albany in gown and mortar board! Then to the steps of the School's refurbished old building for a ceremony of eulogising and the placing of the appropriate hood over my head. Professor Bill Brown did the oration (in English!) and Professor Graeme Newman the hooding! Unlike Berkeley, the School at Albany attracted few overseas students. There were, I recall, only two Britishers and one Shri Lankan candidate while I was there. One Britisher a keen and I might say, expert photographer, David Orrick, went around and took a number of pictures (e.g. Figure 12). We still keep in touch by e-mail.

Figure 12. Honoured by my School.

It was a pleasant time, and it is good to know that I am entitled to wear 'academic dress', though I now have no idea as to the shape, style or colour of the outfit. I just put it on when it was provided. When it was taken off I was not taking much notice!

One of my last visits to the U.S. is perhaps interesting. This a vacation in 1997 vacation in Florida. We flew to Atlanta and drove south through the Bible belt, which was gruesome. We had no fixed plans, so when a hurricane appeared moving on a random course around the southern parts of the state, we decided to keep our distance and went westerly towards Pensicola. It was an interesting route, and far more attractive to us than the popular central areas. Among other things we made the acquaintance of swamps and dugongs. Towards the end of our trip the hurricane had vanished and we drove (en route for our return flight from Atlanta) to Savannah Georgia where we spent

two days with Peter Hoffman and his wife. Pete had telephoned and insisted on driving down from Washington to meet us.

Attempted reintegration into British life.

I had anticipated that my contacts with the U.S. would gradually fade. I suppose distance is still a significant factor. The vacuum had to be filled by some means. The domestic and social affairs took up quite a bit of time, but as these demands also gradually diminished I began to look for other things to do. I have said enough about social and domestic details of resettlement and I turn now to matters relating to my attempts to re-integrate into the criminology of the United Kingdom. I do not think I made a very good job of these endeavours,

One of my first contacts on return was with the sociology department of the University of Bath. Before I departed for U.S. in 1966 I had talks about their awarding me an honorary degree. So, I thought, the climate might still be favourable. Bath university was fairly near and it seemed possible that I might be welcome to do some voluntary supervisions. I was not. Unpaid assistance was, as the trades' unions saw it, a threat to the rightful incumbents. This was not said directly but I thought that the encoded messages were fairly clear. Whether this was a misperception or not, I could interpret it only in one way. I had had the experience of Strathclyde years before! Bristol gave me a welcome, but I could not feel that I was “accepted” or only in certain roles. It seemed that my work was respected there and that respect extended to my person. I was for that reason entitled to be a guest of the department from time to time, but I did not feel a member of the department. This was mainly because no administrative backing was forthcoming at either Bath or Bristol. Without a parking permit visits were almost impossible, and parking permits were not for me. I gave the odd lecture at Oxford and a seminar or so at Cambridge. These were, I thought, token 'welcome back' gestures that set no precedent and need not be repeated.

At the time, I have to say, I was disappointed not to be welcomed back 'into the fold' and given a place in the work of 'doing criminology' in England. Upon reflection I can see the rationality of the way in which my approaches were dealt with. This was not the England I had left. Competition was writ large -- very large. It was “Thatcherism”. It was expected now that people would be (perhaps should be) exploitative. In that setting any offer of voluntary service to an organisation would enable that organisation to reduce its expenditure. If there were available voluntary supervisions, then the number of paid staff might be cut. It was not my intention to be such a threat to anyone's income, but I could have been, and the situation made it impossible for it to be otherwise.

Research on Return

I made overtures to the Home Office with a view to serving on one or more of the relevant committees. At the actual research level I was most warmly received, but I was given no official function. The operations side keep me in touch by sending me gratis copies of research reports.

I am still mystified by the fact that just before returning permanently to the UK I received an extremely warm letter from the Permanent Secretary, Sir Brian Cubbon. (The same as was concerned with the Schafer affair and victim compensation many years previously). An unrelated fact should also be noted. Around this time, the political climate for the Home Office Research Unit was becoming very hostile. The Unit was to be decimated, furthermore Chiefs of Police had raised objections (Times 9th June 1981), and though I did not then know it, the defamatory "Investigative Leads" to which I made brief reference in my story of Berkeley days had apparently been (re) published and was doubtless around in the department. It may be that this latter document's circulation at the time of my return was just a coincidence. If not a 'coincidence', then a 'conspiracy'? I tend to reject theories of conspiracy.

I do not think it is worth while to discuss this peculiar document further. It is so absurd. However, I have to acknowledge that I fear that much of the work of unofficial "Security Services" is of rather poor quality! I will merely reproduce a section of this publication as an Appendix. Readers may make of it what they will. While it is most likely that this publication was seen by (probably sent to) the authorities it could do little more than provide an unnecessary reinforcement of the existing view that I could not be relied upon to be supportive of the 'establishment'. The 'loose cannon' is unpopular. All civil servants accept the folklore of their 'guide to survival and promotion' which is said to state that on no account should one " make waves or rock the boat". The assignment to me of a 'boat rocking' propensity was not inaccurate and does not rely upon interpretation of any of my behaviour at Berkeley nor accusations in "Leads". Given that the 'top people' wanted a quiet life, I was not the person to take on board. Students be warned, policy research is no guaranteed safe haven for uninterrupted intellectual pursuit.

I can attest to the 'quiet life' preference of the permanent civil service from the time when I was an insider. It was during my Home Office days that a file was circulated asking for suggestions as to who should be recommended to the Minister as members of a proposed Royal Commission on law reform. I nominated Professor Barbara Wootton who had recently given the Hamlyn Lecture. Her name was removed almost immediately by my superior on the grounds that she was 'too radical' and liable to be a 'problem'. Barbara Wootton, while unsuited for a Royal Commission was nonetheless one of the first two women to be elevated to the House of Lords (created Baroness), and she was also to become Deputy Speaker of that upper chamber!

If, immediately after my return, there had ever been any doubt among the Home Office top brass that I was likely to 'rock the boat' if I was permitted to get on board, I quickly dispelled that doubt. My first assignment on my return gave them more than ample proof. I was responsible for more than a rocking -- it was more like a capsize!

The Social Science Research Council gave me a contract to make contact with all the criminology teaching and research in Britain and to assess its significance by comparisons with the field in the U.S. This was a revealing exercise. In short I did not find a happy situation. There were complaints of lack of freedom particularly in the publication of any research that proved critical of current policy. I was able to quote distinguished

academics without disclosure of their identity and my report was one which, I am sure, the establishment would have liked to suppress. Honest inquiry placed me -- a returned prodigal -- in the position of being the messenger with the unwelcome message. The results were in accordance with the proverbial treatment of those in this unfortunate role.

Perhaps if I had left these unpopular data and comment in a remote report the issue would have blown over. But I did not lie down and roll over quietly. In 1983 I published an article based on this project. It appeared in the Howard Journal. But it was placed in juxtaposition to one by the Home Secretary (then Brittain) in which he had set forth his enlightened programme. In my article, among other things, I strongly advocated a Bill of Rights and the contingency fee system for legal representation, both of which were strongly disapproved of by the government and the legal profession. Clearly this juxtaposition was an embarrassment to the Minister who could hardly have been unaware of it. To officials this was a cardinal sin -- one does not embarrass a Minister. Any advocate of such foreign ideas was unwelcome, but from somebody who had been 'one of ours' it was unforgivable. Once labelled, the label tends to remain. Files in government departments are like proverbial elephants.

Popularity in doubt

In too many ways I had 'queered my pitch' and I was not going to be fitted in to any available role. That was quite rational. I had no techniques for seeking popularity. Becoming a big shot was never my ambition -- I had to be realistic -- but being a 'loose cannon' is within my range. I am content with the role of irritant to the established way of thought. . It is a role which I rather enjoy and to which I am well accustomed. It seems that I am still managing to be disturbing, and I sincerely hope that among the disturbed dust there remain some useful ideas. In continuing to be active, though also quite ineffective, I am preserving myself.

I am still not going to be forced into real retirement. I can still write, even though no one may pay for it, and access to data will be restricted. I was lucky in that I retired at the time that the computer was being developed and becoming available at relatively low cost. The loss of secretarial, library and such facilities is minimised by technology.

I was asked to take on the Editorship of the Howard Journal, and was very pleased to accept this honour. In addition a good friend from times past at the Institute of Criminology arranged a 'visiting' status and I was accorded a site on JANET e-mail facilities. Now, thanks to help from Ken Pease with a reluctant modem, I am able to keep in touch with colleagues in the U.N. and U.S. and write materials collaboratively for publication overseas. My foreign standing is, of course, not impaired by my out-of-place behaviour in England.

One post-retirement assignment was, in my view, most significant, though it may be some time before the contribution comes to be appreciated. I was asked to be General Rapporteur for an International Group meeting held in Sicily and hosted by the

International Center which was to discuss 'situational crime prevention'. The convenors were a small group including Ron Clarke, Graeme Newman, Shlomo Shoham (Israel) and Ken Pease (UK). Ron Clarke who had been Director of Research at the Home Office had visited Albany while I was there. He had later left the UK to become Dean of Criminal Justice at Rutgers. His administrative role is significant but not, perhaps, as important as his research orientation. Ron and Graeme Newman (a colleague at Albany of long standing) with Marcus Felson and others have developed an approach to crime prevention which emphasises situational risk reduction rather than seeking to change the would-be offender. This approach is, of course, completely in accordance with my own prejudices and philosophy. The models I used in the Royal Air Force flying safety research programme fit remarkably well with 'situational' theory.

Papers based on this meeting are reproduced in a book in which I published a chapter developing the analogies between situational prevention measures and the R.A.F. system-based configurational analyses of flying accidents. [22]

As rapporteur I had to produce an official report. Among a number of recommendations was one which proposed that countries should consider setting up an organisation to study new developments and products with a view to predicting their likely misuse, such as by criminals. It was hoped that developers might be encouraged to attempt to inhibit any potential abuse with as much effort as they devote to advocating the use of new products or procedures. So far, of course there has not been much evidence of this recommendation being taken up, except perhaps in some states in the U.S.A. where 'environmental impact studies' have a somewhat related role. In the UK there have been encouraging developments in the Neighbourhood Watch scheme and crime prevention strategies are gaining credibility with the police.

Who said retirement?

I have concluded that retirement really has to mean retirement at least in one important respect. It is not desirable to try to do any voluntary work which is being done by paid employees. Voluntary work, it seems, should be confined to categories of activity which are not attempted by wage earners. It was some time before I realised that this was a large part of my problem on return to the UK. Once I had accommodated to this fact it proved impossible to find any activity which did not represent some potential competition with paid labour. It was not that in reality a volunteer would replace a worker, it was a matter of principle that worth should be seen in two distinct categories -- that which was proper for paid labour and that which was appropriate for volunteers. My activity as editor of the Howard Journal was of the latter type and represented no problem. My freelance writing seemed to be also an acceptable category. producing no threat to the number of available paid jobs. It is possible that volunteer status is different in the U.S. and U.K, and post-Thatcher years may also have seen a change here.

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22 Newman G et al (1997) Rational Choice and Situational Crime Prevention. Dartmouth.

I should have been more aware that a change of status to “retiree” was of social as well as psychological importance. I did not see myself as retired: as reclassified. I should have done. Training in retirement strategies is, I think, highly desirable. But an effective training programme must be based on research. I doubt that an adequate research base exists.

I made some attempts to continue with research and publication using much the same skills and techniques as heretofore. I had no primary source data so I made a few attempts by substituting secondary data. But 'number crunching' can be done much more effectively with larger computers than I had available. There was also the daunting problem of putting the data on disks.

So, I wrote two books which were mainly theoretical. I also wrote the odd piece for the Freethinker attacking current drug policy, and sent letters to editors plugging my pet theories, and riding my hobby-horses! I also lobbied members of Parliament.

There is a benefit to having no commitments imposed from outside and a sufficient untied income. One can be a complete free-lance. (? Is “freelance” old English for “loose cannon”?). It is not, in these condition, unpleasant to play a role of irritant -- it inoculates me against any tendency to feel irritated by others. If my views were acceptable they would not be challenging, and hence not innovative. When my views become acceptable I will be getting mentally old; then I may well vegetate.

CHAPTER TEN: TAIL PIECE

I would hope that my story has been occasionally amusing and more often informative. In addition, perhaps it has illustrated something of the interface between scientific procedures and ethical constraints and has shown that research teams need specific managerial skills if they are to be effective. I would like at this point to try to summarise my views on the ethical issues in a more general way, and finally to pass a judgement on my last career.

Ethical/religious concerns of advisers.

I am appalled that many, otherwise intelligent people, seem to think that religious organisations have any special authority in deciding upon ethical issues in any field, let alone that of the applications of the scientific methods. Priests, Rabbis and such certainly have an interest as do all intelligent citizens, and indeed even the scientist, by reason of his expertise, but none can claim any special authority in this area. Nonetheless I think there is a closer association between the philosophy of science and ethics than between ethics and any faiths. I think that my career has been very strongly influenced, if not determined, by ethical principles, but neither these principles nor my moral perspectives (or other activities) owe anything to any religion.

I see a clear distinction between ethical concerns which relate to research procedures in themselves (the how of science) and the ethical issues raised by its funding and utilisation (the 'why' questions). I am bound by the ethics of the scientific method when I engage in a project, but the question of whether I should undertake the work or whether it should be carried out at all, are quite different considerations. To say whether a line of inquiry should be funded and if so with what sums of money, we must turn to the principles of democracy until we find a better way. If it is right for me to work on a project is a decision only I can make, though clearly I will be guided in my decisions by whether I will be free and able to carry out the work with rigour and honesty. [23]

The ethical standards which have motivated my actions seem to me to be almost an integral part of the scientific method and to relate to the very behaviours required to make that method effective within its own boundaries. No scientist is permitted (by other scientists in his field) to forge, manipulate or misrepresent data. It is also unacceptable for a scientist to claim to have carried out work which was in fact carried out by others. These two principles of the scientific method do not derive from the commandments, "Thou shalt not bear false witness" and "Thou shalt not steal" but

23 Perhaps as much effort and money should be invested in trying to foresee any potential ill-effects (including those arising from spin-offs) of scientific work as is spent in finding ways of turning it to private profit?

clearly without the general acceptance of these moral principles, scientific work could not be carried out.

Perhaps even the call to have faith is unethical? It capitalises on the pathological demand for certainty which seems to be a universal plague of humanity. Faith promises to fill in those 'gaps' where we would have to admit that we were ignorant. I have to acknowledge that while probability can (usually) be accommodated quite simply within a scientific framework, it poses quite difficult problems of ethics in operations involving sentient beings, human or animal. But that difficulty must be faced and not avoided by inventing a simple way around.

Structure v. Content.

My objection to religion has nothing to do with the content of religious dogma or practice of any kind. It is self-evident that no two religions (by definition) agree on their content. When I talk of 'religions' I refer to a particular structure of sets of beliefs held by adult persons 'of sound mind'. Religions provide a 'package' which is, as it were, bought by believers. Religions call for the acceptance of explanations without rigorously tested evidence. Most religions offer rewards to believers such as attractive future states (usually after death). Marxism is probably not classed as a "religion", but it would fall within my classification by reason of its 'structure'.

By contrast the essential feature of the scientific approach is that it is open-ended and accommodates uncertainty. Science makes no claims to any revelation and does not ask for 'faith' in anything. Wonder at 'mysteries' is not ruled out, but mysteries challenge our ignorance. We do not need to fill in by attribution to some external authority. Religions call for deference to those who claim 'spiritual' leadership, and for the acceptance of that 'moral' authority, whereas science expects to be questioned. Intuition and imagination are highly valued, but they are not ends in themselves. The subjective satisfaction of the scientist is no criterion of the value of the work performed.

The ad hoc nature of policy research contracts

I have stated in many places that there are two independent sets of ethical concerns in relation to research, namely those involving the scientific procedures and those involving the applications to which a successful outcome might be applied. I fear that the layman often confuses these two quite different issues. Scientists should be held responsible for the ethics of their procedures, and they probably have a marginal responsibility for assessing likely utilities of the outcomes and warning of potential misuse. In this respect the products of scientific endeavour present precisely similar problems to those presented by any other invention or mechanism. Scientists might do well to devise and set up procedures which would carry out potential risk analysis in respect of their work. But they cannot stop the misuse of their results and, indeed, may be unaware of much of the potential for good or ill. Clearly it is absurd to ask that they restrict their work such that only 'good' may come of it.

Topic, Method and Structure of Field.

Most research workers, including myself, work on one project at a time, or at least one type of project. Each of my research projects has been separately funded and I have had to treat each topic of my research as self-contained. Usually it was only the statistical methods which were common to different problems. Comparing and contrasting a series of one's projects is a luxury of retirement. When I reviewed the preceding chapters I thought I could identify some integrating features. I think there is a 'theme' which derives from my statistical orientation; it is my concern for ‘structure’ in relation to uncertainty and risk. But I will review briefly.

First there was my engineering period which was also the main prior factor leading to flying safety research, second came my policy research work in the Social Survey with its economic forecasting, and thirdly the criminological research. It is clear that the topics researched differ, but less obvious are the fundamental differences in what I have come to call 'the structures' of the projects. I think that the idea of 'structure' is important and I confess that though I had implicitly taken it into account in my later research, I have nowhere given an account of it. I will now try to do so.

Methodology (which in my case was obtaining and analysing data) is one of three elements in policy research. The second may be seen as the content of the specific problem investigated and the third the implicit (or explicit) philosophy or stated purpose of the field of application. The three elements, each with their own constraints, must fit together if the research is to be successful. The only occasion in my experience that this issue was directly addressed was at the 'boondoggle' week-end preliminary meeting with the Federal Parole Board in Vermont.

Perhaps instead of the image of wearing a different 'hat' for different roles, one may think of a three-cornered hat!. It took me some time to recognise this tripartite feature. I was too enthused with the power of statistical methods. In my enthusiasm I saw no reason why statistics could not be applied to everything! I also failed to take into account that the central fact of uncertainty is not universally appreciated. Before I say more about ‘structure’ I should, perhaps, set out in some detail the statistical approach to dealing with uncertainty and the special use of the term *error*.

The problem with 'error' and uncertainty.

Much misunderstanding arises from the statistical meaning attributed to the word 'error'. To the layman, error is something to be avoided; to the statistician it is something which is integral with any research. Error does not mean 'mistake'. Indeed the statistician is more concerned with the measuring of error variance than in any of the other measurements involved in any design. If he cannot measure the error (probability) he cannot make reasonable statements about any other measurement. It is unfortunate that the legal and classical education of many senior executives and policy makers does not cover these points nor other simple points in the calculus of observations. Misunderstandings which arise from the essential element of probability

(uncertainty/error) often have serious consequences in the design and interpretation of research and its application in policy. Few understand the different methods of sampling and what these mean in terms of valid inference.

Research may reduce uncertainty or make it more manageable, but first uncertainty must be quantified and its function clarified in relation to the specific project. As I moved from one field of application to another, the function of error changed. This changing role of error, uncertainty and probability and my means of accommodating to it represents my *leitmotiv*. It is the major feature of my 'three-cornered hat'. I do not think that this feature is limited to kinds of research, but it may mean that some kinds of problems are embedded in systems which are incompatible with this essential concept. Attempts to carry out research where such a structure applies will fail. My learning is summarised in that statement. The requirement of compatibility over these three features is, I think, one of general importance. I will give a few practical examples of this compatibility before asserting a general theory. There were no difficulties in reconciling probability with problems which I encountered in my first field of application. Any improvement in flying safety was a 'good thing'. Planned maintenance specifically fully exploited error variance. Risk was not a difficult concept and there was no need to invoke subjective certainty.

With demand forecasting the requirement was not to estimate any 'truth' but to provide a basis for decision. It was the decision which mattered. There was no point in refining any estimate to a greater accuracy (or detail) than the available decisions could accommodate. The customer for the research knew the room for manoeuvre and hence could specify the degree of precision needed. The scientific nature of the observations, information and analysis could be discussed in relation to the actual decisions to be made. In virtual fact in these instances the research worker and the decision-maker were on equal terms in spelling out the rationality of the matter under review.

This level of cooperation also applied with the work on deafness. The research estimated the anticipated cost of providing, 'free at the point of delivery' hearing aids of a specified type to all who might benefit by their use. The problem seemed straightforward. I estimated demand -- the number of persons who were likely to benefit by the provision of an aid. Whether the cost was reasonable and provision should be made was a political, not a scientific decision.

The function of error measurement is most plainly evident in quality control, where the error acceptable to the customer is assessed in relation to the error acceptable to the supplier. These two types of error are conventionally referred to as 'producer risk' and 'consumer risk' or technically as 'errors of the first and second kind'. The same kinds of 'errors' in medical research are termed 'false positives' and 'false negatives'. Unfortunately the word 'false' again has undesirable evaluative overtones. The figures relate to probabilities which are an essential feature of the scientific method. No one can rectify that 'falsehood', and indeed for clinicians to try to do so would almost certainly make the situation worse.

Statistical research which did not measure error or did not openly discuss its implications for policy would, in my view, be unethical -- a point I have had to make to authors far too frequently, particularly when serving as editor or reviewer for journals in the field of criminology. It is, of course, reasonable to set up procedures for dealing with cases where errors occur. An example in medicine would be for significant compensation to be paid where inoculation with its beneficial results for thousands necessarily involves the unfortunate rare adverse reaction. ('false positive'). This would fit with the more general concept of 'social contract' in that it may be seen as balancing on the one hand the civic duty to have inoculation and on the other society's liability to assume responsibility for risk. Ethics do not conflict with statistical philosophy which accepts uncertainty (probability) as a fact of life.

With separate projects the question of how to deal with error is specific to the decision relevant to each study and this fact simplified the presentation of many instances in my career. In the medals forecast, for example, it did not matter how large was the 'error variance' of the estimate of any individual's likely action, though the method necessitated its estimation. This is because the decision whether to establish the out-station in Wales was made only on an aggregated figure, and the aggregate was estimated from the averages for each type of medal. Averages are remarkably robust! In this case I did not even seek to minimise individual error, but only to ensure that it was not biased.

A similar situation applied with the studies of de-rationing. The task was simply to provide an estimate of the market price (an average), and this estimate was the basis for the policy decision. I did not decide to de-ration or to continue rationing, I derived an estimated price or quantity likely to be demanded. In the case of deafness, the error variance was related to the provision of several estimates which depended upon postulated future policy or technological developments.

For all cases where I was concerned with de-rationing and estimates of demand or future likely prices, I could not use any available aggregated data covering the period of rationing because it was just those conditions which might not shortly apply. It was, therefore, methodologically essential that the methods derived an estimate of the relevant behaviour of each individual sampled. The magnitude of the error variance of these individual estimates was again of no importance so long as they were unbiased. It was the likely error of the overall estimate which was relevant.

The situation changed with the application of essentially the same methods to the publicans in the Carlisle Scheme. Now the estimates for each individual were to impact upon that individual. Error variance then had a quite different meaning. Similarly the methods applied to interviewer performance were subject to error variance. In these cases, procedures had to be designed to deal with the error feature. In the case of the Carlisle publicans, where the suspicion was significant, it was tested by normal police methods. With interviewers, those who presented a suspect profile were inspected by supervisors. In other words, procedures, appropriate to the error variance (uncertainty) were built into a total scheme. In all of these cases the idea of error created no special problems -- we were not making 'mistakes', and our uncertainty was accepted and managed intelligently.

I do not think that I realised at the time that the ethical issues changed with the different structures of the three types of research design. Now it is clear that with the medals forecast any carelessness on my part would not have had any impact on any individual in my sample of respondents nor upon those they represented. (All who wanted their awards would have received them). With the hearing aids research I had greater responsibilities to my informants and to those they represented in that the research assigned individuals to categories of hearing loss, and these categories were the basis for subsequent decisions. However, the impact on individuals was indirect. In the Borstal research assignment to risk categories had a direct impact upon individuals to the extent that the risk was taken into account in any decisions.

Is 'prognosis' sufficiently similar to estimation?

My transition to criminology was predicated on the similarity of the methods I had used in demand forecasting with the so-called 'prediction work' of legal scholars in the United States. The expert criminologists to whom I was introduced apparently thought that estimates of the probability of recidivism would be useful. These were referred to variously as 'prediction' or 'prognosis'. Indeed many professionals concerned with offenders claimed to be already making prognoses. It seemed self-evident to me that statistical methods could provide more accurate estimates of the risk of recidivism than subjective assessments by probation officers, prison governors or even psychiatrists. This was not an unsupported claim. Meehl had made comparisons between some hundred published pieces of research and found that the methods which he called 'actuarial' were able to classify accurately a greater proportion of individuals than 'clinical' predictions could. I did not realise, at that time, coming as I did with no concepts of 'clinical insight', that those who made clinical predictions were doing so without measuring or even subjectively assessing their errors -- of first, second or any other kind.

Presumably various professionals in the criminal justice field might use any 'prediction tables' which might be worked out, and it is possible that some judges in sentencing were influenced by the estimated 'risk of recidivism'. Though in what way, was again uncertain. I was satisfied that though some of these decisions would be in error the total number of errors would be less than had the assessment been based on clinical prediction. My belief that though there was error in clinical judgements, the error in statistical estimation would be less and my ethical justification was in the fact of the improvement. I accepted the situation up to the point of my intervention. I had no remit to challenge other elements of the judicial system. These restrictive terms of reference defined the range of this and other policy research. Indeed I made no claims to provide support for any general theory of crime or punishment during the whole of my Home Office career.

It was much later when exposed to the political climate in the United States, specifically through having to learn something of Constitutional Law that I did venture into some general philosophy of jurisprudence. Until then each project was an *ad hoc* study with a particular purpose and in my reports I addressed the limitations as specifics of each

project. I began expanding my activity first by publishing illustrative worked examples of how the method of discriminant analysis devised for the Borstal commission could be used as a general quasi-experimental design. Simply this meant that the risk estimates (derived from the 'predictions') enabled the 'intake' to institutions or treatment modalities to be assessed in terms of the initial 'quality' (i.e. 'risk'). If one intake of inmates were of higher risk than another, then one would not expect equal outcomes from an identical procedure. If the kind of young men sent by the courts to Borstal became more inclined towards hardened offenders, their risk of recidivism would indicate how the 'success rate' for Borstal might be expected to drop. Thus, in general, expected outcomes could be calculated and related to observed outcomes. This was a reasonable use for the tables. The three dimensions were concordant, or, pressing my metaphor, the three corners of the hat were of the same colour! Error was dealt with in the levels of confidence in observed difference

After some years the success rate for Borstal did drop, and politicians (and indeed some others) were convinced that this could only mean that the system was failing and that a more severe routine was required. It is a possibility that the publication of the Tables had enabled judges to identify the better risks that previously might have been assigned to Borstal and to find alternative less punitive disposals. In such a case the expected rate of success could have accounted for the drop in actual success. If anyone did calculate the expected drop due to higher risks being committed it did not convince the authorities. Borstal Training was abolished.

Situations in criminal justice where error variance can be accommodated within the existing philosophy are few. This is because the statistical measurement of uncertainty is not accepted. Both the judiciary and the public are more willing to trust those who assert certainty.

Avoiding probability.

One theory of criminal law which it is thought logically avoids concern with probability is termed "Just Desert". This justification for sentencing offenders asserts that it is neither necessary nor morally justified to consider " what he was likely to do". Probability, it is claimed, does not apply because sentence would not be pronounced unless the historical facts were established beyond reasonable doubt. I would think that on investigation probability and error might be found in the establishment of the historical facts, but I will not press that point.

I described the processes developed for Parole Guidelines in the course of my narrative. I now realise that my statistical background coloured my attitudes as well as my procedures. There were viewpoints which I may have too lightly dismissed. It is, it seems, necessary for decision-makers concerned with values to be subjectively certain of their rightness. Subjective certainty is recognised in statistics by a level of probability such that it is rational to behave *as though* the matter had been proved. I think I can

concisely state my position by saying that I accept the philosophy of *as if* (Vaighinger) [24] but with certain adaptations to accommodate the hypothetico-deductive method.[25] My difference with Popper was in respect of his treatment of 'error'.

Decision theory offers a considerable number of techniques and even philosophies. But none have made much appeal in the field of crime and punishment. There are various models which offer ways of rationalising the acceptance of a tentative belief as satisfying and operationally true. But we cannot discuss a level of probability required for the acceptance of a moral or religious statement. If one wishes to believe that an action is 'right' that belief cannot be conditional on any estimates of likelihood. The rigidity of this philosophical base does not map with the model of evolutionary process. Those religions which rejected Darwinism are at least self-consistent -- the two systems are structurally incompatible. It will be recognised that this kind of issue underlies the difficulties we are currently experiencing with the rationale of international law and the concept of war crimes. It is not possible to open up this matter here. I note the problem only to illustrate the progress of my own thinking. Research and research management cannot be insulated from policy formulation. If research methods are to be used in decision making the research should be integrated with the decision structure. This has to be team work.

Avoidance of involvement in parole policy was initially possible and, naturally, I made use of this fact. Statistical guidelines could work with parole decisions because these decisions were tentative and their logic was an acceptance of precedent with a requirement of 'fairness'. My research provisionally accepted these cultural values and built in a system for continuous monitoring. There was always the challenge to change, but to change in accordance with stated principles which were themselves open to challenge. The system was dynamic, there were decision rules and defined procedures for considering breaking/remaking those rules. The fluidity was unpopular with legislatures who preferred to have only rules -- the 'rule of law'. Perhaps the model could work only because the decision makers were a small group, though their remit was nation-wide. It was also very important that there was (thought to be!) a sufficient degree of agreement on objectives.

When guidelines became popular they also became political. The interest of Senator Kennedy and others and the move towards a standing Sentencing Commission for the Federal government meant that the power of research personnel was lost to the lawyers and politicians. Of course, the lawyers thought they were improving the system by making it more 'positive'. I did not see any way other than to leave what remained of the system with others. By this time I was beginning to disengage from research concerning offenders.

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24 Vaighinger M (1924) *The Philosophy of As If* Kegan Paul London

25 Popper K. Conjecture and refutation

Punishment as a scarce resource.

Policy in the criminal justice field differs in one very important respect from all other aspects of governmental and social policy. "Doing justice" does not permit of any constraints. If a decision is "just" it is also just right and not open to challenge, certainly not on grounds of cost. If prisons are overcrowded, then the prison system must adjust to meet the "need". The courts, that is the "justices" must, they claim, focus their undivided attention on the appropriate sentence for the individual. There could be no such thing as a punishment budget. I fail to see any reason why courts should not be constrained by limitation of resources in the same way as health, education, the military or even the police are constrained. Why, of all social resources, is punishment seen as completely unbounded while taxation casts its shadow over all other government activity! Would it not be reasonable openly to consider the rationing of punishment? If the justices would themselves take a wider view, a form of self-regulation, might be possible.

Should criminology survive?

If the scientific method is powerless to sort out the appropriate basis for the punishment of offenders, does 'criminology' have any meaning? Am I correct in assuming that the central concepts which determine the definition of 'crime' and hence of 'criminology' are 'blameworthiness' and 'culpability' ? These constructs are beyond the reach of direct research designs. Is academic criminology an appropriate discipline to sort out these philosophical issues? Maybe or, perhaps, positions defined by other disciplines on these points should be taken as axiomatic ? The boundary between civil and criminal law is not firmly fixed and many take the view that certain acts for which criminal sanctions are provided are not crimes because persons who engage in or practice them should not be punished. Perhaps criminologists should function as a 'broad church' on these matters? This seems to be the present position. In different universities criminology tends to take on its own identity and study topics of its choice. This diversity had some merit when the field was small and developing, but now there is a serious problem. If the idea of crime is accepted as a term having any weight in the meaning of 'criminology', what may happen if members of 'criminology' departments extend their interest to 'harms'? Studies which ignored the concept of blame might be subsumed under 'crime prevention' but the methodology and kinds of models required are different. The different orientation is quite fundamental.

My views as to the utility of the various representations of criminology have turned more negative in the course of writing this work. Immediately prior to starting I summarised my views in the document reproduced below.

After considerable thought I have concluded that it is undesirable to state policy aims in terms of 'goals' or 'objectives'. Rather they should be stated in terms of preferable 'directions'.

While I acknowledge the risks of simplification, below is an attempt to summarise the main inferences from my research into incarceration as they might impact upon future criminal justice policy

Policy directions should:-

SEEK TO MOVE AWAY FROM		SEEK TO MOVE TOWARDS
Concentration of power.	TOWARDS	Extension of participation in decisions to all those involved in the event.
Blame allocation, with emphasis upon accusatorial processes: punitiveness.	TOWARDS	Negotiation and conflict resolution methods.
Symbolism, ritual, drama and display.	TOWARDS	Economic analysis and use of managerial techniques with good communication
Moral posturing and punishment.	TOWARDS	Conciliation and restitution
Concentration on actions after the event. (Person based.)	TOWARDS	Concentration on actions before the event. (Situation based.)
Assuming all good intent must have good outcomes.	TOWARDS	Considering all social policy for possible undesirable side-effects.
Legislation based on intuition of persons in authority.	TOWARDS	Legislation based on research findings and oriented towards quality of life.

Table 1. My views on social policy, Cambridge March 1994.

I still accept all of the points presented in the diagram but I would emphasise the difference between approaches which relate to actions regarding offenders or persons found guilty and actions regarding crime prevention.

Prisoner of traditional thinking?

Reviewing my career, I think I now would say that I became a prisoner of traditional thinking. My adventure into criminology began as an enforced march, but I continued to see myself as a statistician concerned with public policy. I remained for some time unaware of the arguments raging around whether the law's definition of a crime was to be accepted as the boundary of study for the academic base for 'criminology'. When I became aware of them I thought they did not concern me. Now, despite the awards by official criminological organisations, I am still more than uneasy about the enforcement of morals. I find it increasingly acceptable to claim that society has to manage the social system in such a way as to accommodate all kinds of persons *as they are*. When the philosophy of law moves towards ideas of 'people-changing' it becomes far too untidy to be dignified as an academic discipline. No measures which have attempted to reduce crime by dealing with offenders have shown much success. Perhaps it is a good thing that people-changing is not possible. If methods become known whereby 'bad' people could be made 'good' as easily as some seem to claim, then it would have to be equally easy to turn 'good' people into 'bad'. Scientific power does not have an inbuilt ethical dimension, nor is moral posturing a useful managerial tool.

I concede that the individual or collective committing a crime will have to be dealt with, but as a matter of satisfying public demand rather than of social control. I suppose that there will always be some persons who cannot or will not accept the constraints of a society, and these persons will have to be dealt with by some politically accepted means. The detection, arresting, trial and disposal of offenders has its impact on general public mores, but in a way which is poorly understood and little researched. Should we not cease trying to persuade ourselves that any form of acceptable punishment ritual will reduce the probability of future reprehensible acts?. The problem, perhaps for criminology, is to ascertain the minimum intervention that will provide sufficient public satisfaction such that the risk of people 'taking the law into their own hands' is minimised. Scientific methods could help with measurement and provide scenarios to facilitate decisions.

Society has become extremely complex and we cannot match contemporary complexity with naive simplicity. The public must be informed. Where the degree of complexity is such that only trained, experienced experts can appreciate that complexity, the public must be able to trust the decision system. In sorting out what might be done in this sector the ethical issues will be progressively both more important and more difficult.

This class of problem may well remain within the ambit of criminology which, with its links with law, psychology and sociology, may be the best mediator. But this work will not relate in any way to other concerns about reduction of crime or minimising of harms. The orientation of the one is almost independent of the other; philosophical constructs which may provide models in the one will not apply in the other. Can the two independent approaches live together under one academic umbrella? I have come to think not. This raises a major issue and I can do no more here than present a few

thoughts, or as they say in advertising "Let's run it up the flag pole and see if anyone salutes it". [26]

The separation out of concern for cause and blame allocation from concern with remedial measures for harms however caused, brings ethics into a far more important role than heretofore because the interface of the issues with scientific methods would also be clarified. With this approach, ethics will become concerned with the research methods and with issues of risk and uncertainty, as well as systems of control. Matters of individual morality will remain where they were previously, namely, with ideas of crime and punishment. The impact of the change will be wide-ranging. For example, there will be an emphasis upon designing trustworthy practices of decision making, (rather than concern for the trustworthiness of decision makers); dramatic incidents will not demand disproportionate attention; the 'unit' of reference will be the structures rather than the individual; there will be no need to feel certain of being right, but only of minimising risks of harms; the nature and quality of an act rather than any quality of the actor provides the basic data, with the incidental advantage that complaints of 'crimes' which do not result in a ''clearance' will also provide data. Data on successful criminal acts will need a different skill from data collected with the sole aim of identification of the offender.

To achieve adequate development of the issues we now see as "crime prevention", I suggest requires it to be recognised as an academic study in its own right. A department of 'Public Safety Policy' would seem to express the right coverage. This, like criminology, would be an umbrella discipline that could co-ordinate research from sundry sectors and interpret in terms of policy recommendation, carrying out its own research into effectiveness and the changing nature of public demand. From an academic viewpoint, the advantage of this classification is that it is positive -- i.e. promoting safety, whereas crime prevention is essentially negative. Some further advantages would be the relationship afforded with other academic disciplines and international organisations. There is no international criminal law, and the United Nations places criminology within "Social Defence". The field of crime prevention I have in mind would encompass much more variety than any existing category. The term 'public safety policy' which I propose seems to be very similar to the United Nations, and it may seem unnecessary to put forward any other descriptive title. I have one difficulty with this, namely that the definition of Social Defence, as embedded in so many official and unofficial documents has ossified into a legalistic concept of the enforcement of law and order. The Helsinki Institute recently went somewhat further and pointed out the restrictive aspects even of the concept of 'crime prevention', proposing that what was required were techniques of crime *anticipation.*[27] I agree completely. The difficulty seems me to be not in any weakness in the concepts but in finding the necessary slogans and obtaining the support of public figures. The statistical indicators of the incidence of crime are unsatisfactory because comparative studies are prejudiced by differences in national legislation.

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26 Or, less reverently, "walk it down Bond Street and see if it gets picked up"

27 Leppa S (1999) Anticipating instead of Preventing. Heuni Paper No: 11 . (http/www.vn.fi/om/heuni

Broader based indicators, such as those relating to the "quality of life" might be further developed.

We could surely apply "prediction" methods to the identification of potential criminogenic features in the design of new products and existing situations and "design out" crime. We could develop and exploit simulation techniques and scenarios and try to invent systems which have controls inbuilt so that in simple terms "crime" did not "pay". The theory of games, decision theory and personnel strategies provide many possibilities for exploration, and we might develop means for anticipating crime. [28] A category of studies relating to public safety policy (PSP) could replace the individualised moral perspective of what is right or wrong with a broader-based concept of beneficial performance. Defining 'beneficial' is close to defining 'right' and 'wrong'. However it has at least one important difference. The former is dichotomous and usually related to a set of beliefs outside the scope of scientific inquiry, whereas 'beneficial' may be graduated and has a potential of socio-political definition. At present criminal law provides satisfaction to the law makers and perhaps to the law abiding, but it does not achieve its stated purpose of preventing crime.

Few criminals are geniuses, and if they succeed may we not hypothesise that the designers of the reward or control systems were less clever or gave their task less attention than those offenders who exploited it? This specification may be unpopular with authority because it increases the type and level of public accountability. If the pilot can be blamed for the crash, perhaps there is no need to examine the aircraft structure? If an individual can be blamed, the system may be exculpated and the problem is simplified and, perhaps more importantly, dramatised. That may be too cynical... but...

Put in the most old fashioned and simplistic language, I might say that I now advocate less concern with individual " sinners" and more concern with "temptation".

28 But we must guard against this forward looking orientation turning to suggestions that we should seek to anticipate the criminality of would-be criminals.

APPENDIX 1: LIBEL IS IGNORED.

I promised that I would say more about the mysterious defamatory statements made in the publication "Investigative Leads" published in August 1981, reproduced in Figure 13.

First; the evidence. I have retained a few items to indicate the kind of material included in the report, the majority I passed to the Tavistock Institute, because, being similarly accused of anti-governmental activities, they might have wished to take legal action against the authors and publishers. Their decision was that to take action would only give more publicity to the authors than could achieve by circulating a poorly produced pamphlet. I agreed.

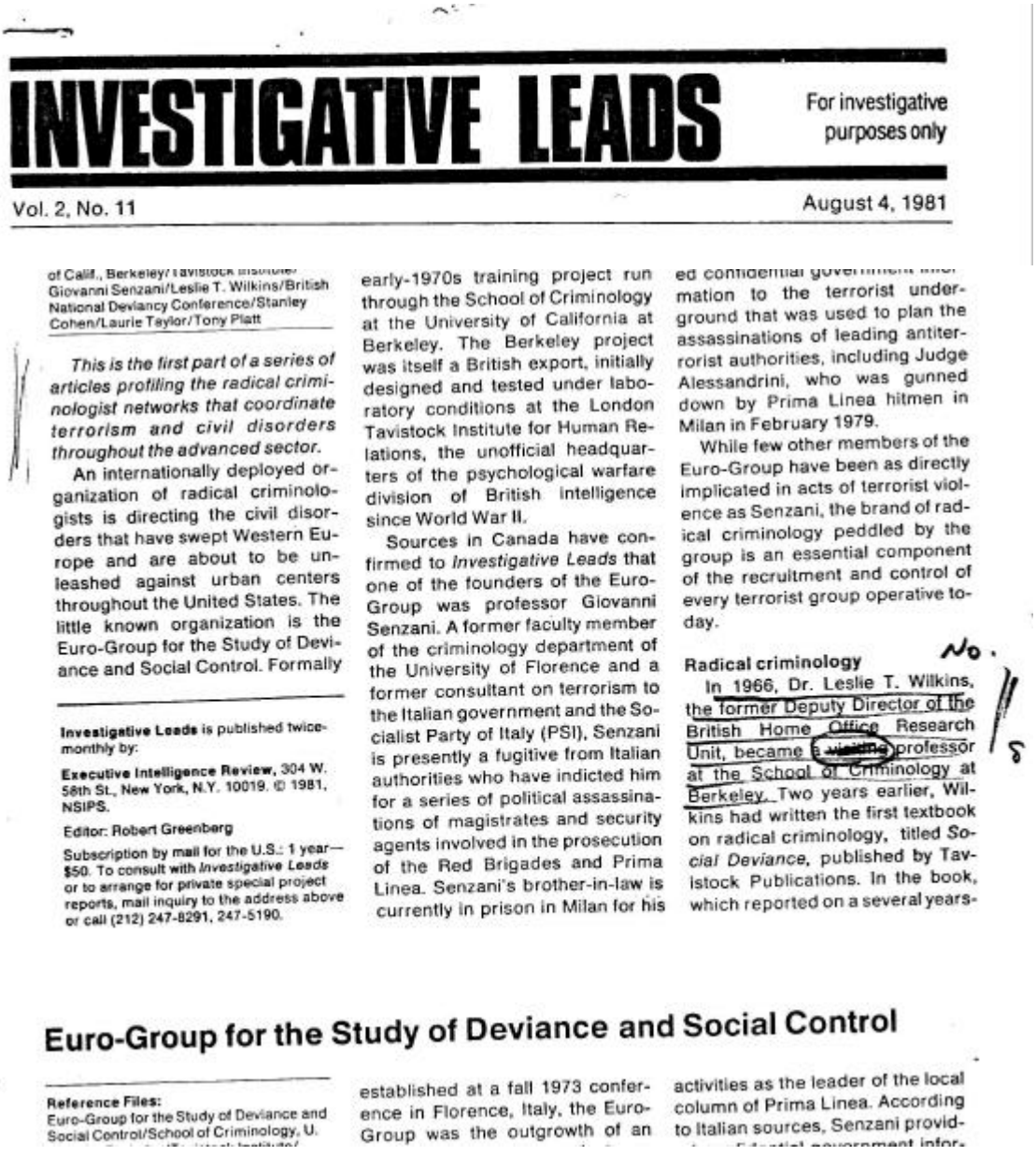
I find it intriguing to speculate on the authorship of the "Leads", and particularly on the date of publication since the events noted were not even claimed to be recent. It may be significant to point out that at this time (1981) criminology was seen as a subversive activity by certain (right wing?) Groups, and the Home Office Research Unit was particularly under attack. I select one cutting in support of this position for reproduction. But is a document relating to Berkeley relevant and why are British institutions such as the Tavistock Clinic attacked? While the myths relate to Berkeley in past times, the source o f this libellous publication would seem to be Britain. There is strong internal evidence of this. There are many simple errors which no writer in the U.S.A. could have made. For example, it is stated that "in 1969 Wilkins became acting Dean of the School of Criminology at Berkeley". (For the record, I ceased to be Acting Dean in the summer of 1969 and I left Berkeley in the Fall of 1969 and was at Albany from then until retirement. No one with the least knowledge of the field or with access to such reference books as "American Men of Science" or membership lists of either of the two U.S. learned societies would make that mistake. While I am not in the British Who's Who there are many simple opportunities in the U.S. for such facts to be checked in various reference books. In Britain it would not be easy to get confirmation of dates and other biographic data. The document also lists members of the British National Deviance Conference which was a quite small (and I would say insignificant) group of British criminologists of sociological (rather than legal) backgrounds, and most of these persons were not well-known in the U.S.A. The linked attack on the Tavistock Institute also seems to be a 'domestic' matter. But even in this case there is some sloppiness since there appears to be some confusion between the Tavistock Press (a branch of the law publishers Stevens) and the Institute which are totally unconnected.

I do not deserve the credit given in this publication for being the "founder of British Society of Criminology" (well !!), and that I was "affiliated with Tavistock Institute", which is even less true. The preponderance of evidence is that the document was written by some Britisher who had some associations with contacts in the United States.

I was as unpopular with the 'right' as with the 'left'. I had set down my approach in “Social Deviance" as tolerance, which could be adapted to inform policy on deviant

behaviour. My views on drugs may have been assumed to have persisted and there are many who benefit from this trade -- on both sides of the law! I find this matter puzzling but only slightly interesting -- certainly not interesting enough to take any time to seek further information which could sort out the issue. It merely seemed necessary to give some background to publications which, though not `in the public interest' are after all, a matter of public record.

I cannot believe that this experience indicates the quality of 'intelligence' which informs the anti-terrorist activities of the armed forces. I certainly hope that it is not! So, I think I will say that the 'case is closed' . It matters naught; it's probably not even worth this appendix.



INVESTIGATIVE LEADS

For investigative purposes only

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of Calif., Berkeley/Tavistock Institute/Giovanni Senzani/Leslie T. Wilkins/British National Deviancy Conference/Stanley Cohen/Laurie Taylor/Tony Platt

This is the first part of a series of articles profiling the radical criminologist networks that coordinate terrorism and civil disorders throughout the advanced sector.

An internationally deployed organization of radical criminologists is directing the civil disorders that have swept Western Europe and are about to be unleashed against urban centers throughout the United States. The little known organization is the Euro-Group for the Study of Deviance and Social Control. Formally established at a fall 1973 conference in Florence, Italy, the Euro-Group was the outgrowth of an early-1970s training project run through the School of Criminology at the University of California at Berkeley. The Berkeley project was itself a British export, initially designed and tested under laboratory conditions at the London Tavistock Institute for Human Relations, the unofficial headquarters of the psychological warfare division of British intelligence since World War II.

Sources in Canada have confirmed to *Investigative Leads* that one of the founders of the Euro-Group was professor Giovanni Senzani. A former faculty member of the criminology department of the University of Florence and a former consultant on terrorism to the Italian government and the Socialist Party of Italy (PSI), Senzani is presently a fugitive from Italian authorities who have indicted him for a series of political assassinations of magistrates and security agents involved in the prosecution of the Red Brigades and Prima Linea. Senzani's brother-in-law is currently in prison in Milan for his activities as the leader of the local column of Prima Linea. According to Italian sources, Senzani provided confidential government information to the terrorist underground that was used to plan the assassinations of leading antiterrorist authorities, including Judge Alessandrini, who was gunned down by Prima Linea hitmen in Milan in February 1979.

While few other members of the Euro-Group have been as directly implicated in acts of terrorist violence as Senzani, the brand of radical criminology peddled by the group is an essential component of the recruitment and control of every terrorist group operative today.

Radical criminology

In 1966, Dr. Leslie T. Wilkins, the former Deputy Director of the British Home Office Research Unit, became a visiting professor at the School of Criminology at Berkeley. Two years earlier, Wilkins had written the first textbook on radical criminology, titled *Social Deviance*, published by Tavistock Publications. In the book, which reported on a several years-

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Euro-Group for the Study of Deviance and Social Control

Reference Files:
Euro-Group for the Study of Deviance and Social Control/School of Criminology, U.

long social profiling project in London's maximum security prisons, Wilkins argued that violent crime is a "sociological phenomenon" caused by the injustices committed by the state. Arguing that criminals are merely individuals who deviate from the social norms imposed on society by racist and repressive governments, Wilkins argued that society must become more tolerant of its classical criminals and focus its attention on overthrowing the repressive state.

In 1969, Wilkins became the acting Dean of the School of Criminology at Berkeley.

Simultaneously, a group of Wilkins' collaborators created the British National Deviancy Conference. The BNDC emerged from a 1967 experiment at the maximum security wing of Durham Prison, conducted by Stanley Cohen and Laurie Taylor. Using profiles of prison gangs gathered in the course of the Durham Prison project, Cohen, Taylor, Jock Young, Ian Taylor, and Paul Walton founded the British Deviancy Conference in 1968 as a vehicle for applying the prison social control methods on the general population. All of the founding radical criminologists of the BNDC became regular contributors to the British radical journal *Anarchy*. They simultaneously established a string of community-based insurgency groups in London, including the Squatters, the London Street Commune, Cas-Con (an organization of radical social workers committed to "revolutionizing" street criminals and prisoners), Radical Alternatives for Prison (one of the first prison halfway house experiments), P.R.O.P. (a radical prisoners union), and the *Red Rat Journal for Alternative Psychiatrists*.

It was out of this nexus of radical social experiments that every significant British anarchist-terrorist group—most notably the Angry Brigades—was forged.

In 1974, Taylor and Cohen published their sequel to the earlier Wilkins book under the title *Deviance and Social Control*. The Taylor-Cohen book was published by the same Tavistock Press.

In the United States during 1969-73, Berkeley Criminal School Dean Wilkins recruited a group of radicalized graduate students into a project paralleling the British National Deviancy Conference. Among those students was Giovanni Senzani.

Wilkins' principal protégé during the Berkeley period was Tony Platt. An Oxford University trained British national, Platt was brought to Berkeley in the mid-1960s. After spending three years in Chicago in the late 1960s profiling street gangs on a $100,000 grant from the Ford Foundation, Platt returned to Berkeley where he became involved under Wilkins' tutelage as a behind-the-scenes controller of the Oakland Black Panther Party. According to Platt, "It is the obligation of intellectuals to both develop theories of oppression and exploitation and to participate in the process of transforming society. Criminals have written better books on crime than criminologists. Take for example the work of Eldridge Cleaver, Angela Davis, Sam Melville, and George Jackson."

Platt's intimate knowledge of the Black Panther Party (BPP), the George Jackson Brigade, and other West Coast early-1970s terrorist operations was the result of his active role in creating and deploying these groups. Platt protégé David DuBoise, for example, was installed as the editor-in-chief of the BPP newspaper. Other Platt associates, operating off of the initial efforts of the British National Deviancy Conference, established the San Francisco Bay Area United Prisoners Union, a joint prisoner-social workers cell that helped found the Symbionese Liberation Army at Vacaville Prison in 1973.

The Euro-Group

In fall 1973, six radical criminologists from Berkeley and the London-based BNCD sponsored the founding conference of the Euro-Group for the Study of Deviance and Social Control in Florence. In addition to Tony Platt, the other sponsors were:

- Stanley Cohen, a South African who received his P.D. at the British Fabian Society-run London School of Economics and who co-founded the BNDC;
- Margherita Ciacci, a graduate of the University of Florence who received her Ph.D. at the Berkeley School of Criminology;
- Karl Schumann, a leading West German radical criminologist associated with the University of Bremen and with the leading antipolice organization in the Federal Republic, Arbeitskreis Junger Kriminologen;
- Laurie Taylor, secretary of the British National Deviancy Conference during 1968-74 and a criminology professor at York University;
- Mario Simondi, secretary of the Euro-Group from its founding through 1978, a Berkeley Criminology School protégé of Wilkins, now a professor at the Senzani-contaminated University of Florence.

Reflecting the insurgent profile of the Euro-Group, the proceedings of the first conference were published with a dedication to the inmates of Europe's maximum security prisons.

Figure 13. Reproduction of a page from an intelligence report published in 1981, obtained through friends in Berkeley, making false claims about events that occurred 20 years previously.

APPENDIX 2:

Publications in date order.

Books are in capitals, monographs in text with underscore, chapters in edited books in italics, others are articles in journals.

Certain other important work is also noted.

1941	(first article accepted for publication)	
	Production and Overtime	The Factory Manager v 9 p 248
	A Statistics Department?	The Factory Manager. v 9 p 46
1946	"Happy Landings" Statistical Tables and charts in Nos: 3,7,8,9,10 (Photograph/uniform # 8)	Air Ministry Restricted
	Chairman of PA2(b) Safety Sub-Committee *Minutes contain research materials*	Air Ministry Restricted
	Air Errors of Traffic Control	Air Ministry Restricted
	Inexperience in Flying Accidents	Flying Accident Digest # 3
	Pilot Experience and error	Flying Accident Digest # 6
	Editor and contributor of whole issue of Flying Accident Digest # 7	
1947	Surveys can Save Lives	Survey Journal 2(4)
	Interviewers' Guide	Social Survey
	Opinion and Attitude Surveys	Social Survey M 13
	New Statistical Methods	"Target" (C.O.I. publication)

1948	Prevalence of Deafness	Social Survey Report
	Demand for Campaign Stars and Medals	Social Survey Report
	Interviewers' Guide (revised)	Social Survey Manual
1949	Road Accident Publicity	Social Survey Report
	Incentives and the Young Worker	Occ: Psychol: 10. p 1 - 12
	Prevalence of Deafness	Paper to International Audiology Conference 19.7.49
	Planning a Social Survey	Psychol at Work 2 (4)
	Interviewer Efficiency	Social Survey M 28
1950	Incentives and the Young Worker (a more extensive analysis)	International Journal of Opinion and Attitude Research 4(4) p 541-63
	Evening Class Wastage (with K. Smith)	Voc Asp of Ed 2(4)
	Survey of Knowledge and Opinion about Economic Situation	Social Survey Report
	Noise in Semi-detached Houses	Social Survey Report No 161
	Uses of Fuel	Social Survey Report
1951	Estimated Demand for Solid Fuel	Social Survey Report
	County Colleges, demand for	Social Survey Report
	Adolescents in Britain	Social Survey Report

	Stationery demand and supply in government offices	Social Survey Report No: 168
1952	Design and Analysis in Prediction Surveys (with D. Lamberth)	Social Survey M 55 Read International Conference of ESOMAR/WAPOR
	Estimating the Social Class of Towns	Applied Statistics v 1 (1)
	Mass Media Reports (signed ABH)	Operational Research Q 3 (4)
1953	Demand for Medals	Read: Royal Stat Soc (see Figure 3)
	A Note on Intensity Measurement (with A Harris)	Incorp Statn: 4(1)
	Practical Applications of Recent Developments in Statistical Methods	Paper Assn Incorp Stat 29 May 1953
	Practical Applications of some recent Developments in Statistical Methods (with J Fothergill)	Incorp Stat: 4 (2) p 92
1954	Interviewing for Social Research	Personnel Management
	Royal Commission on Taxation Appendix is my research report.	Second Report Cmd 9105
	Constructing Criminological Prediction Tables.	Brit Assn Ad Sc. Sec J Oxford 3.9.54

1955	PREDICTION METHODS IN RELATION TO BORSTAL TRAINING: - : (with H. Mannheim) reprinted 1965	H.M.S.O.(London)
	Symposium on Prediction Methods. (with discussion)	Brit J. Criminol 6 (2) p 82
	Some Developments in Prediction in Applied Social Research	B.J. Sociol p 348 - 363 as read 6.6.55 with Barbara Wootton in chair
	Analysis and Interpretation (with J. Fothergill)	Incorp Statn: Apl p 1 - 25 (special number)
	Classification and Contamination in Approved Schools	The public release of this work was banned by Home. (It originated the idea of regression to the mean in group dynamics)
1957	*Unpublished Statistical Materials*	*(in) Staveley E. (pp 404-410) ASLIB, London.*
	Use of Technical Information (with L. Moss)	Social Survey Report and summary in Nat: Phys: Lab Paper # 16
1958	A Small Comparative Study of the Results of Probation	Brit J. Delinq: 8(3) p 210
	Methods of Exploration in Social Research	R.S.S. Conf Papers (Cambs)
1959	Probation Officers' Work A Time Study	Home Office Mimeo
	Operational Research and Admin.[n]	O & M Bull: 14 (6) p 250 5

	Who Does What with What?	Research Newsletter (Sacramento)
1960	DELINQUENT GENERATIONS	Home Office Report # 3 H.M.S.O.
	Interpreting by Number	Engineering 189 p 59
	Criminology: An Operational Research Approach	*(in) Weldon and Argyle SOCIETY Routledge and Kegan Paul*
1961	Braediktionsmetoder ...	Nordisk Tidskrift p 382
	Crime, Cause of Treatment. Recent Research and Theory	Ed: Research 6(1) p 18 - 33
	Research (Probation, Parole ...) Management Science and Research	Citizens Committee New York Management Science 8 (1)
1962	*An Essay on the General Theory of Prediction Methods*	*(in) Wolfgang M. et al. Sociology of Punishment; Wiley*
	Strategy of Research in Evaluation and some examples of methods	President's Commission Papers
	Review article	J. Documentation 18(1) p27-28
1963	The Measurement of Crime	Brit J of Criminol p 321
	Juvenile Delinquency: A Critical Review	Ed: Research v(2) p 104-120
	What is Crime	New Society 42 18 Jul
	The Divide - Action & Research	Prison Journal (U.S.) 43 p 5-24

1964	Delinquent Generations (A Rejoinder)	B.J.Crim p 264
	New Prediction and Classification Methods (with Mcnaughton-Smith)	J of Research in Crime 1(1)
	Offender in Society	*(in) British Psychol Society Symposium : Keele Univerity Press*
	Approaches to Community Mental Health	*(in) Progress in Clinical Psychology. Grosse and Stratton*
	The Times dated April 20th included the critical piece on drug policy.	
	Some Sociological Factors in Drug Control	*(in) Gerver et al Mass Society in Crisis McMillan*
	Report on visit to Korea for Asia Foundation.	
1965	Research Methods in Criminology	International Review of Criminal Policy 23 U.N
	Management Science in Great Britain .	*(in) Star M.K., Executive Readings in Management*
	SOCIAL DEVIANCE (Social Policy, and Research):	Tavistock Publications Action Prentice Hall (U.S)
	Stockholm U.N. Conference Papers	(Staff Papers)
	Research in the Personal Social Services A Code of Practice	(Monograph by chairman)
	Confidence and Competence in Decision making	Brit J. Criminol. Jan p 25

	New Thinking in Criminal Statistics	J. Crim. Law 56 (3) p 277
	A Behavioural Theory of Drug Taking	Howard Journal 11 (4) p 6
	Evaluation of Penal Treatments	Sociological Review Monographs Keele University Press
	Human Systems Research	Sigma Papers # 6
1966	BAIL: ISSUES AND PROCEDURES (with R.Clifford)	Canberra, Australian
	General Principles of Co-operative Learning	International Review of Criminology 24 p 3-8
	Some Notes on the Social Psychology of Violence	Monograph, NIMH Report
	Persistent Offenders and Preventive Detention	J of Crim Law 57(3) p 312
	Criminology: An Operational Research Approach	Monograph UNAFEI Series No: 2 United Nations Tokyo
	International Consortium Proposal	United Nations Tokyo.
1967	SOCIAL POLICY, ACTION AND RESEARCH	Tavistock (London)
	Survey of Field (Facts and Figures)	*(in) Effectiveness of Punishment* *Council of Europe*
	Information and Decisions regarding Crime	*(in) Yefsky S.A.* *Law, Science and Technology*
	Some Factors in Sentencing Policy	J. Crim Law 58(4) p 503 -
	The San Francisco Project	Nat Inst of Mental Health

Paper for Bureau of Census Meeting on Crim Stats.

1968	*A Behavioural Theory of drug Taking*	*(in) Buckley W. Modern Systems Research Aldine,*
	Values vs Variates	*(in) Wolfgang M Crime and Culture, Wiley*
	Acting Out or Social Action	Am: Orthopsychiatric Assn paper
	Improving Criminal Statistics	Proc: Amer Stat Assn: p 102

Sundry papers presented as evidence to the National Commission (President's Commission on the Causes and Prevention of Violence)

	Computers: Impact on Public Policy	Public Admin Rev 28 (8) p 503
	The Arts, Youth and Social Change	Research Report NIMH 67212
	Changing Concerns in Corrections with R. Carter	Issues in Crim 3(2) p 197 -
	The Concept of Cause in Criminology	Issues in Crim (3(2) p 147
1969	EVALUATION OF PENAL MEASURES:	Random House
	Data and Delinquency	Yale Law Rev: p 731 - 737
	Trends and Projections in Social Control Systems	Annals of Amer Acad: 381(Jan)
	The San Francisco Project	Final Report N.I.M.H. monograph
	Research, Demonstration and Social Action	N.C.C.D. Davis Monograph

	Crime Prevention and costs	U.N. International Review # 25 p 21
1970	*Requirements of Prediction*	*(in) Johnson et al Sociology of Punishment. Wiley N.Y*
	PROBATION AND PAROLE (with R. Carter)	John Wiley New York
	Criminal Statistics, The Future	Amer Correctional Assn: Oct 1970
	Revolting Youth, Law and the Future	N.I.M.H. Mono
	The Information Explosion and Social Control	Nat Council Crime and Delinq Mono
	Variety, conformity and control dilemmas for social defence	International Review of Criminal Policy U.N. # 28 p 18 -
1971	*The Deviation Amplifying System*	*(in) Carson W.G. Crime and Delinquency, Robertson, Glasgow*
	Five Pieces in Penology	Public Admin Rev 31 (6) p 595
	Computers and Criminal Justice	The Quarterly Review 29 p 46
	Crime and Illegal Behaviour	Paper Amer Soc Assn Denver Sept
1972	CORRECTIONAL INSTITUTIONS	Lippincott, New York
	Critical Review of Hogarth	Osgood Law Review 10(1) p 269-
	Foreword to Assaultive Youth	(in) Wenk E. part art: J of Res:9(2)
	Making it Ethical	Hastings Center Report 2(5)
	The Necessary Criminologist	*Acceptance of Sutherland Award*

		Am.Soc.Crim
	Some Ecological and Linguistic Aspects of Rationality and Morality	J Crim Justice 1(1) p 1-6
1973	Crime and the Tender-minded	Crim: Made in Canada (1)2 p 108 (reprinted in Community Education Series 1)
	Identifying Social Defence Policy Needs for Research	United Nations Conference Paper Copenhagen
	The Operational Use of an Experience Table	Criminology 12(2) p 214
	Crime and Criminal Justice at the Turn of the Century	Annals Am: Ac: 408 p 13 - 20
	Information Selection and use in Parole Decisions (shelf)	NCCD Monograph Research Report
	Information Overload	J. Crim Law 1073, 64 (2)
	Current Aspects of Penology Research.	Proc: Amer. Phil Soc. 118 (3)
1974	Sources of Deviance in the Schooling Process	Internat. Rev Educn: 20 p 306
	Crime and Crime Prevention Measures Presented A.A.A.S. Feb 1974	World Correctional Service Feb 1974
	The Coming Breakdown of our Criminal Justice System	Skeptic Special Issue # 4
	The Utilisation of Experience in Parole Decision-making	Monograph National Institute of Law Law Enforcement

	Directions for Corrections	Proc: Amer Phil Soc 118(3) p 235-
	Sources of Deviance in the Educational Process	Internat: J. of Ed:20(1973/4)
	The Necessity for Innovation	NACRO (Lancaster University)
1975	Statistical Methods of Parole Prediction	Amer Academy of Psychiatry and the Law, Boston
	A Typology of Decisionmakers	*(in) Amos et al Parole. Thomas,*
	Inefficient Statistics	*(in) Amos et al as above*
	Information Overload	*(in) Amos et al as above*
	Making Paroling Policy Explicit	Crime and Delinq: p 34 -
	The Impact of Developing Technology on Crime and its control	Amer Soc: Crim. Toronto Conference paper
	Crime and Crime Control	Prelim Papers for U.N. Congress (Crime Trends ....)
	Decision-making in the Criminal Justice System. Reviews and Essays	National Inst: of Mental Health Monograph
	Behavior Control in Prison	Hastings Report 5(1)
	Preface (Juvenile punishment -- some legal notes).	
1976	CORRECTIONAL INSTITUTIONS 2nd Edition rev: -: (wih R.Carter)	Lippincott, New York

	PROBATION, PAROLE AND COMMUNITY CORRECTIONS: (with R.Carter)	John Wiley, New York
	Project Maps Guidelines on Sentencing Decisionmaking.	Report in LEAA Newsletter 5 (8)
	Preface to book by D. Duffee	
	Sentencing Guidelines: Structuring Judicial Discretion.	Final Report of project. Crim Just: Research Centre Albany
	Patuxent Examined	Rutger's Law Rev. 29 (5) p 1102
	Criminal Sentencing	Judicature (60)5
	BAIL; ISSUES AND ANSWERS (with W. Clifford)	Australian Institute of Criminology
	Equity and Republican Justice	Annals 423 pp 152-161
	Some Objections in appendix to report (shelf)	*(in) von Hirsch, Doing Justice Hill and Wang, N.Y.*
1977	Doing Justice: Review Article	J of Crim Law 67(3) p 356-
1978	GUIDELINES FOR PAROLE AND SENTENCING:	Lexington Books, Mass:
	Guidelines for Sentencing	Monograph National Institute of Justice
	Policy Control, Ethics and	*(in) Abt L et al Discretionary Justice van Nostrand*
	Decision Theory and Comparative Analysis	Internat: Annals of Crimin. 17 (1)

	Multijurisdictional Sentencing Guidelines.	National Institute of Justice Program Test Design
	Make Crime Harder	University News 2(12) Albany N.Y.
	Structuring Judicial Discretion	Nat Inst of Law Enforcement D.C.
	Decision Theory and Comparative Criminology	Annales Internationales de Criminologie 17(1 & 2) p 191 -
	Changing Philosophies in Sentencing	Speech UNAFEI Tokyo
1979	*Human Subjects*	*(in) Klockars C Deviance and Decency, Sage Los Angeles*
	The Problem of the Question	*(in) Heller, (check title) Tavistock*
	Comments on current issues	*(in) U.N. Documents*
	Doing Something with Them	*Speech to John Howard Conference Toronto Sept 1979*
1980	*World Crime -- to measure* Sentencing Guidelines to reduce disparity	*(in) Newman G. Comparative* Crim. Law Rev. Apl. p 201
	Problems with Existing Prediction Studies and Research needs.	J of Crim Law 71(2) p 98-
	Sentencing Guidelines	Brit J of Crim Law (Oxford)
	Problems with the Concept of Crime	Paper presented at Symposium held at SUNYA to commemorate founding of School.

1981	*Crime and Quality of Life*	*in) Eliston F et al Ethics, Public Policy*
	The Origin of Parole Guidelines	*(in) Parole in the 1980's U.S. Parole Board Report*
	The Principles of Guidelines for Sentencing	U.S. Dept of Justice Monograph
1983	CONSUMERIST CRIMINOLOGY	Gower,London and Barnes and Noble (US)
	British Criminology Revisited	Howard Journal Sept 1983
1984	UK / USA, Contrasts in Criminology	Howard Journal 23(1) Feb 1984
	Machine Inferences and Clinical Judgment	Crim Just. & Behav: 11 (4) p 387
1985	*Juvenile Justice Process*	*(in) Rappaport R Children, Youth and Families. Cambridge U.P.*
	Symposium on video tape in celebration of "Founding of a Discipline" (Library at Albany -- U.S. video system)	
	A lengthy interview with lawyers of the U.S. Defenders Association	in The Defender 6(5)
1986	*Three Projects involving Prediction in Social Science.*	*(in) Heller F. Uses and Abuses Sage.*
	Criminal Statistics - National and International	*(in) Hartnagel T. Critique and Explanation. New Brunswick and Oxford*
1987	*Disparity in Disposition of Justice .*	*(in) Pease K Sentencing Reform. Manchester U.P.*

	Public Demand for Punishment (with K Pease)	International Journal of Sociology 7(3) Special Issue
1988	Future Penal Philosophy	Prison Journal 67 (2) pp 76
	Future of Graduate Education in Criminal Justice	J Crim Just Ed: 1(1) p 21 - 32
1990	*Retrospect and Prospect Fashions in Criminal Justice*	*(in) Gottfredson and Clarke (Eds) Policy and Theory in Crim Just: Avebury*
1991	Crime and Crime Control	Korean Institute of Criminology
	PUNISHMENT, CRIME AND - : MARKET FORCES	Gower, Guildford and Dartmouth Brookfield U.S.
1992	"Does it help to transform the problem?"	The Canadian Journal of Law and Jurisprudence. 5 (1)
1994	*Chapter in book by Jacoby et al (of Jefferson Institute) My chapter titled "Uses of Simulation Methods in Prosecutorial Decisions.*	
	Drugs Policy feeds crime habit.	Freethinker April 1994
	Review article. Challenges to the Enlightenment, A Defense of Reason and Science.	Freethinker Aug 1994
	Using Murder Review article (Social Construction Theory)	Social Pathology 1(1)
1995	What do we do now?	in Barnes W (Ed) Taking Responsibility Centre of Crim: Toronto Uni

1996	Letter in B.M.J. re rationing. (with A.J. Wilkins)	Brit Me Jnl 7064 v 313
	Action and inaction in Social Research and Policy	*Journal of Research in Crime and Delinquency, 33,(1), Feb.*
1997	*Wartime operational research and situational crime prevention*	*in) Rational Choice and Situational Crime Prevention (ed) Newman, Clarke and Shoham, Ashgate, Dartmouth*
1998	DID I REALLY BECOME A CRIMINOLOGIST? An autobiographically based comment on social research and politics	Mimeograph. (Self-published)
1999	Wrong crop: wrong problem	Freethinker, October
2000	UNOFFICIAL ASPECTS OF A LIFE IN POLICY RESEARCH	Posthumously printed.