

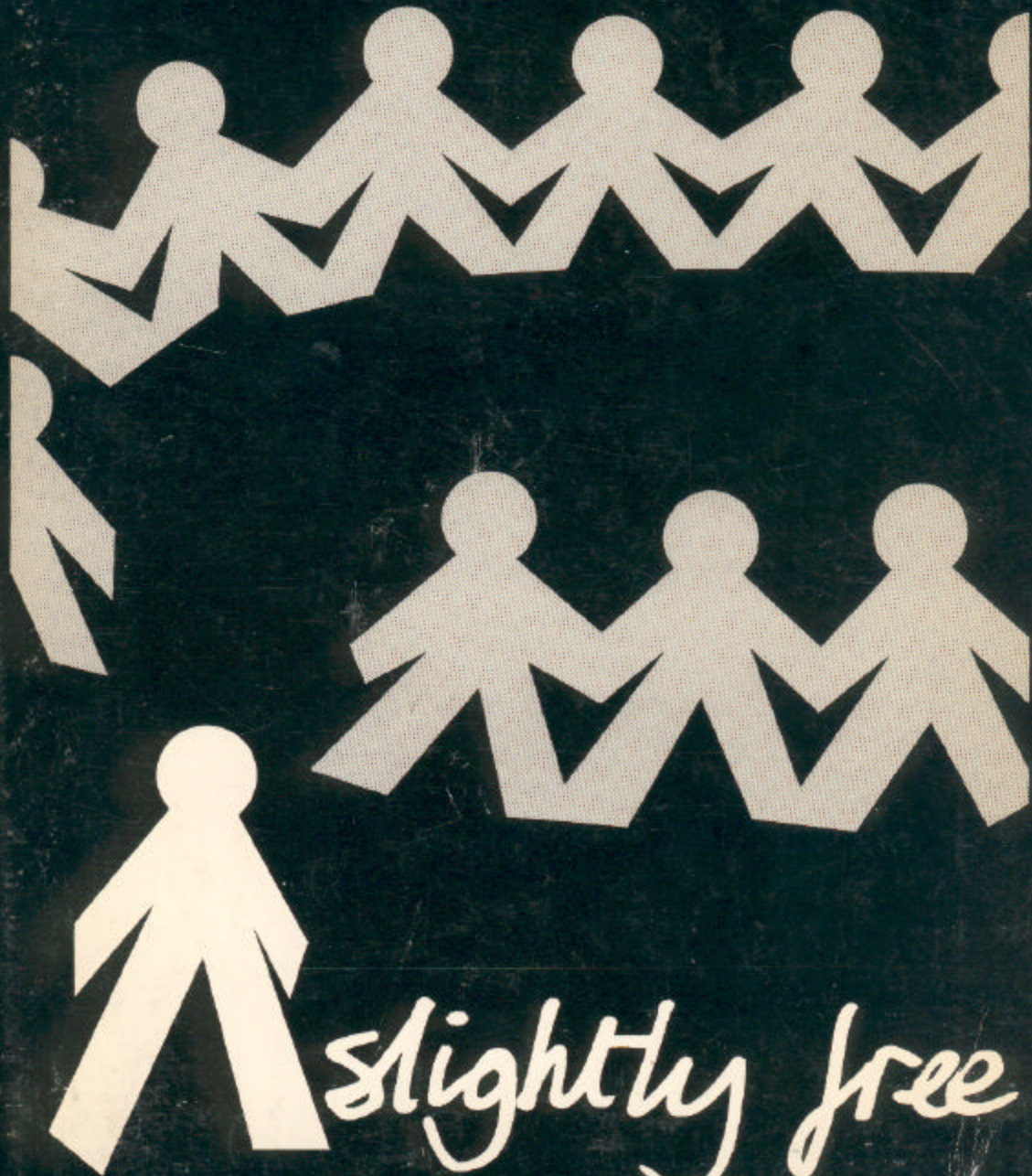
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Law Reform Commission  
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Commission de réforme du droit  
du Canada

*Permission  
to be*



*slightly free*

# Permission to be Slightly Free

A Study  
of the  
Granting, Refusing and Withdrawing  
of  
Parole  
in  
Canadian Penitentiaries

P. Macnaughton-Smith

Centre of Criminology  
University of Toronto

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Peter Macnaughton-Smith, formerly of the Centre of Criminology of the University of Toronto, now with the Max-Planck Institut für ausländisches und internationales, has written a provocative account of decision making at the National Parole Board. Reform of sentencing within our criminal law must of necessity look at the results of the process of incarceration and release. It is with this in mind that we present the author's views.

A French edition has been published under the title *La liberté au compte-gouttes*.

Mankind are greater gainers by suffering each other to live as seems good to themselves than by compelling each to live as seems good to the rest.

Though this doctrine is anything but new, and, to some persons, may have the air of a truism, there is no doctrine which stands more directly opposed to the general tendency of existing opinion and practice.

*John Stuart Mill, "On Liberty" 1859.*

The demand that all other people shall resemble ourselves grows by what it feeds on. If resistance waits until life is reduced *nearly* to one uniform type, all deviations from that type will come to be considered impious, immoral, even monstrous and contrary to nature.

*Ibid.*

We do not apply it (this concept of individual freedom) to children, at home or at school nor to employees, nor to persons of any class or age who are in the power of other persons. Like Queen Victoria, we conceive Man as being either in authority or subject to authority, each person doing *only* what he is expressly permitted to do, or what the example of the rest of his class encourages him to consider as tacitly permitted. It will be found that those who are most scandalized by the liberties I am claiming for the convict would be equally scandalized if I claimed them for their own sons, or even for themselves.

*George Bernard Shaw, "The Crime of Imprisonment"*

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## Preface: Reassurances and Thanks

Right from the start it is clear that this book is not in praise of “fighters against crime”; so right from the start, in case any such crime-fighter feels unfairly attacked, I offer some words of peace.

Any group of men such as a parole service, a prison system, a police force, or a bench of judges or magistrates is naturally interested in how well they are doing their job. From time to time they may wonder three things about their work: whether they are doing it as well as the circumstances allow, as well as they did in the past, and as well as somebody else might do it. So when a critic or a researcher appears less than delighted with some of their activities, they may well feel that he is answering their three questions adversely. But these are precisely the three questions that I have neither asked nor answered. My only detailed information relates to a few years and to one nation. Nowhere have I said that the activities that we study in this book have been better or worse carried out by other people, in other lands or at other times. I am more interested in the common features of human societies than in awarding points for their differences. So however much an individual may feel that I have turned the spotlight uncomfortably close to him, there is no suggestion that he more than another person merits such a spotlight, or that he looks worse in its light than another man would.

At a more important level I have made certain criticisms which I have no wish at all to explain away in a preface. I shall claim in the course of the book that most of what we officially do about the acts we call crime and the people we call criminals is worse than ridiculous; and if so, anybody who spends any part of his life shutting people up (or otherwise harming them) or collaborating with people who shut people up or harm them must come to terms with this in his own way, and I have neither praise nor comfort for him. But neither have I blame; it is he and not I who has to decide whether joining in the shutting-people-up game in any capacity is how he wants to spend many of the adult daylight hours of his one and only lifetime.

On one group my comments might appear somewhat one-sided. There is much to be said both for and against the police, and it is being said, loudly and softly, passionately and calmly, by many people; but it would be unfair for me to write of policemen as though hauling people off to court or to prison was

their only activity or even their main activity. The police spend most of their time doing many other things besides that, some of them very good things indeed, and this is not a book about either their good deeds or their bad deeds. A criticism of society's habit of shutting people up and in other ways doing bad things to them should be taken for what it is, and not as a comment of any sort, unfavourable or favourable, on any other police activity.

I should like to thank some of the many people and groups who have helped in so many ways with this study. One of the best ways of helping a research project is with money, and this has been generously provided at different times by the Nuffield Foundation and the Ford Foundation to whom we are very grateful. The National Parole Board gave us access to the data needed for the study, and many members of the Parole Service and the Penitentiary Service spent a great deal of time answering our early questions, inviting us to visit penitentiaries, making our visits as little unpleasant as such things can be, and discussing many things with us. I am especially grateful to Mr. Frank Miller, formerly Executive Secretary of the National Parole Board, for his consistently clear and practical advice and constant helpfulness no matter how busy he was when we bothered him. Mr. F. W. Pay of the National Parole Board's Records Department in Ottawa supplied us promptly with the files we wished to study and rarely hurried us to return them, even though they were often still "live" from the Parole Service's point of view. In the early stages of the study Mr. R. S. Beames of the Toronto office of the National Parole Board allowed us to make considerable use of his time, his attention, and his experience. Mr. J. Hug of the Department of Correctional Services, Government of Ontario, supplied us with a copy of a computer programme he had written to carry out a form of data-analysis of my devising.

I have been very fortunate in the colleagues in the Centre of Criminology, University of Toronto, who have worked with me on this project. Mrs. Audrey Mank bore perhaps the heaviest burden, reading and recording the information on nearly a thousand National Parole Board files without either losing her vigilance or going mad. Part of the time she was helped by Miss Maureen Spencer. In certain decisions on the interpretation of the files, and in preliminary data processing, I was lucky to have the help of Clifford Shearing, whose general interest in the philosophy of the study was extremely stimulating. But the person who has done most in bringing this work to a reportable stage is Miss Barbara Schloss. Helped for a while by Miss Susan Eagles, but mainly on her own, she has handled the processing, computer programming, and analysis of the data with extraordinary efficiency and expertise, and with an extremely encouraging patience in the face of difficulties. I should also like to thank all those who have typed any part of the report at any time, but especially Miss Marbeth Greer who typed all of the first draft at least once and much of it twice. She has the uncanny ability to read my multiple palimpsests when I can no longer decipher them myself.

Other colleagues in the Centre of Criminology have not worked directly on this project but have helped with their ideas and enthusiasm. The Director, Professor John Edwards, in the early days of the study took an enormous load of administrative problems connected with the project off my back, and he has always very patiently accepted the discovery that my aims (and my time schedule) on this project were completely different from what he had imagined. Dr. John Hogarth is a friend with whom I always find it extremely profitable to exchange ideas, and whose enthusiastic concern for the well-being of the Centre as a place where good research could be hoped for was, until his departure for York University, one of the main supports of my efforts to do such research. Dr. Malcolm Courtis supplies very penetrating examinations of some of my speculative ideas. Irvin Waller has acted as an always accessible reservoir of knowledge on correctional activities and correctional research over the world in general.

Most of the criminologists outside the Centre whose thinking has noticeably influenced this book are referred to by name in the text or the references; but there are in particular two of my friends who have had an influence much greater than can be sufficiently acknowledged in that way. Leslie Wilkins introduced me to the study of crime, and to prediction techniques in particular, and his mixture of hard objectivity and adventurous creativeness has been an example to me ever since. But of all the people with whom I have shared my thinking it is Dr. J. W. (Hans) Mohr who has had the most profound influence on my ideas about how to study people; this influence would require a separate essay to describe, but it will be clear to all readers of this work who know anything of his outlook.

Dr. Courtis, Professor Edwards, Dr. Hogarth, Dr. Mohr, Miss Schloss, Mr. Waller and Professor Wilkins, all mentioned above, Professor W. Outerbridge of the Centre of Criminology, University of Ottawa, and Mrs. F. Simon of the Home Office Research Unit, England, have read various earlier drafts of this work. Many of their very helpful suggestions have been incorporated.

But however much help I have received, and however great some influences on me have been, the responsibility for the following book is mine. No one else is to blame for its faults, and most people might not want to be held accountable even for what I sometimes imagine to be its virtues. So I must presumably take the blame for these as well.

## PART I

How do we study men who have been shut  
up and who ask to be let out;  
and men who shut up other men?

## Section 1

### Introduction: Why not to read this book, etc.

This book suffers from an incurable conflict between its medium and its message. The mere fact that the book exists, that it is in your hands at this moment, that it is classed as a serious study rather than fiction, humour or light entertainment reinforces the idea that crime and the things we should do about it are a large, grave problem rightly of great concern to all citizens and requiring close study by expert social scientists and men of public affairs, if we have any confidence in either of such people; in short that the cure for present failures lies in more effort by more people with more resources at their command. All this is the contrary of the truth, as I shall argue later. Compared with life's other troubles the acts that we call crimes are in themselves a small-scale social problem, and the official, contra-criminal things we do about these acts are mostly ridiculous, unpleasant and without relevance.

Yet in this case, why should I wish to spend much of my life studying such things, or why should you wish to read about them? If something is merely laughable, why not just laugh? Does not my spending four years carrying out and describing a research project on the National Parole Board imply that I do not mean what I say, that really I think that the subject of official contra-criminal activity is important?

No. We have to reverse two ideas about research. Researchers do not, as is widely thought, mainly examine questions to find answers. Often they examine existing answers, and facts which won't fit these answers, to find new questions.\* Thus they do not study something because it is already important; in fact we could say that they give it a part of its importance by studying it. For example, the actual often laughable words uttered by hysterics

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\*For ample documentation and development of this point, see Kuhn (1962).

had not been important for some centuries *until* Freud and his precursors once again listened to them instead of dismissing them as the trivial mouthings of unpersons.

The actual words of hysterics are *still* not an important *social* phenomenon; immensely more good and harm flow from the words of the officially normal. Yet much of our understanding or misunderstanding of human behaviour, much of the behaviour itself,\* arises from theories developed by (or in opposition to) Freud and other workers like him, who decided to study such "irrational" words and thus made them important.

This book is not about the words of hysterics, laughable or otherwise. On the contrary, it is mainly about the ordinary everyday actions of two very respectable types of citizen: members of the National Parole Board, and criminological researchers. Nevertheless, the behaviour studied is in one sense distinctly odd. For the story that we tell is of some men who have been compulsorily shut up for a while in large buildings far away from their families and their work, and who have then been told that they may ask to be let out early. Some of them have so asked, and their requests are considered by another group of men, who decide to let some of them out early but not the rest. But then, out of those that they decide to let out, a little later or even many years later they, or some other people, decide to shut some of them up again (and to tell them again that they may ask to be let out early.) Surely if we do not think this whole situation odd this is itself a fact that needs explaining; that almost all the advanced nations of the world do these things and do not think it odd; that some of these things, or things like them, have been done by all the literate peoples of the world for more than six thousand years and still very few of us think it odd. So even if the acts that we call crimes, and what we do in our official response to them, are not socially important, they remain well worth studying, and even if parole is a very small part of what is done in response to crime nevertheless any partial understanding of this shutting-up and letting-out game, or of any other crime or counter-crime activity, might be well worth gaining, just as a small part of man's effort to understand himself.

Still, the fact remains that I and my colleagues by studying and by writing about any aspect of crime or of society's official response, you by reading what we write, however pleasant and worthwhile this study and writing and reading may be, are, nevertheless, adding seriousness in a social area where ridicule is more appropriate. I have done what I can to make amends; in speaking of these matters I do not think that my tone is too respectful. Nor, on the other hand, do I think that I have veiled the scientist or the researcher in a luminous glow of sanctity. I have tried to "tell it like it is" (i.e. as I see it) about the research as well as about parole.

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\*e.g. the behaviour of a psychiatrist, or behaviour in any way related to the existence of psychiatrists.

Another point about the book may need explaining. Considered as a research report, it may seem very diffuse and chatty. Now our basic ideas of how to report research have been derived mainly from the physical sciences; and it seems to me that the problems of communication between a physicist or chemist and his readers differ in two important respects from those between a researcher in crime and *his* readers. Firstly, the physicist's readers are assumed to share a knowledge of the concepts he has used in his study; if he uses Bessel functions to describe the shape of a vibrating drum, he will write his research report only for readers who understand both Bessel functions and the properties of vibrating drums. It would be ridiculous for a researcher in crime to write like this. The colleagues who read his work may some of them have some knowledge of some scientific discipline; but there is no single system of specialist knowledge universally shared by most of the readers of a work on crime. The author may assume his reader's intelligence and goodwill, but nothing else.

Also, the physicist assumes the emotional neutrality of his readers towards the phenomena under study. Although they are as emotional as any other group of people, the physicist does not expect that their understanding of the complex properties of vibrating drums will be seriously blocked, distorted or biased by their own preference for bass drums as against kettle drums, or their childhood frustrations with tin drums. But we students of crime have been playing cops and robbers since we were four, or at the very least listening to legends of Robin Hood or of Pontius Pilate; we know that criminals and the people who run our contra-criminal activities are people just like ourselves, which is to say that we are people just like criminals; and of course everyone is an expert on people just by virtue of being one. We do not regard dispassionately attempts to "explain crime" because these involve explaining something about people and we might find we had explained away a part of ourselves.

So I cannot assume any shared specialist knowledge, and I cannot assume in myself or in you scientifically neutral emotions. I am forced to take these things into account, if only to find a way round them; and, as I say, this makes the report diffuse and chatty compared with the brief precision of the physicist. On the other hand, it means that the layman can read as well as the expert, if he is willing to do some hard work.

I have made two exceptions to the decision to write like this. Firstly there are good, enjoyable, compact accounts for the layman of the basic ideas and methods involved at the more elementary levels of probability and statistics. It would distort the proportions of the present work to repeat such an introductory course here; if any reader doubts whether he understands sufficiently the concepts of a sample, a probability, a random event, an estimate, a variance, a statistical association between variables, or a

correlation, or the simpler techniques associated with these terms, he is advised when he reads Section 7 and onwards to keep by his side (or even read in advance) some introductory book on statistics.\* Ideas or techniques going beyond this level in the present work are explained as they occur.

Secondly, there are three appendices where certain matters, not essential to a general understanding of the book, but possibly of interest to the statistically minded, are discussed in the language of statistics without any concessions to statistically untrained readers. But I have not used this sort of in-group language in the main parts of the book. There are also two appendices where Parole Acts and Amendments are quoted in detail, and here of course I leave the original language in its incomprehensible splendour.

To return to our distinction between the criminologist and the natural scientist, there is another important difference between studies of physical phenomena and those of social institutions. When a physicist or chemist studies the properties of iron, he is, I imagine, studying to some extent the same iron that physicists studied ten, a hundred or even a thousand years ago. But social institutions are said to change, especially those many guilt-ridden bureaucracies that seem to be constantly in a state, either of having recently completely changed their operation, or of having such a total change at an advanced stage of preparation. So, certain self-styled practical men may assert, the data collected in the present study refer to a past state of affairs and the study has no practical relevance. What is needed is rapid studies on up-to-the-minute data.

Now I don't believe a word of this. Palace revolutions and the writing of new constitutions may seem from the point of view of the deposed monarch and the former court favourites to have cancelled all past history; from the point of view of the millions not blessed with his royal blood or royal favour, very little may have changed. The nastiness that we do to-day "for the good of society" is remarkably like what we did yesterday, and remarkably like what Hamurabi did four thousand years ago, though he stated his intentions in a less flatulent prose than that of our present day tyrants. Again, if yesterday's data are worthless to-day, to-day's data will be worthless to-morrow. What I can welcome is not studies that fade like the morning dew but those that examine more stable features of the human condition; contemporary relevance is not the same thing as to-morrow's irrelevance, and the activities of Canada's Parole Board in the early nineteen-sixties relate to more than just five years in the history of a few members of one nation, cancelled a few days later by a stroke of a legislator's pen.\*\*

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\*For such a reader I strongly recommend M. J. Moroney, "Facts from Figures", Penguin Books, 1951, 3rd edition 1956 and later reprintings.

\*\*See in due course Section 16.



## Section 2

### The plan of the book, and the reasons for the plan

It is surprising how many reports of social research, although they give a clear enough account of the researchers aims and of his conclusions, give only the cloudiest sketch of the research itself, of what the research team actually did. It is not clear to me why some people who regard themselves as social scientists should let lapse what has been one of the principal rules of the scientific game: that in reporting research, sufficient information should be given to enable the reader adequately to repeat the research (basically, to "see for himself"). Of course there may be other, less avoidable reasons than lack of information why the research cannot even in theory be repeated; the problem of the inherently unrepeatable experiment arises more often in the social sciences than in, say, physics. (Geologists and astronomers have the same problem, but they never seem to have worried much about it.) In any case, the philosophical concept of an "adequately repeated" experiment is tricky. However, all this is no reason why bad reporting should avoidably widen the gap between social research and other science.

In any case, in a book like the present one, where we wish to take the discussion further afield than in many research reports, a clear statement of what we did and what we didn't is more than ever necessary as a foundation for the reader to build on. So Section 4 is a preliminary broad outline of this, without any account of what we thought, why we did one thing rather than another, what we were trying to do, what we found, or what we concluded, except where these are absolutely necessary. Simply, it is a historical narrative of what we did. Before this, however, Section 3 gives an account of some of the main decisions that had to be taken at a very early stage of the study.

The rest of the book, Sections 5-16, is basically an expansion of Section 4. Each stage of activity and each concept introduced in section 4 is discussed in greater detail, together with the thinking, choosing, goals, findings and speculations that were left out of Section 4.

Thus Section 5 discusses our reasons for undertaking the project at all. Section 6 deals with the facts and assumptions about the world of crime and anti-crime, including parole, which are needed to understand the project, and how these facts and assumptions are reflected in the research. Sections 7-15 deal with our data and analyses, and Section 16 draws together some of our main conclusions or inconclusions.

To make the design of the whole book correspond to Section 4 makes for clarity; its disadvantage is that the reader who wants to start with the results and then proceed to discussion of these, skipping the rest, will have to read his chosen sections in a rather strange order. However, the list of contents and the guidance just given are enough to direct him, though he will often find that to understand his chosen section he has to read others as well.

## Section 3

### Early decisions about this study

In any research, no matter how objective the researcher may wish to be, a high level of purely personal choice is inevitable at many stages. Few of these choices can be referred back to an explicit underlying methodology. One's hope is that not too many of these choices will be determined by bad habit or irrelevant accident. The choices may refer to aspects of the phenomena studied, of the objectives, of the techniques or of the resources.

Some of the earliest choices in the present study were the following. The bare decisions are listed first, and a few factors leading to them are mentioned afterwards.

The phenomena studied would be the decision by Canada's National Parole Board to grant or to refuse parole to male inmates of Canadian penitentiaries, and the loss of the partial liberty so conferred as a result of a later decision by the Parole Board, police or judicial activity, or more than one of these.

Decisions about inmates of all male penitentiaries, and of no other institutions, would be studied.

All the data used in the study would be drawn from the files about each parole applicant and parolee kept in Ottawa by the National Parole Board.

The techniques used would be the relatively objective ones of statistical analyses of a large sample. In particular, the techniques of mathematical prediction (see Section 10) would be used. Both the granting of parole and the loss of the partial liberty that it confers would be "predicted" and the relationship between the two would be analyzed.

The objective, however, would not be the development of a "prediction instrument" to help this or any other parole authority achieve its goals (see earlier this Section, and Sections 5 and 6 for the goals of what we do about

crime; and more specifically, Section 12 for the goals of parole). It was to throw as much light as possible on the actual practice of a parole board, so as to clarify the nature and properties of the phenomena; and the function of the specific techniques chosen was to provide an appropriate structure, or at least boundaries, to one particular study which could otherwise become very diffuse.

Most of the choices listed above were choices of what not to do. It is perpetually surprising how much of research consists of this type of decision. Women were excluded from the sample because they are too different (criminologically) from men to be included in the same sample. Inmates of places other than penitentiaries were also excluded because in Canada the nature of the parole-granting or refusing process is sometimes very different for them.

The restriction of data to one file per man studied made enormously for clarity and convenience. Also, this is the file on which the decision to grant or refuse parole is based and this was the major part of the subject under study.\*

The decision to carry out a prediction study was a meeting of several circumstances. The reasons for the decision were historical rather than logical. It should be explained that the financing and the official cooperation necessary for the project had largely been obtained before I was invited to undertake it. It was known that I had worked on prediction studies in the past. At that date the growing disillusion of many prediction-researchers with criminological prediction as an aid to decision-making was not yet widely known among their colleagues,\*\* and certainly not among many "correctional" administrators and fund granting agencies. A parole system was due to be introduced in Great Britain, and this probably raised the enthusiasm of the Nuffield Foundation for this study. Also, I was glad to undertake the project, though as the reader will gather this was not for the reasons that most people thought.\*\*\*

Thus we can see how the decision to "do a prediction study" came about. What has this to do with scientific strategy? It seems a long way from the disinterested quest for knowledge. But as all researchers soon find out, the disinterested quest finds strange means and strange occasions to keep itself "alive and (as the old phrase so aptly puts it) kicking."

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\*The study of the subsequent keeping or loss of the partial freedom conferred by Parole was largely used to shed light on the original decision to grant or refuse it. See Sections 12 and onward.

\*\*Even as authoritative a work as the second edition of Johnston, Wolfgang and Savitz (1970) still reflects the early optimist view of prediction. But see later in the present book, and especially Section 14, for a more wary approach.

\*\*\*A word of appreciation is due to all those people who took an early interest in this study, but especially Professor John L. J. Edwards, Director, Centre of Criminology, University of Toronto, for the speed and goodwill with which they accepted the discovery that my aims were almost the reverse of what they had thought.

## Section 4

### Roughly what we did

We examined a preliminary batch of thirty National Parole Board files. At this stage I shall stop to explain nothing. If you don't know what the National Parole Board is, or even if you don't know what a file is (ah! what bliss) never mind. Just remember that there is such a board and that it has files, and all will be made plain later. The same principle applies through all this chapter. The figures in brackets after certain words refer to the section where the fullest discussion is found. These thirty files were about thirty men whose parole applications had come up consecutively before the Parole Board for interim or final decision. Fifteen of these files related to men in jails or reformatories, but the other fifteen related to men in penitentiaries. The latter fifteen files were minutely studied to see what information was in them. The information on the file was divided into two parts; that which was put on the file before the man's release from penitentiary, and in the case of men granted parole (6) and some other cases, that which referred to the time (7) after his release. Pre-release information put on the file after his release was generally ignored.

Pre-release information was provisionally listed for inclusion in our study if it was available on most of the fifteen files and capable of being systematically recorded. At this stage no question of relevance was consciously considered; availability and recordability were the only intended criteria. For the post-release period we listed, in the case of men on parole, information related to further convictions; and to arrests, charges, and police questionings; and also to suspensions (6) revocations (6) or forfeitures (6) of parole.

The next step was to decide provisionally in what form to record this information, which, when we had drawn a proper sample of men to study,

was to be put onto punched cards for computer analysis. In other words we designed the first draft of the coding schedule (7). At this stage these thirty files, the 'pre-pilot sample' had served their purpose. They play no further part in what follows.

We then established the sampling frame (8) and drew the sample of cases to be studied. The sampling frame consisted of a list kept by the Parole Board of the outcomes of all decisions (we used at this stage all those made between the 1st of January 1962, and the 31st of December 1964) and the file numbers of the cases to which they referred. If a decision was to grant (6) or refuse (6) parole we regarded it as a final decision; any other decision, such as defer, or make further enquiries, we defined as non-final. Each final decision was paired with a two-digit random number (between 00 and 99). If this number was 00 or 01 the decision was included in the pilot (4) sample. If the number was between 02 and 09 inclusive the case was assigned to the "study sample", (4, 8). If the random number was between 10 and 17 the case was assigned to the "replication sample", (4). The study sample and the replication sample together formed the "main sample". If the random number was 18 or greater the case was not included in any of our samples.

At this stage the pilot sample contained 115 cases, the main sample 1,039. No further cases were added; now we have to consider rejections.

There were two reasons for rejecting files; either because they should not have been included in the sample in the first place, or because they fell into definable categories that we believed would receive different treatment or consideration from the group that we were interested in studying. Thus the first reason arose from a question that was technical and objectively answerable; did this case belong in the sample as we had defined it; and the second reason arose from a question that represented a choice about the research; did we wish to study some particular category into which this file fell. This choice was in some ways like the decisions listed in Section 3 not to study women or inmates of prisons other than penitentiaries, but these two decisions were foreseen and such cases did not enter into our sample, whereas we are now discussing cases which entered into the sample and were then rejected. The reasons for the cases of this sort listed in Table 2 are as follows:

*Parole decisions taken on or after September 1st, 1964.* On this date "minimum parole" was introduced, a system which we need not discuss here except to note that it was granted to some who would otherwise have been refused parole. Others were refused longer parole who would have received it had minimum parole not existed. Thus cases handled before September 1st, 1964 were not comparable with those treated under the new circumstances existing after that date.

*Files contained further decision on minimum parole.* In some cases decisions taken shortly before September 1st, 1964 appeared to have been reached after considerations of the approaching changes in the parole system (see above). Such cases were not comparable with decisions not based on such considerations. We do not know how successfully we separated the two types of case, but we rejected those where after refusing parole there was a further decision to grant or refuse minimum parole, and we accepted those cases that did not contain such decisions.

*Parole granted for deportation and Files mentioned deportability of applicant.* Obviously the basis for granting or refusing parole here is not that which we are studying in the rest of the sample.

*Parole cancelled before inmate's release.* As we did not know the story behind these cancellations, nor to what extent they were inmate-activated, we did not know whether to regard parole as having been granted or refused in those cases.

*Inmate's largest single sentence less than two years.* Such men are rarely found in penitentiaries (see Section 6) and may well be considered on a different basis from the rest of the sample. There were too few of them (although they represent 2 per cent of our sample) to carry out separate analyses.

The files which should not have been included in our sample were those where the decision turned out not to be a final decision or not to fall within the chosen dates, and those where two decisions (one of them not final) entered our list, but of course the file was only coded once; and one file was overlooked by us and there was one which a member of the parole service preferred not to release to us without higher authorization. We did not seek this as it would at that time have delayed the study (we were almost ready to begin the analyses) more than seemed worthwhile for the sake of one file.

The existence, on the Parole Board's list from which we drew our sample, of cases which appeared to, but did not really, belong to the defined population of study raises the question whether any cases which we should have sampled were missed through not being on this list. The only case where we should expect this to apply is where a final decision falling within our period was listed as falling in a different period. We should expect to have missed about six files in this way.

The 207 rejections reduced our main sample from 1,039 to 832 cases.

We arranged to have sent to us the files in the pilot sample. At this stage we should repeat the thanks already offered to two members of the Parole Service: Mr. Frank Miller, the Executive Secretary to the Parole Board, and Mr. F. W. Pay, of the Parole Service's Records Department in Ottawa. As far

as data-collection is concerned, this whole study went extremely smoothly, and this is very largely due to these two very helpful people.

The data on the pilot sample files were coded in accordance with the provisional coding schedule on Mark-Sense data sheets, which are designed to be converted mechanically into punched cards. At this stage (and also at two later stages) we discovered which items of the provisional coding schedule "wouldn't work" either frequently or in particular cases, and so had to be revised or rejected. These items both required and generated a great deal of discussion aimed at the conceptual sharpening of everyday terms and phrases, both those used in the files and those that we had introduced into the coding schedule. Most terms, such as, for example "victim" which we use in discussing crime (or any other social phenomenon) are far harder to define precisely enough for this type of research than one would judge from the ease with which we use such words in everyday life.

When the whole of the pilot sample had been coded, tabulations were run to establish which variables gave rise to too many "don't know" or "does not apply" responses to be valuable, (in some cases these replies were judged to be informative (7)). The tabulations also indicated which categories of response to some items arose so rarely that it was better to combine certain categories, and which responses varied so little as to make that item useless.

In the light of this information a final coding schedule was designed. Arrangements were made for the files in the main sample to be sent to us and returned to the National Parole Board in batches. The data in the main sample were coded.

Of course, difficulties still arose that we had not already allowed for. Now our approach to these had to be different. Changes in the coding schedule could rarely be made. Just occasionally a new category was worth adding, though if we had not needed it in coding the pilot sample it would refer to very few cases in the main sample. What was normally done was to allocate unforeseen cases forcibly to existing categories, and specify the implications that this had for our original definition of the category.

When all the cases in all the samples were coded, the punched cards needed for the computer analyses were produced from the Mark-Sense data sheets. Then a group of 57 cases (three random batches which had been coded early, middle and late in the coding operation) was coded a second time, and the first and second codings of the same individuals were compared to establish which information could or could not be regarded as reliably coded. For the implications of the concept of reliable coding see (7).

For the variables regarded as reliably coded, analysis now began. The first stage was to define a set of transforms so that the data, which, because of the size and deliberate inclusiveness of the design, had necessarily been coded



in the form most convenient to the coder, could be analyzed in form most relevant to the researcher. A very simple example of this is that the coder might record two dates where the fact that interested us might be the interval between them. Other more complex cases were frequent.

The analyses of the variables in their new form fell into four main categories. Firstly there were basic tabulations of frequencies, or "head-counts" on variables taken one at a time (8). Secondly, relationships between variables considered in pairs were investigated, especially relationships between variables referring to the granting or refusing of parole and the keeping or losing of the partial liberty which parole confers (9, 13) and the other variables in the study. Thirdly, variables were combined in sets to form equations of various sorts "predicting" (10, 11, 14) the grant or refusal of parole and the keeping or loss of the partial liberty that parole confers. Fourthly, we analyzed the relationships between the other three sets of analyses, and especially between selection for parole and the subsequent keeping or loss of partial liberty. (13, 15)

The role of the pilot sample has been described. The roles of the study sample and replication sample were as follows. All analyses were carried out on the study sample. Those analyses that relate to prediction, and certain others concerning relationships between variables, need, for logical and technical reasons, to be replicated (roughly, in everyday language, checked) by means of a second sample and it was for this purpose only that a double sample was used.

After the analysis, one could almost say that we were ready to begin the research. This was the point at which understanding what we were looking at and what we were doing was no longer just a necessary precondition of our activity; it was the activity itself.

After that came the writing, the long discussions with many colleagues, and the writing again.

## Section 5

### Goals of the Research, Goals of “Corrections”, and the Relationship between Researchers and “Correctors”

We may suppose that “Correctional” administrators and workers, like the rest of us, mainly see the organizations where they go each day to work as inherently worthwhile. They will not in general see them as needing total change or abolition. However, many of them see their agencies as needing considerable alteration in detail to increase efficiency, and they see the role of the researcher to lie in suggesting, considering, or evaluating such detailed changes. Again like the rest of us, they will see this point of view as so normal and reasonable, in fact so inevitable, that they will be sincerely bewildered at the unrealistic, long haired (and simultaneously egg-headed) triviality or pig-headedness of those researchers who disagree.

Yet like most inevitable points of view, this one becomes much more evitable when we start to look at the unspoken but implied assumptions beneath it. For example, the view just outlined takes for granted that we can meaningfully discuss the efficiency of official responses to crime. Consider this notion of efficiency. People discuss the efficiency of a transport system; on the other hand I have not heard them discuss the efficiency of a religious service, a tree or a love-affair. So the idea that official responses to crime have an efficiency, in any reasonable sense of that word of many meanings, is only to be taken for granted if we also take for granted the idea that in some important way these responses belong in the same bag as things like transport systems and not in the same bag as things like religious services, trees and love-affairs. Now I don't want to argue just yet that this point of view is wrong, although I shall do so later. At this moment I want merely to point out that it is not an inevitable point of view; in fact it is a relatively recent one on

whose origins it is illuminating to speculate, though in this book we can only do so very briefly.

We may suppose that very few accused people would attend trial, and very few convicted persons submit to sentence, if these activities were purely voluntary. To try, to sentence, and to carry out a sentence, are exercises in the power that rests on force;\* furthermore, power is used for the good of its possessor, not for the man subjected to it, unless coincidentally. Now the use of power over other people tends sooner or later to acquire justificatory explanations, developed either by the powerful themselves or by theorists on their behalf.\*\* There are many kinds of self-justification possible but here I am only interested in two.

The first consist very simply of giving reasons for what one is doing. Nowadays some of the reasons given for officially, centrally convicting people and doing bad things to them are in terms of the "needs and aims of society" and this leads naturally enough to discussion of how efficiently these needs are being met and these aims being pursued. But this does not explain why people should happen to be talking in this way at this point of history and not at others. The second form of self-justification is to show that one's activities are approved by certain other respectable people. Which people at any instant have the magic power to hallow one's actions appears as a cultural and historical ebb and flow. As for convicting people and doing bad things to them (including inflicting compulsory "help" and telling them it isn't punishment) this has been justified at different times and different places by reference to the real or supposed values of many different people; the conqueror (whose goals are perhaps summed up in the word peace) the king (unity) the priests (holiness) the philosophers (wisdom) the people (fraternity) the ruling class, (order) and doubtless many others. Today the magic power to justify what we do is often imagined to reside with the scientists, which is one reason why many people are frightened of them. So we see that the virtues of arresting people and doing bad things to them have been expressed in different lands and ages in many different and even contradictory ways, depending on the values thought to be important to the conscience keeping group. Today one of the fashions is to talk of social efficiency.

But this by no means implies that the researcher can find a satisfying explanation of what we officially do about crime in terms of a search for social efficiency, any more that he could explain the very similar behaviour of other

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\*It has been suggested that this is a dogmatic assertion of an Austinian concept of law, (Austin, 1832). I do not think so; I am not concerned here with what the law *should* be, nor with its "real nature" or "essence" in any metaphysical sense. It would be pushing legal theory further than a mere non-lawyer would care to venture to suppose that people would walk to the court, endure a trial, convey themselves to prison and sit there for two years or more in the absence of *force majeure* or an effective threat of it.

\*\*J. W. Mohr has suggested in conversation that the justification may come first and may later acquire fulfilment.

lands and ages in terms of a search for peace, unity, holiness, right, order or fraternity. In fact I should be glad if he was looking for a very different type of explanation altogether; what sort of explanations we are seeking will I hope become clear later in the course of the book.

As for the separate question of what cooperation between correctors and researchers is possible or desirable, I have no intention of treating it here. Christie (1970) has given a first-rate account of this that should be read, not only by all in any way concerned with the role and the function of the researcher in society but by anyone with an interest in the relationship between crime, power and knowledge.

## Section 6

### Facts and assumptions about crime, contra-crime, parole and research in the present study

In Section 1 we said “ . . . the story that we tell is of some men who have been compulsorily shut up for a while in large buildings far away from their families and their work, and who have then been told that they may ask to be let out early. Some of them have so asked . . .”. The time has now come to describe some aspects of the asking-and-letting-out game that we are studying; in other words, the National Parole System.

If we read that act which brought this system into being in February 1959, abolishing the earlier systems, (see Appendix D) we find among other things the following. (My language in this account is not always that of the act itself, and I have added comments where I thought these would be helpful.)\* There is to be a paid Parole Board of between three and five people, including a chairman and a vice-chairman, and they are to be served by officers, clerks and other employees, generally known as the Parole Service. Their headquarters is to be in Ottawa.

Subject to a very few limitations, the Board has the only say, and therefore the final say, in granting, refusing to grant, suspending or revoking parole. Parole means authority, although conditional toleration would be a more precise term, granted to an inmate to be “at large during his imprisonment”. We shall see reasons for this strange form of language later. An inmate means someone convicted of an offence against an Act of the Parliament of Canada and under sentence of imprisonment for it, but this

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\*Since the period studied, a number of changes have occurred. Before assuming that what I say is still true, the reader should refer to Appendix E. There is some discussion of the general tendency of these changes in various parts of the book, but mainly in Section 16.

meaning does not include "juvenile delinquents". Fifty-four people out of our main sample of 832 were under eighteen at time of sentence (1 was fourteen, 4 were fifteen, 16 were sixteen and 33 were seventeen) but were classified and handled as criminals rather than as juvenile delinquents; I don't know if they would have been better or worse off as juvenile delinquents, but at least it shows that the meanings or lack of meanings of official words are stranger than one might suppose; George Orwell's novel "1984" has a discussion of the benefits to government and losses to the individual and to society of corrupting the plain meaning of a language in this way.

The Board makes an automatic review at specified intervals of all inmates sentenced to more than two years, whether they apply for parole or not. At the time of our study about 40% of the penitentiary inmates, nearly all of whom have at least two year sentences, did not apply for parole,\* an omission which some members of the Parole Service regarded as "irrational" or at least as a sign of inferiority. In these cases the review was indeed automatic and parole was not granted; we decided that all the cases included in our sample would be decisions about parole *applications*.

The Parole Board also considers applicants (but not non-applicants) with sentences of less than two years, but these do not usually arrive in penitentiaries unless, for example, so ordered by a magistrate because someone has decided that he (the inmate) is too hot to be held in other institutions;\*\* in our study 21 cases arose, and were excluded from the analysis (see Table 2) whose largest single sentence was less than two years.

With sentences of more than two years the Board has to decide "whether or not to grant parole" but as they have many formally different ways of expressing their decision to decide not now but later, this rule merely means that one day they must make up their minds.

The Board may grant parole on any terms or conditions it chooses. It may provide (impose) any "guidance and supervision" that it chooses; and it may revoke any parole (reimprison any paroled man) if it so chooses. These facts greatly helped me to choose the title for this book.

"The sentence of a paroled inmate shall, while the parole remains unrevoked and unforfeited, be deemed to continue in force until the expiration thereof according to law." Nevertheless when parole is revoked or forfeited the inmate is "to serve the portion of his original term of imprisonment that

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\*See annual reports of the National Parole Board for these years.

\*\*With very few exceptions a man with a sentence of less than 2 years in a penitentiary is either serving a "remanet", i.e. the residue of an earlier sentence after losing parole (technically this is not a sentence) or falls under section 129 sub-section 3 of the Canadian Criminal Code. This deals with people convicted of escaping. Many re-captured escapers from reformatories, etc., are not charged, and nothing is known in detail about the decision to charge or not. However, my comment above is probably a fair summary.

remained unexpired at the time his parole was granted.’’\* The tyrannous illogic of this is interesting. I use those two words precisely, not abusively. A man on parole is not free, or rather, he is slightly free. He is subject to day-to-day orders and controls, often of a degrading nature, (see Studt 1967 for a somewhat similar situation in California).\*\* The justification offered for

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\*Because of statutory remission the exact interpretation of these words is more complicated than it might seem. A man who expected to spend  $x$  years in penitentiary, but was released on parole after only  $y$  years, and later lost his parole, would in fact spend in all rather more than  $(x + 1/4y)$  years plus any new sentence. This is derived as follows:

Suppose a sentence of  $t$  years is imposed on a man.

Suppose that if he does not receive parole, because of statutory and earned remission he expects to spend  $x$  years in penitentiary.

Suppose that he spends  $y$  years, is released on parole, loses it, and then spends a further  $z$  years apart from any new sentence.

The statutory remission on a sentence  $t$  is  $1/4t$

The earned remission after  $x$  is  $1/10x$  (3 days per month)

$$t = x + 1/10x + 1/4t$$

$$x = .6818t$$

The earned remission after  $y$  is  $1/10y$ . After spending  $y$  a man is counted as having spent  $1.1y$ .

On return to penitentiary he is therefore regarded as having a new ‘‘sentence’’ of  $t - 1.1y$

He expects to spend  $z = .6818(t - 1.1y)$

$$= x - .75y$$

He has already spent  $y$

The total time spent is  $x - .75y + y = x + 1/4y$

Thus a man who spent 4 years in prison before being paroled and then lost his parole would spend a year more in prison than if he had never been paroled. He could of course apply for a new parole. At the time of our study this was very rarely done and even more rarely (never, within our sample) granted.

At the time of our study the man who lost parole spent in prison an additional period known as ‘‘dead time’’, between arrest and the official date of revocation or forfeiture of parole, usually at least six weeks. In 1969 the system was changed; a man is now credited with his dead time, but he now loses his past earned remission so that our formula becomes  $x + .32y$

My quoting of these formulae has led to strong criticism, not of the injustice that they represent, but of my accuracy. Accordingly I have had the formulae checked by a former colleague, now engaged on other research in a penitentiary, who together with that penitentiary’s admissions clerk has worked over the methods actually used in calculating the relevant dates for men returned to that penitentiary. It also agrees with the facts quoted in *R. v. Morin*, Saskatchewan Court of Appeal, 1968.

\*\*Irwin Waller has pointed out that the experience of parole for a member of my sample would be very different from that described by Studt. In an as yet unpublished study of men from Kingston penitentiary, he found that many paroled men appreciated their relationship with their parole supervisor, who they felt took a sympathetic, if not otherwise very useful, interest in their problems as ex-prisoners. These are problems that they are under some pressure to hide from the rest of society, who are not in any case very interested except when hostile.

It seems to me that when the forces of contra-crime inflict on a man the status of prisoner, and later of ex-prisoner, it is reasonable to provide people who will sympathize with the man’s plight and problems, just as it is a sign of a reasonable quality of civilization for any man to find such sympathy available to him. There is no need to take tyrannous powers over a man in order to provide such sympathy as he feels he needs. As for the liberty enjoyed by the paroled man, it is true that many of Waller’s ex-inmates did not appear to *feel* themselves to be victims of tyrannous powers. But we must remember that the views of prisoners and ex-prisoners on contra-crime often seem to reflect a very successful mystification process. In any case contra-crime is far from being the only form of exploitation to go unperceived or even denied by many of its victims. But this does not invalidate my point of view, nor that of the reader who has had the good fortune not yet to have been the bewildered victim of these exploitative processes. We cannot call a man free who

this is that he is still really serving his sentence, (and so must presumably expect to be degraded). Yet at the decision of the Parole Board he may be returned to prison to serve this part of his sentence again, and even more because of lost remission, contrary to what lawyers like to call human rights. I have discussed this with members of the Parole Board, the Parole Service, and other people. Many people who take an interest in this subject are shocked at this granting of a power, not subject to appeal, to make a man serve one sentence more than twice. No member of the Parole Board or Parole Service has seemed to be shocked within my hearing, though one of them has told me that he thinks there should be an appeals mechanism. I have not discussed this with all of them and it is possible that a reasonable view prevails among some of the others; but among others the Chairman of the Board has expressed to me strong approval of the existing system. As for the rest, it is clear from conversations, and from the positions they adopt in criminological conferences, that what I see as tyranny many of them see as desirable normality. It is unrealistic to shift the responsibility for these matters to Parliament; this implies a very naive view of democracy in practice. Amendments to the Act have constantly responded to the initiatives of the Parole Board, and have rarely provoked parliamentary discussion, and the whole tendency has been towards greater powers to re-imprison without effort, appeal, redress or justice. (Compare Appendices D and E, and see also Section 16.)

If a paroled inmate is convicted of an indictable offence committed after the grant of parole and punishable by imprisonment for a term of two years or more (nearly all indictable offences are so punishable) his parole is thereby forthwith forfeited. He can apply for another parole. Even if the conviction is after the end of the time on parole, if it is held that the offence was committed during the parole, the former inmate returns to prison to serve the paroled part of his sentence a second time plus the lost remission.

If we turn from the Parole Act itself to the "annexed Regulations" applicable from September 1960, we find that before being regarded as "eligible" for parole a prisoner will usually have been in prison for at least one third of his sentence, or for four years, whichever is the less; in the case of men in penitentiaries on sentences of between two and three years, at least one year will be spent captive. A man sentenced for life but not on a sentence of preventive detention will usually be imprisoned at least seven years. When a death sentence is commuted to a life sentence, the man will usually spend at

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has to seek permission to marry, to leave town, to leave his job, to buy a car, or to incur any major responsibility including a hire-purchase debt; and who may well have his chosen friends, including girlfriends, forbidden to him, as also may be the right to drink beer, wines or spirits. Whether these facts greatly limit his day-to-day activities is not the point; a man who lives subject to these constraints, however lightly used, is not free, and if we think he is, then the concept of freedom has fallen on hard times.



least ten years in prison. However, in nearly all cases the Parole Board has the power to release a man sooner when it decides that there are special circumstances. These are undefined.

In sentences of two years or more, the Board should consider the case within six months to fix a date for considering it a second time. The board should either grant parole, making its decision before the man becomes eligible, or should continue to review the case at least every two years.

One question we have not yet asked. Parole is seen as an alternative to imprisonment. We have a sample of men who might be expected, if it were not for parole, to spend between them a certain number of man-days in prison. But because of parole some of them will spend less time in prison, though others will spend more. What proportion of the total is saved in this way?

Our study sample consisted of 406 men, of whom seven were serving life or indeterminate sentences. We have to leave these seven out of our discussion for the following reason. Our sample is based on final parole decisions. Now a man on a life or indeterminate sentence only receives a final decision if this is a decision to grant parole. If he is refused his case keeps coming round until he dies, is transferred to a mental hospital or otherwise ceases to interest the Parole Board. Thus because of an administrative arrangement our sample contains no lifers or indeterminates unless they receive parole, and it would obviously make the sample unrepresentative to include our paroled lifers and indeterminates in those circumstances.

Our remaining 399 men were between them expected, if not paroled, to spend 330,992 man-days in prison. (This figure allows for remission etc., and represents an average of 830 days per man.) 263 of them were refused parole. For them there was no saving by the Parole Board of time spent in prison. 37 men were granted parole and later lost it, and they, as we explained above, actually spent more time in prison than if parole had not existed. In this way they lost an estimated 6,124 man-days, or an average 166 days each.\* The remaining 99 men were granted and kept their parole. Before release they spent between them 67,204 man-days in prison, or an average 679 days each. If not paroled they would have spent in prison 106,401 man-days, or 1,075 days each. Thus the net saving to our sample, excluding lifers and indeterminates, was 33,073 man-days, or almost exactly 10 per cent of what they would have spent if there had been no parole system.

Now as we have seen, parole applicants represented at that time about 60% of those entering penitentiary. In this study we have no data on the time spent in prison by those who did not apply for parole. On the one hand a man undergoing a long imprisonment has perhaps more motive to seek parole; on

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\*This does not include "dead time", (see footnote, p. 27) which probably accounted for at least another 1,500 man-days; but because we cannot be specific about this period, we have left it out of account.

the other hand he might perhaps see himself as less likely to be granted parole (though the contrary is in fact true; see table 94 lines 15 and 16). At any rate, it appears that our saving of 10 per cent of our sample's time in prison, other than life or indeterminate sentences may represent perhaps about 6 per cent of the time in prison of the whole determinate-sentence male penitentiary population. (The true figure for our sample will be less than the 10 per cent that we have quoted for the following reason. In this study anyone who kept parole for 3 years has been treated as having kept it altogether. For an explanation of this decision see Section 12. But in fact a few of our sample will have kept parole for more than three years and then lost it. For them there was an actual increase of time spent in prison, yet we will have credited these few people with long savings, thus inflating our total. If any of the 37 men who lost parole were later re-paroled this might again represent a saving or a loss according to whether they kept or lost their second parole.)

This saving of say 6 per cent of the time spent in penitentiary is welcome, but the figure has one possibly strange aspect, though here I am speaking merely of a personal impression. The substitution of six per cent of one phenomenon by a supposedly newer alternative has the air of being an experiment or a pilot study, meant to lead if successful to a large increase in the new alternative. Now certainly from time to time members of the Parole Board and Parole Service speak of their activities as due to expand, but I have never heard any of them speak in terms of an expansion from modest pilot experiment to full-scale operation; rather I have formed the impression that they speak as though they were already making a major contribution to the problem of not putting men in cages. Reducing imprisonment by about three weeks per year, although welcome, is not my idea of a major contribution.

This concludes our description of official powers and nature of the Parole Board. The object of this study is of course to move from this sketchy understanding of their official powers and nature part of the way towards an understanding of what in fact they do to whom, in what circumstances, and why. The reader may reasonably think that in order to do this we should have some understanding of the aims and reasons behind penitentiary system; indeed behind the whole contra-crime system itself, of which such things as penitentiaries and parole boards are a part. This would be nice; but we have no such understanding.

We might suppose that the custom, universal in nearly all societies, of choosing certain people in certain circumstances, officially calling them criminals, and officially doing nasty things to them, had a clear visible purpose, and that the people selected to be called criminals differed in some clear visible way from other people. All justifications and reasons known to me for the choosing, labelling and annoying of criminals start from this assumption. Most research into crime starts from such an assumption. On one's first day as a criminologist it is a very reasonable assumption.

But it remains an assumption, and as I shall argue below, an assumption that should be dropped, or, at the very least, much more consciously held in mind as a droppable assumption, in serious study in criminology. Turk (1969) has documented the lack of serious evidence that in any relevant, consistent, describable way the people that we treat as criminals are different, other than consequentially, from those we don't so treat, or that their behaviour, their needs, and society's objective needs with regard to them are different from those found with other people.\* If our reaction from this conflict between "what everybody knows" and what research finds or fails to find is to reject research (a sociologist is a man who spends ten thousand dollars to find a whorehouse, etc.) consider just what it is that "everyone knows". Criminals are wicked (and we are rather good) but they're not really wicked they're sick (so I suppose that we're not really good, we're just healthy) and in any case it doesn't matter which they are because the things they do are dangerous and inconvenient (and what everyone else does is always safer and more convenient) and we have to teach them a lesson, which they won't learn because they're incorrigible, and we have to integrate them back into the community, and also symbolize society's rejection of them. The young ones are the worst and we must spare them the shame of being treated like real criminals. Now some of these *clichés* may well be true, or may well not be, but they cannot all be true at once; we shall not believe anyone who asserts too many of them together. They are rather like proverbs; you can find whatever you want. Which ones the powerful members of a society believe are true will surely make a difference to what that society does; yet human society as a whole, over nearly all of its geography and history, has done very similar things in the name of the law and has offered whichever reasons happened to be in fashion at the time.\*\* When the reasons change and the activity remains, the reasons begin to look like excuses. I do not mean excuses offered by a contra-crime industry imposing itself on an unwilling public. The bulk of the public seem to want more, not less, contra-crime, and it is of them as much as of the contra-crime industry that I say that their reasons look like excuses. In our own age (perhaps it is the age of mystification) the reasons are advanced

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\*Obviously there are *some* differences between those citizens because of whom the police are called and others. We do not know what these differences are, and Turk's observations only apply to those differences that we should normally call criminal (or criminogenic). We must be on our guard, since apparent differences are often *imported into* their studies by researchers who then discover what they have imported. Turk's case is detailed and repays detailed study.

\*\*It has never made any attempt to find out which assumptions are true or false, even in this age of social research, which in nearly all cases *starts* from an assumption that crime is caused by the different nature or circumstances of criminals. As Turk (p. 25) puts it "Efforts to determine causes of criminality have foundered on the fact that criminality is not a biological, psychological or even behavioural phenomenon, but a social status . . . Criminality is determined by what the authorities do, rather than what they claim."

almost proudly in self-contradictory pairs such as justice and rehabilitation\*. But as George Bernard Shaw (reprinted 1961) says,

Now if you are to punish a man retributively you must injure him. If you are to reform him you must improve him. And men are not improved by injuries. To propose to punish and reform by the same operation is exactly as if you were to take a man suffering from pneumonia, and attempt to combine punitive and curative treatment. Arguing that a man with pneumonia is a danger to the community, and that he need not catch it if he takes proper care of his health, you resolve that he shall have a severe lesson, both to punish him for his negligence and pulmonary weakness and to deter others from following his example. You therefore strip him naked, and in that condition stand him all night in the snow. But as you admit the duty of restoring him to health if possible, and discharging him with sound lungs, you engage a doctor to superintend the punishment and administer cough lozenges, made as unpleasant to the taste as possible so as not to pamper the culprit. A board of Commissioners ordering such treatment would prove thereby that either they were imbeciles or else that they were hotly in earnest about punishing the patient and not in the least in earnest about curing him.

Now in the case of a person who accepts the point of view I am putting forward we must distinguish where possible between his general human reactions to this situation, that is to say those reactions relevant to his daily life, and the more limited set which apply to his activities as a researcher. My own reactions outside research should not strictly be relevant here, but since (as we have argued in Section 1) it is so difficult to keep these out of criminological research, it will be safer if I mention them briefly before passing on to the strategies and techniques of research implied by this viewpoint.

The fact that people called criminals do not voluntarily undergo the sentences imposed by the state, but endure them compulsorily\*\* and the fact

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\*Quimet (1969) is a splendid (and official) example of the reconciliation of contradictions simply by putting them between the covers of the same publication. Hegel would have been mad with jealousy.

\*\*There is some over-simplification in this statement. Consider the prisoners who help to fight a prison fire and then return to their cells. But at what point acceptance of past, present and future *force majeure* becomes voluntary collaboration is hard to determine. As a generalization, sentences are compulsorily imposed, not voluntarily undergone. By coincidence, while I was writing this footnote (14th April, 1971) 500 inmates of Kingston Penitentiary had shown their power to destroy much of the "Big House" (the main cell block) and take warders as hostages. As far as I know none of the revolted inmates demanded his liberation or the abolition of the prison. Apart from Red Cross type demands directly associated with the revolt itself, such as maintenance of medication and food supplies, the revolters main demands seemed, at least according to one newspaper account, to be about "the lack of proper rehabilitation methods available to the prisoners both inside the penitentiary and on the outside once they were released. They also complained about the tighter security and the closed-circuit television scrutiny they will

that other people are forbidden to do to each other the things that the state so expensively does to us in the name of justice and corrections, suggests to me that these things clearly do what most people in most contexts see as bad. But if so, before I can feel enthusiasm for these activities I must be shown some sort of good in them or in their results that will at least probably outweigh the bad and the expense. Now doing nasty things, even to bad people, makes no appeal to me as a good thing in itself; so if evidence for its good results is not found, and if the arguments for them have been for thousands of years excessive and self-contradictory, like the searchings of people who are convinced that our contra-crime activities *must* do *some* good, but are making wild guesses as to what, this seems to me enough evidence for everyday life, though not necessarily for research, that contra-crime does no great good and a great deal of bad, and has done this for so long that it is naive to suppose that it seriously exists to do *any* of the good things most often alleged in its defence. Of course, this is not to say that we wouldn't be better off if we did something about some of the objective problems that we confuse with "crime"; if instead of chasing, accusing and harming one man for being involved in the several-person interaction of which his share may later be called a crime, we thought, not in individualistic or moral but in practical terms, about how to reduce the frequency or unpleasantness of that interaction, or even about whether the agents of the state were the best people to deal with it. We could even treat each type of problematic interaction on its own merits instead of lumping together under the rag-bag heading of "crime" all those cases where we don't know what to do and decide therefore to blame and annoy someone.\*

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\*These last few pages are the first place in the book where I have written as though society or its institutions had aims, reasons, intentions and excuses, and it may be as well to specify what I mean when I apply such individualistic terms to a collectivity. Strictly, of course, needs, aims, intentions, reasons, rationalization, excuses and such-like always appertain to individuals, and to

(continued on p. 34)

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have to live with at the new maximum security prison at Millhaven, about 10 miles west of Kingston.

"The old penitentiary, which was built in 1883, is being phased out and by this fall more than half of the present inmates at Kingston Penitentiary are expected to be transferred to Millhaven.

"This new prison is equipped with closed-circuit TV cameras which will keep all the cells under constant watch. Yesterday the prisoners complained that what little privacy they now have at Kingston will be lost once they are moved". (The *Globe and Mail* Friday, April 16, 1971, page 2.) "The prisoners also voiced disapproval over the Parole Act, recent transfers to nearby Millhaven prison and "false" statements from arresting officers at trials." (The *Globe and Mail*, Friday, April 16, 1971, page 29.)

Thus the prisoners seemed to be even more concerned than the authorities about the so-called "just and efficient runnings of the system" which it was beyond their dreams to abolish; but even so I imagine that prisoners see their incarceration as compulsory; and that is as far as we can examine this complex, fascinating and important topic within the limits of the present study. (Note that Kingston penitentiary was built in 1835, not 1883, and that the existence of closed-circuit television surveillance of cells at Millhaven has been denied; but neither of these facts affects my arguments in this note.)

How then will this point of view affect research? It will make it unlikely that we shall seek any opportunities to examine "causes of crime" or "effects of treatment", those two will-o'-the-wisps that have for ninety years led most criminologists to drowning. Instead we shall in all situations look at processes that can be observed to occur, stating very clearly the rare occasions on which acts or processes are assumed by consent rather than observed. And our studies of values and criteria will either be of those which we assert on our own responsibility, or of those which we can infer from the acts and processes we observe, but not usually the assumed "aims of society" or officially claimed functions of institutions.

A consequence of keeping to what we can observe is that we shall study the criminal as the recipient of certain observed official attentions and acts, rather than as the initiator of certain undefined unobserved, imputed behaviours which sometimes cause and sometimes fail to cause official responses. This is not, in doing research, either to deny or to accept what any other researcher or any official may say about criminal "behaviour" and official "response," although some such assertions may appear servile or laughable enough at the level of everyday life. We simply wish, as a research strategy, to avoid unnecessary, unprofitable and possibly destructive entanglements in such viewpoints.

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*(continued from p. 33)*

talk of them in connection with a group must always be either a shorthand or an anthropomorphic metaphor. In everyday life this economy is well worth while; and so it is in research, but it is dangerous if done without clarification.

We tend to suppose that most individuals have reasons (if we include the ambiguous concept of an unconscious reason) for most of their activities, even when we have no means of estimating what these reasons are. But in some cases we go beyond this assumption, and we feel that we have very strong evidence of what a man's intentions are at a given moment, even if he does not state them, or perhaps denies the intentions which seems so obvious to us. We feel that such and such an act, carried out in such and such a way, in such and such a context, almost necessarily implies and communicates a certain intention.

Now sometimes a society carries out a similar type of action on a sufficiently stable basis for it to seem a regular feature of that society rather than an incidental activity of some of its members. By a similar type of action I mean one which, if done by an individual, would lead us to impute needs, aims, intentions, etc. to him with a high degree of confidence; and in such a case and (I hope) in no other I have as an explanatory device spoken of the needs, aims, intentions, etc. of the society or the institution, formal or informal, which carries out the action.

But just as our unhesitating, obvious "seeing" of a man's intentions may occasionally be "wrong" (in the sense that a larger segment of his activities might be better explained by imputing a different intention to this particular act) so many members of a society may be wrong, in the same sense of the term, in their unhesitating, obvious seeing of the reasons for some of the actions of their society and its institutions. One of the main bases of this book is a questioning of the "true aims" of society's "responses to crime". It is interesting that when we change our beliefs about an individual's (or a society's) intentions, the new belief, which we may at first strongly reject, quickly comes to seem as certain and natural as did the old one.

## PART II

Which men do they  
provisionally let out early,  
and why?

## Section 7

### The nature of the information used

After all these generalities, at last we come to our facts: to the things that the researchers saw on files and recorded on data-sheets; but what did they see, what did they record, what did they think it meant and what did they do with it all?

Suppose, to take one example as an illustration, that we read on a file the statement, on a classification officer's report, that a prisoner has completed his education up to Grade 7. We may regard this if we like as information about the prisoner's education. It is in fact information about many other things as well, as we shall see, but if we are greatly interested in his education it is on this aspect of the statement that we shall first concentrate; and in this case we are up against a host of difficulties. Do we know whether the statement is true or not? What do we know about the education of someone about whom the classification officer has written "educational level unknown" or the possibly very different case where he has written nothing at all? Or what of the case where he has written Grade seven and a letter from the prisoner's parish priest mentions Grade nine? Whenever we try to treat marks on paper as measures or estimates of underlying facts, we face problems of precision, validity, reliability and missing data. When the marks on paper arise as a result of a routine process, not carried out for a specific scientific purpose, nor by a specific scientific technique, these problems of precision, validity, reliability and missing data may become overwhelming. To crown it all, we may later find that we had no very good reason for having been more than superficially concerned with his real level of education in the first place. For ninety years scientific criminology has asked what makes criminals commit crimes, and has mainly sought the answer inside "the criminal's



mind'', his body or his friends;\* and after ninety years of failing to find an answer there or elsewhere we are now coming to understand the inherent self-deception involved in beginning with this question in this form, and to turn our attention to more profitable and less specious starting questions.

Fortunately the statement about the prisoner's education written on the classification officer's report tells us much more than we have so far mentioned. It tells us very literally what educational level the classification officer records the prisoner as having achieved; and this is very likely the educational level which will be attributed to him by many of those who deal with him in prison or on parole. Regarded in this light, the category "Grade completed — unknown" and the category where no entry is made do not become missing data; they become very important attributions of ambiguous status, or absences of attributions. The present study is based, as is so much modern criminology, on an interest in the criminal, not as a strange nasty man who has done a strange nasty thing (which may well sometimes be as undeniably true of him as of many of us; our neighbours of course, not ourselves) but as a person about whom strange, nasty decisions have been made, over whom strange, nasty powers have been taken and to whom strange nasty things have been done. So the data of importance will not be estimates of what it is like inside his head, his body or even his social world, but who has written or said what things about him, to whom else; in whose power he has been; where and to whom he has been sent.

So we record, not the nearest we can get to his real educational level, real criminal behaviour or real future employment prospects estimated as well as possible from the data on file, but simply and directly what education the classification officer attributes to him; what criminal behaviour the police attribute to him in writing to the parole board; what some parole service officer attributes to him in the way of employment prospects; these things not being treated as somewhat unreliable estimates of facts, but as relatively precise statements of a different kind of fact.

This way of looking at the data becomes even more important in the case of "Marital Status attributed to Applicant by admission form." (Table 21). Even in relatively simple cases a man may easily be supposed to be married to, separated from or in a "common-law union" with one partner or to be married to, in a common-law union with, or not seriously involved with, another. The decision recorded is to a great extent a decision about who may

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\*This is not to ignore the important sociological studies of crime in relation to poverty, unemployment, racial discrimination, class biases, sub-cultures, and so on. But many of these studies merely catalogue the circumstances where crime is alleged to be frequent or rare, without explaining why; and in general the explanatory studies still see crime as one man's act proceeding from one man's head; the so-called sociological approaches are mainly models of how his head is supposed to have been influenced. See Turk (1971) for a discussion of the lack of genuinely sociological approaches to the *explanation* of criminality.

visit whom; and alleged length of co-habitation is only one of many criteria for being accepted by the prison authorities as a de facto wife.\* They are presumably less likely to grant this status to a woman they suspect of smuggling drugs to prisoners. But this is not a study of the phenomenology of official records, just a statement of our reasons for regarding our data as attributions and decisions rather than as estimates of simple facts. Incidentally, it is extraordinarily difficult to convey the difference between the two types of fact in any tolerable form of words. Most of Tables 3-92 have extraordinarily long, clumsy titles, which I regret; and even so, a title like "Size of Birthplace (if in Canada) attributed to applicant by admission form" sounds more as though the Table summarizes a set of simple real facts about the prisoners' birthplaces while being over-scrupulous (and fairly insulting) about the source of the evidence, than that it summarizes a series of attributions of origins by various people in various penitentiaries' records departments. This clumsiness and imprecision of language in this sort of context may arise partly because western civilization has laid enormous emphasis on precise descriptions of things and of relations between things, and has remained very vague (though often poetic and often perceptive) about people and the relations between them. It is perhaps because of this disabling lack in western languages that so many people who want to "re-humanize the social sciences" are hostile to logic. Thus the attempt, even in the very small matter of the titles to a set of tables, to talk precisely about who said what to whom about whom else, leads to ungainly length, ugliness, and perhaps failures of communication.

To return to the details of the National Parole Service files, they contain two separate types of document. On the right side of each file is all correspondence to and from the prisoner regarding his application for parole, correspondence to and from those persons making representations or enquiries on his behalf, the prisoner's certificate of parole if granted, and any amendments made to it.

The left side of the file is composed of reports and assessments sent in to the Board. From the time of incarceration onwards there are many persons who will make assessments of the inmate: among the Penitentiary staff themselves, classification department officers, custodial authorities, and possibly psychologists; in the National Parole Service there will be

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\*Thus if a pair haven't been together a certain length of time, the authorities may wreck their relationship and the well-being of a number of children even more than they already do by imprisonment, by making visits difficult or impossible. We should always bear in mind the extent to which imprisonment, or any other contra-crime, may punish indifferently anyone whose life is close to that of the prisoner. See Morris (1965). Morris's social work orientation usually prevents her from even questioning the imprisonment of most of the men she is studying, and often leads her to regard any statement by a prisoner or his wife as necessarily wrong when it differs from any official version. This renders rather more impressive her fairly thorough listing of some of the ways in which you are punished for being related to a convicted man.

Representatives, Regional Representatives, Parole Analysts, and their Supervisors, etc. In general, documents from these sources make their appearance on the left hand side of the file in the order just mentioned.

Most of the files are in English, but files about prisoners in the Quebec penitentiary of St. Vincent de Paul and its subsidiaries may be written in English or French. For the most part all the French-language forms correspond directly with English counterparts or are meant to fulfill the same function. We did not record the language of each file, a decision that I now regret, though we did record the stated language of education of each prisoner.

The main forms and documents from which we were able to extract codifiable, comparable data were the following:

*Admission Form:* The Penitentiary Admission Form is completed by the Records Section of the penitentiary on the arrival of every inmate, either on a new conviction or a loss of parole. This report begins the inmate's file.

*Criminal Record:* The Criminal Record form for the inmate is sent to his penitentiary by the Royal Canadian Mounted Police Records Section. The record form lists the majority of court appearances, charges and dispositions of the inmate, but not all. Generally, the offences recorded will include all indictable Criminal Code offences. (Taking a Motor Vehicle is a non-indictable offence which is often included) all known Highway Traffic offences, Drug offences, and Liquor Control Act offences, and some Juvenile Delinquency offences.\*

*Other Police Letters and Forms:* On these documents, reports of offence details are not given in any standardized form, and in many cases the information that interests us will be found after searching several police forms. The most common of these are the provincial police or local police offence reports in which an extensive report of the offences can usually be found. Sometimes the police department may supply this sort of information in the course of a letter, sometimes on a form.

*First Application for Parole:* Parole can officially be granted or refused on an application by a prisoner or by someone else on his behalf, but in practice the National Parole Board expects those seeking parole to submit their own application, including their outline plans for their future if parole is granted. The application is supposed to be submitted five months prior to the "Parole Eligibility Review date" in order to allow time for an investigation to

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\* A large part of this list is not part of what one would normally consider a "criminal record". However, all that concerns us here is that on these files, on these documents, these "non-criminal" categories occasionally appeared. The list is more a catalogue of charges and convictions for fingerprintable offences than a "criminal record" in any stricter sense.

be made. If the inmate does not apply but someone else does so for him, then the inmate is interviewed and asked to file his own application.

*Community Enquiry or Pre-Release Report:* This assessment of the prisoner's home and community is initiated by the Regional Representative or by the National Parole Board. The assessment is made either by the National Parole Service officer, or by the representative of another probation or social service agency. A general policy is that the intended supervisory agency makes a pre-release report as well. In some cases an investigation is carried out several times, in which cases more than one report will be on file. For our purposes all such reports are reviewed together when recording data.

*Decisions:* All official Parole Board decisions on the granting or refusing of parole are noted on the file. Many decisions are taken that do not concern the present study because they are not final (the decision not to decide) or because they do not refer to the intended permanent release of the prisoner. Such decisions include

- Decision reserved
- Decision deferred
- Parole denied at this time
- Temporary parole
- Short parole

Decisions which do concern us here are:

- Parole granted
- Parole denied
- Parole with gradual release\*
- Parole in principle\*\*

*Classification Report Upon Admission:* The main purpose of this report is said to be "to quantify certain factors with respect to the personality and life experiences of groups of inmates up to the time of their admission . . . to reveal the most productive methods of rehabilitation. Also it may reveal factors which will indicate the types of inmates who have the best prospects of making a permanent social adjustment when released into the community." (Dominion Bureau of Statistics, (no date)). In general, the Classification

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\*Three men in our study sample, and none in the replication sample, were granted parole with gradual release. They survived the "gradual" period without any reversal of this decision, and we treated their parole as beginning with their "full" release on Parole.

\*\*I.e. the application is seen as "suitable" if "suitable" outside arrangements can be made. Five men in the study sample, and two in the replication sample, were granted parole in principle and were in fact released on parole.

Department will have several reports in the file pertaining to "progress", "behaviour", "discipline", etc.

*Field Representative's Report:* The Field Parole Officer interviews the inmate during the fifth month prior to the parole eligibility date. This report is seen as an opportunity for assessment to be made by the Parole Service themselves, while Classification Reports emanate from the custodial authorities.

*Cumulative Summary:* When the complete submission of the Field Officer is received by the Parole Board, the Parole Analyst (i.e. a headquarters official) reviews the entire case to ensure that all interviews and reports have been made in an acceptable form and that all factors seen as pertinent are taken into account in arriving at his conclusion and recommendation. The Parole Analyst may decide to endorse the recommendation or, if he does not agree with the recommendation, to prepare a report; in this case, if the Parole Analyst's Supervisor agrees the Field officer may be asked to prepare further reports or to "reconsider" his recommendation. Parts I and II of the Cumulative Summary are the most common segments on the files we studied. These parts are sent out to the agency conducting the community assessment. Part III represents a summary of this inquiry. Part IV is for the use of the Regional Representative for his additional comments on the case, or his recommendation if it differs from that of the Parole Service officer.

*Release Form:* The Penitentiary Release form is a series of statements by the custodial authority about the activities of the inmate while incarcerated, as well as a repetition of data on the Admission form. In addition, there will be other statements about the inmate's participation in trade training and academic progress, which may not have been dealt with in Classification Reports.

*Parole Agreement:* The Certificate of Parole consists of two parts. It is an order to release and an "agreement" on specific conditions for the parole. Essentially the agreement is a "contract" between the paroled inmate and the National Parole Board. The main objective is said to be "to protect the fundamental civil and human rights of the paroled inmate\* and to make it clear that in spite of the conditions and restrictions of the release, the Board's approach and attitude in respect to rehabilitation are positive." (National Parole Board, (1967) ). Another objective is said to be to define the authority of the Regional Representative and of the supervisor over the paroled inmate and to delineate their roles.

*Parole Agreement Amendments:* Any of the conditions of the Parole "Agreement", except Clause 8, (an order to abide by special conditions, such as total abstinence from alcoholic drink) may be modified or removed by the

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\*But see section 5.

warrant. The Regional Representative may also at his discretion re-impose, add to and impose additional restrictions. Many of the Parole "Agreement" Amendments we will encounter will be due to the parolee's having moved to a new supervisor's area.

So from this mass of documents, forms and reports on the left, correspondence on the right, what information did we draw and record? Some of it is listed in Table 1,\* and summarized for the sample but not for individuals in Tables 3-92. These are in a sense the simplest data on the files. We find what we might well expect to find where one bureaucracy has temporary custody of a group of men and another bureaucracy is collecting information on them. There is the same heavy concentration of statements on dates, ages, decisions, places, processes and official states and statuses long past as well as recently past, including marriage and religion; a little, but not much, on the events and interactions which are alleged as the reasons for the man's incarceration (the "story of the crime" does not seem to be greatly needed by those who claim to "defend us from crime") and then again a stack of information on processes, opinions (nearly everyone's but rarely the prisoner's and never his supposed victim's) and judgements on a host of topics, but mostly semi-moral judgements, by the prison authorities or the National Parole Board. All in all, they are much like the data available to most researchers who decide to study people by studying files.

However, there is more information on the files than is indicated by Tables 1-92. One very important set is the variables which we recorded and then decided not to use. As mentioned in Section 4, the decision not to use them was taken after all the data had been coded a second time for a check-sample of 57 Cases; and the variables rejected were those where the first and second coding failed to agree in too many cases. Now it is inaccurate to regard this sort of disagreement as simply representing errors by the data-coder. In any study, including this one, there will be some of these, but there is evidence, from the high level of agreement between our 57 pairs of codings on other variables, that most of the coding in the present study was probably carried out with a high degree of accuracy. Thus we have to seek some other explanation for most of the discrepancies that led to the rejection of these variables.

In many cases it is reasonable to suppose that the information on the file can bear more than one interpretation. In general, the data were not literally copied, but "coded", or in other words interpreted. Now we have tried to specify operational rules for this process. These rules are chosen by the researchers and thus to some extent subjective; but once they are chosen, it is hoped that the translation of the information on the files into a set of data on

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\*All tables and equations have been gathered together at the end of the book for convenience.

punched cards approximates to an objective process. However, sometimes the words on the files resist by their variability the imposition of formal decision rules, and the approximation to an objective process may be very rough. In this case we arrive at data of low *reliability* (in the technical sense). Usually data of this sort are simply regarded as a nuisance, but it seems to me that they have implications which go beyond this. We are not talking about data which *appear* to be vague; on the contrary, we have words which appeared to yield a clear enough meaning to be included in the study, but which also appeared to yield two contradictory meanings when coded on two separate occasions.

Now if such data appeared like this to a coder who was spending most of her working day on this work, and whose main aim was precision, how will they appear to those people who handle the file in the everyday work-situation of a parole board? We shall not discuss here the relationship between information and decision in everyday life, but it is worth noting that information which appears clear but in fact means different things on different days to a researcher may possibly appear clear but in fact mean different things on different days to those people who have access to it while they make decisions.

A third set of information on the files was what we decided at once, or at an early stage of the study, not to use. Some items were extremely rare, or so common that their absence was rare. Certain verbal commentaries defied standardized coding to an extraordinary degree. We had expected a certain stereo-typing of style to occur where certain people had to write appraisals of the prisoner, but in the first fifteen files studied the unstandardized reports in essay form (such as in the Community Enquiry) applied more than five hundred adjectives to the inmate; and these were not easily classifiable into any smaller number of variables. It seems impossible that this mass of incommensurable words should be of any use in helping decide whom to keep imprisoned or to whom to allow partial freedom, but our sole concern here is that it is equally impossible to record and analyse such data.

One fact that may seem very important we have not used. This is whether the prisoner passes most of his sentence in a maximum, medium or minimum security institution, a fact which will probably be closely related to how far he is perceived as "good" or "bad", "safe" or a "threat to society" and thus parolable or not. All the members of our sample were "admitted" (an amusingly modest word) to the main penitentiary "serving" their province, and this is always a maximum security institution. At later stages they may be transferred once or many times to, from and between this main penitentiary and other institutions in the province; the decisions about this, largely initiated by the classification officer, depend on his judgement, but also on the logistics of warehousing human bodies, on trade training or work decisions, on disciplinary or atmospheric situations in different prisons, on medical,

psychiatric and bureaucratic facilities or simply on the fact that an inmate is shortly to be released. It is clearly recorded where each man was "admitted" and whence he was released (although in our sample, of course, we only knew this latter for the men who received parole) but where a man was at any intermediate time was not always quickly visible (though it usually could be found by a determined search). Even if his particular prison at a particular point in time was in fact directly obvious to the Parole Board when they studied such a file, we still did not know which point in time was the relevant one; nor whether a man who was suddenly removed from a minimum security institution was viewed as a security risk or merely had toothache. So reluctantly we decided not to use this information. As it turned out, we have probably lost something, but not much, by this decision.

The reader will notice that the data that we have used have been almost entirely drawn from the documents on the left hand side of the file, and he may wonder why more attention was not paid to the right hand side of the file, mainly the letters from outside the penitentiary written to seek a grant of parole for the prisoner. The letters that came from public citizens of "good repute" (clergy, doctors, lawyers, etc.) were too variable in the information contained to be useable. Those from wives, mothers and so on were more consistent; those that we read always showed an anxious willingness to secure the loved one's or bread-winner's release by writing any words, and even if possible believing them, no matter how degrading they might be, that might placate the angry god. But they didn't know any magic words. All alike received similar formal, meaningless, insultingly remote replies.\* But one can see the problem. How does one write to a wife to say that her husband has been locked away from her, that you have the power to release him, and that you will probably decide what to do in about a year's time?

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\*I shall not quote here any of this correspondence since I do not propose to reveal confidentially expressed private distress. So if any reader chooses to disbelieve my description of the National Parole Board's replies, I shall offer no evidence to refute him. Before disbelieving me, however, he should know that the first two researchers other than myself to read any substantial number of these files expressed loudly, spontaneously, and unknown to each other, their disgust with these replies. One of them mentioned the matter to a third worker who agreed strongly. So did I.



## Section 8

### Summary of the information used, up to release on parole or its refusal

In this Section we consider certain facts and figures about our sample: those facts and figures commonly but misleadingly known as the basic data. In terms of what we actually did in this study, the real basic data would be the facts that we have just discussed in Section 7, presented as we first recorded them: that Bill Bloggins, for example, is stated on his admission form to be 32 years old and a Catholic, and many other simple and complicated things that people have entered on forms or written about him. But from all these basic data it is easy to derive certain head-counts, to find out how many people said by the records section to be thirty-two years old, or Catholics, we have been studying; and it is these summary statistics (the wrongly named basic statistics) that we shall now be studying.

Why should we wish to know these things? Why is it of interest to us that 41 per cent of our sample were in St. Vincent de Paul penitentiary for the Province of Quebec, (Table 5) or that 28 per cent of our sample were reckoned likely to meet "alcohol problems" on parole according to various documents (Table 62)? The answer given by classical statistical theory would be that the people we have been studying formed a random sample from a larger population of interest; that we would not necessarily care what percentage of our sample come from one penitentiary except that this figure was also an estimate of the corresponding percentage for the population in whom we were interested.

I do not think that in the social sciences this classical statistical explanation is often true; certainly not in the present study. If the reader looks back to Sections 3 and 4 he will see that a number of factors defined the choice of whom to study. There were the deliberate restrictions of date, nation and

type of institution, type of parole decision and sex of inmate. There was an orthodox randomization process, and a later rejection of certain members of the sample, with an accompanying redefinition of the population.

Now in strict logic it would be possible to consider only the random part of the choosing as "sampling"; all the rest would be part of "defining the population of interest." But this seems unrealistic. If we had happened to choose some other years, nation, or type of institution, or exclude a different set of "exceptional cases," for example, would we really feel that the whole subject of the study had changed? Are we only seeking to understand the phenomena as they apply to our narrowly and rigorously defined population, or in some wider sense? It seems to me that our interests are wide, and that the limitations of date, nation, institution, etc., were imposed for practical rather than intellectual reasons; imposing these limitations must be regarded as one stage in the process of selecting our sample. Perhaps instead of the two-layer, population and random sample, model, a three-layer model of population of interest, non-random sub-population of study, and random sample of this sub-population would fit the case better.

But in this case we are not saying that 41 per cent of the people that interest us were in the Quebec penitentiary. We are making the looser statement that 41 per cent of the people it was practicable to study were so located, and the question returns, why should we want to know this.

The reasons are two. In the first place, research of this sort does not exist in isolation from other studies or from general thinking on the issues involved. If the author, or a reader, notices that some relationship that he would have expected between two variables does not appear to exist in this study, he will wish to know whether this is due to the distributions of the variables in this study. The same is true of any relationship that has appeared to exist in other studies but not in this one. Or if some characteristic is held by current thinking (research, administrative or lay) to be important, we shall want to know how often it actually occurs.

Secondly, the figures may start trains of thought. When we see that 38% of the applicants were said to have been drinking shortly before the time of their offence (who said it?) (see Table 83) or 55 per cent of them unemployed at that time (see Table 80) we may be given to think. *What* we may be given to think is not clear; but if a hundred people think about these figures, even if most of us think a cliché or a myth, some of us may have a more valuable thought. For this, it is sufficient that the thought-provoking figures should be true\* and should relate to a reasonably defined sample; it is not necessary that

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\*The thoughts and actions generated by a false finding may be very interesting, but there is a greater risk of their being inappropriate or harmful in the long run. See for example Eysenck (1965) for a popularized but useful explanation of how Freud's insistence on the dangers of "symptom-substitution" has led to a reduction in the relief of suffering.

they should lead to secure inferences about some uniquely defined original population of interest.

Tables 1-92 are the "head counts" that may be of interest. Table 2 is based on all the files that we handled or considered at any stage of the project, apart from those chosen for the pilot sample. Tables 3 - 92 are based on the study sample (see Section 4). I do not propose to point out particular features of these tables in the main body of the text, although Tables 5, 6, 14, 17, 21, 22, 23, 27, 37, 38, 40a, b, 41, 47 and 49 carry a certain amount of discussion attached, which should be read as text even by those not greatly interested in the contents of these particular tables. But this is not a book where the reader is advised to treat the words as complete in themselves, nor the numbers in the tables as being as avoidable as the photographs in a travel book. The meanings of tables are not made clearer, even for the least experienced reader, by changing neat blocks of figures into loose strings of words, and so the tables contain a great deal of matter not repeated in the text.

## Section 9

### The relationship between the granting or refusal of parole and individual items of information. Parole selection as a purposeful or as a bureaucratic\* activity

In later sections we shall be looking for groups and patterns of variables which "predict" the granting or refusing of parole, and the keeping or losing of the partial liberty which parole confers (this word "predict" will be discussed in those sections). But before doing this, in the present section we shall look at the relationship between individual variables and the granting or refusing of parole.

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\*I am not using Weber's (1922) definition of bureaucracy, but something nearer to the colloquial use of the term, though rather more precise than this. Imagine an organization that is supposed to have an external, objective goal (see footnote to page 53). For example, a hospital is supposed to cure sick people. Now some of its activities may well be directly aimed at this end, but they cannot all be. For it will be found that such an organization has to undertake certain activities in order to keep itself in existence, and to gratify at least certain minimal interests of its members; and that it may well act also under the pressure of internal forces and conflicts. Now we can call these activities bureaucratic, though, since they will be found in all organizations, we shall only call the organization itself bureaucratic when these activities not directed to the external goal grow to such a level as to impede and not to promote that goal.

Note that even when almost no activity of the organization is being effectively aimed at the external goal, many individuals in the organization may still sincerely be pursuing these goals. A health-service so bureaucratized that it promoted no-one's health might still contain dedicated and able doctors. But in this case, either their activities would be simply blocked by the inertia or policy of the organization, or they would be based on misinformation or irrelevantly framed in terms of a "bureaucratic mythology" that the individual sincere worker had absorbed and that saw these activities as externally goal-oriented when really they were not.

I am not here concerned with *what* unrealistic activities occur; collecting files often tends to be *seen* as more bureaucratic than, say counselling; but I am only concerned here with the actual, and not just supposed, relevance or irrelevance of the activities with regard to the organization's supposed external aims.

Which of the variables that we collected would we expect to be related to the granting of parole? First (and this is always the case in any statistical study of this kind) there are those whose relationship we somewhat contemptuously call artifacts. These are variables which for necessary logical reasons *must* be in a strong relationship with the granting or refusing of parole, rather than simply being in an empirical but logically non-necessary relationship. Thus, collecting data on such a relationship tells us nothing beyond what we could have deduced *a priori*. In the present study dichotomies 147 and 148 (see Table 94) are the only examples of this sort.

Also we find semi-artifactual relationships. When the Parole Board are seriously proposing to grant parole, they normally order a "community enquiry." There are exceptions, but the ordering of this is strongly indicative that parole will be granted, and its absence is generally indicative that parole will be refused. We may regard the holding or non-holding of a community enquiry, not as a completely separate variable related to the granting or withholding of parole, but as a probable indicator of which decision has provisionally already been made. (see Table 94, dichotomy No. 127)

After that come "artifact-dependant" relationships where the existence or absence of certain information indicates the presence or absence of some other artifactual or semi-artifactual indicator of parole. For example, it might be reasonable to speculate that perhaps parole would be more frequently granted to men whose families were expected to be "supportive", without necessarily knowing what this vague word was intended to mean. But on the other hand the answer "not known" to a question on this subject may well indicate, not a hard-to-estimate family atmosphere, but that no community enquiry has been made, since parole has already been decided to be unlikely. Thus even if a reply "yes" to the question "Is the family likely to be supportive?" turns out to be associated with a higher probability of parole, part or all of this association may be artifactual. It is sometimes difficult to judge to what extent a plausible relationship (or even a surprising one) between parole-granting and some other variable may or may not be "artifact-dependant" rather than indicating a relationship between the phenomena that we are trying to study. In the present study see especially Table 94, dichotomies 128 - 148.

When the artifactual, semi-artifactual and artifact-dependant relationships are eliminated, what other variables would we expect to be related to the granting or refusing of parole? Obviously our reply to this will depend on our views of the goals, outlooks and mechanisms of the Parole Board. Let us consider some extracts from official writings of Parole Board spokesmen, and use these as the starting-point in considering what will be expected of our data if the Parole Board is in fact in any way pursuing the sort of correctional, rehabilitative or social defence aims that they officially mention, or helping a larger system to pursue such aims. Then we shall consider what would be

expected of our data if we took the contrary view, that these alleged aims of the system were not in fact seriously pursued; that they were merely rationalizations for the operations of an agency possessing a somewhat paradoxical role within a larger system (the whole contra-crime operation) whose main activity was objectively irrelevant nastiness.\* On this view the paradoxical aspect of the Parole Board's role arises as follows. However much we may argue that the main activity of the main system is nastiness, one of the main activities of the Parole Board is setting some prisoners slightly free if they want to be, which is very far indeed from being nasty\*\*. From this point of view the relationship of parole to the main system is like that of brakes to a car; the purpose of the car is to travel fast and get us there quickly, and the brakes aid us in this by slowing down and stopping the car. We get home faster and safer in a car with good brakes; it is hard to say (at any rate this study was not designed to find out) whether the contra-crime system as a whole commits more or less nastiness as a result of the mitigating effects of some Parole Board activities.

Having considered what we should expect under two contrary sets of assumptions, we shall then consider what is actually the case.

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\*Throughout this work I make reference to the presence or absence of "objective goals", "objective relevance", etc., of contra-crime. It is fashionable nowadays to see the *raison d'être* for contra-crime in terms of the reduction or elimination of certain behaviours within a society. This is by no means its only conceivable justification; so-called "retributive" theories of punishment, (in practice almost any theory which twentieth-century western man finds hard to understand) are not always intellectually disreputable, though I find them repulsive. Equally, an existential decision, in certain contexts, to be a punisher or supporter of punishment, rather than a non-punisher or opponent, would be I suppose a valid choice, although one that nevertheless repels me. The fact remains that nowadays almost the only defence of their activities that punishers and their political masters, in contrast to philosophers, dare advance is the reduction or elimination of behaviours that someone has chosen to forbid. When these behaviours are actively harmful to the society, one could speak of an objective gain if their incidence in the whole society could be reduced without bringing about any greater loss.

This is the only sense in which I speak of the presence or absence of objective gains from contra-crime. Other effects of contra-crime such as providing employment for judges, police, correctional workers etc. will only be seen as a profit in a society which regards concealed unemployment as a good thing; in fact there should be more profit in releasing resources than in consuming them.

Other gains such as reduction of anxiety, and of expense and loss that often accompanies anxiety, is only to be regarded as objective if there are rational grounds for being anxious about some *specific* phenomenon, and for then being *less* anxious about the *same phenomenon*, directly on account of the activities of the contra-crime industry. Otherwise the gain is the gain that goes with misinformation or superstition; a gain always accompanied by a greater loss.

\*\*Though we must not forget that in twice as many cases (at that period) the Parole Board encouraged applications for this partial freedom and then refused them; and that frequently after granting such partial freedom it withdraws it again, imposing an even greater degree of imprisonment than before. Even within its own sphere of operation it may well be responsible for more nasty acts than for the contrary. At present (1972) its excursions into "mandatory supervision" and other provisions of the new amendments to the act (see Appendix E) represent a great increase in their powers to shut people up in the name of letting them out.

We begin our consideration of the Parole Board's own point of view with some extracts from the "Handbook on Parole" by T. George Street, Q.C., Chairman, National Parole Board, Ottawa. (December 1960).

#### FUNCTION OF THE PAROLE BOARD

3. The Parole Act provides that parole may be granted to an inmate, if *the Board considers that he has derived the full benefit from imprisonment and that the reform and rehabilitation of the inmate will be aided by the grant of Parole.*\* Therefore the function of the Parole Board is to select those inmates who show that they sincerely intend to reform, and to assist them in doing so by a grant of parole.

4. *If you are selected for parole it will be because we consider you intend to live a law-abiding life in the future and that parole will help you do so.* This does not mean that you should just complete your parole period without misbehaving and then return to crime.

#### POLICY OF THE PAROLE BOARD

7. The policy of the Board is, as far as possible to:\*\*

(b) consider the offender rather than the offence;

(d) judge each case objectively, according to its merits and circumstances;

(h) consider each case from the point of view that what the inmate is apt to do in the future is more significant than what he has done in the past.

#### PROCEDURE OF THE PAROLE BOARD

8. The question whether a parole will be granted or refused is necessarily a matter of judgment based on various reports received about each inmate. We ask the police what they know about the offence. The trial judge or magistrate tells us how he dealt with the case in court and his opinion of you and how you acted in court.

9. The reports from various people at the institution are especially important, because if you have changed, it has happened at the institution. We find out what you are doing to improve yourself, where it is possible in the institution to do so, such as whether you are interested in learning a trade or in taking educational courses, religious instruction, individual or group counselling, medical or psychiatric treatment and what your conduct, industry and attitude have been. We know what treatment and training facilities are available in the various institutions, and you cannot be blamed for not doing certain things if you did not get a chance to do them. An investigation is made

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\*National Parole Board's italics in all these quotations.

\*\*Some items omitted from this section.

in your home town to find out about your background, your work record and general reputation in the community.

10. From these reports we try to decide whether you have changed your attitude and seem likely to behave properly in future. Usually some reports about you are favourable and some not so, but all are considered together. An application is not refused or granted because of any particular report or recommendation. Each case is thoroughly investigated and is carefully considered by all members of the Board.

#### SELECTION FOR PAROLE

11. There are many considerations involved in deciding to grant parole, some of which are as follows:

- (i) What kind of offence you committed against society and how serious it was;
- (ii) Was your behaviour in the past good or bad;
- (iii) What kind of a person you are and whether you can live "outside" without committing offences;
- (iv) The likelihood that on release you may return to a life of crime and the probable injury society would suffer if you did;
- (v) What efforts have you made during imprisonment by way of improving your habits, education and skills, to show that you really intend to be a good citizen;
- (vi) Your family and marital circumstances and background and early development and whether anyone is willing and able to help you, when you leave prison;
- (vii) Your proposed parole plans and whether they are sound and will help your rehabilitation;
- (viii) What employment you have arranged, or may be able to arrange. It is important that you should be working and if you cannot get a job yourself, perhaps one can be arranged for you;
- (ix) Do you understand what got you into trouble and if you have a problem have you overcome it? This means learning to understand yourself better, your strengths and weaknesses, so you will be able to control your behaviour and stay out of trouble.

12. *The main test is whether or not you have changed your attitude and sincerely intend to behave properly in future. We want to help as many inmates as we possibly can, but you must be willing to help yourselves. If you do not want to help yourself, probably no one can help you.*

#### ALCOHOLICS

15. Many inmates are alcoholics and when intoxicated get into trouble. Others may not be alcoholics, but commit offences only when under the



influence of liquor. If you are one of these, we must be fairly sure you are willing and able to make a real effort to stop drinking entirely. If a man has a problem with drinking it is in his best interest if a condition of his parole is that he must abstain from intoxicants. Therefore, if you have a problem with alcohol we expect you to recognize this problem and do something positive about overcoming it. The Alcoholics Anonymous program is very helpful, but in order to benefit from this program you must be sincerely interested in helping yourself and be willing to give up alcohol. The Board is pleased with the number of former inmates who have continued A.A. association after release and in so doing have been helped in their civil life adjustment.

#### PRIOR CRIMINAL RECORD

16. A criminal record is not a bar to parole and hundreds of men with serious records have been paroled. However, if a man has established a pattern of criminal behaviour we obviously have to be more careful about granting a parole because we must be reasonably sure that his criminal behaviour will not be repeated. The same applies to a previous parole violator. These two types can be paroled, but they should do a little more than the average to show that they have changed.

17. When we are satisfied that you do not intend to return to crime, you will be paroled but for the reasons mentioned above, we must be more careful because of our duty to protect society.

#### DRUG ADDICTS

18. Drug offenders can also be granted parole but naturally, because there is such a serious problem involved in the use of drugs, we must be more careful than usual but if it seems that you sincerely intend and seem able to stay away from drugs in future we want to try and help you do so.

Let us take this as an official statement of the official view of parole.

Paragraph 7d, if it is seriously meant, implies that if parole has been refused to one man and granted to another, the Parole Board possessed some *specific, comparable* information on the two men in terms of which the second man was seen as more appropriate to be paroled than the first. On the further assumption that this information was recorded on the files we studied (see Sections 3 and 7, and Table 1) we have available some evidence as to how far the other items quoted from the "Handbook of Parole" are true in practice.

Before looking into this we must note that even within the limits of our quotation the handbook is sometimes self-contradictory, or rather, that although its wording is sufficiently vague to avoid literal self-contradiction, nevertheless any Parole Board trying to live up to this set of principles would

find itself expected to do certain incompatible things. Basically, an attempt to apply sections 7(b) and 7(h) is incompatible either with the spirit or with the consequences of applying sections 8, 11 (ii) and 16, and only compatible with the last sentence of section 9 in terms of quite ridiculous assumptions.

Thus we see that the importance to be attached by the National Parole Board to the nature of the offence for which an applicant was sentenced is impossible to estimate from their official statement.

Apart from this a clear enough view emerges.\* The Parole Board sees itself as dealing with men who in the past did a bad thing because they were bad. The role both of prison and of parole is seen as social defence, which is best achieved by turning the bad men into good ones so that they won't do more bad things. For some men this is impossible, and all we can do is to imprison them for a while and then release them to do more bad things. But some men admit their badness, achieve some measure of insight into its causes, and try to change; and these will be helped by being imprisoned until they have received maximum benefit from this, and then released into a heavily controlled and supervised environment subject to severe limitations on the normality of their life and under the constant threat of being returned to the prison from which they have already derived the maximum benefit. Such men can be identified by the Parole Board and given the partial liberty that they are seen to deserve. Any distinction between what a prisoner deserves, what is good for him, and what is good for society, is systematically ignored.

Now this strikes me as a load of pretentious and tyrannical idiocy, (see Studt, 1967, for a criticism of the consequences of similar assumptions made by Californians instead of Canadians) precisely the mixture of boy scout psychology and amateur religion that high-status people think is good for low-status captives;\*\* but at any rate it is a coherent set of reasons for our contra-crime activities and for including parole among these. Moreover it is a view that would lead the Parole Board to collect specific items of information on applicants and to base consistent decisions on these items. In fact we do not need to list these items, so clearly are they specified in our quotations from the "Handbook".

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\*The document "Canada's Parole System," a brochure for Judges, Magistrates, Police and Parole Supervisors, by T. George Street, Chairman, National Parole Board, Ottawa, perhaps conveys the atmosphere of the official view of Parole more clearly than the "Handbook" which was written for inmates.

\*\*One reader of this work has suggested that the tone of this comment makes it difficult for the reader "to accept that separation of objectivity from subjectivity in the treatment of (my) subject which (I) have set for (myself) when distinguishing findings from commentary." This seems to me to raise two points of importance.

First, the reader may ask to be assured that my findings were reached by suitably objective methods, in spite of my unconcealed emotions. I ask him to observe that

- (i) It was not I who collected the data
- (ii) the data were analyzed by computer, using standard techniques

On the other hand we can consider the second possibility discussed above, that the activities that people naively or cynically, but in any case inconsistently, call justice, corrections, etc., have no specifiable objective except nastiness, and that the role of parole in this nastiness system is ambiguous. In this case we should expect a different set of information to be collected and to be related to decisions about parole.

The information would in this case be more oriented towards the past (and especially that tiny fragment of the past occupied by "criminal activities" (see Turk, 1969)) than towards differences between the past (so defined) and the expectable future;\* it would contain more clearly recorded judgments by bureaucracies than socially relevant facts; the relative importance given to different bureaucracies would depend on their status and their closeness of contact with the National Parole Board rather than on the relevance of their information to the problems said by the Parole Board to be of concern to them. The judgments would be interpreted in terms of a good-bad scale (or rather a "bad-not quite so bad" scale) of greater or lesser stigmatization, rather than in terms of any specific concept of releasability. When consideration was paid to the future, it would be in terms of the bureaucracy's own already formed hunches about an individual's future (reinforcing pseudo-evidence) rather than specific data about specific future contingencies (independent real evidence).

Here then are two opposed viewpoints which would lead us to expect different results. What in fact we find is set out in Table 94. To make a fair comparison between different items of information, all the variables listed in Tables 3-92, with a few exceptions discussed below, are dichotomized: that

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\*The past may often be a good guide to the future, but the concept of the parolee as a bad man reformed by prison means that the realistic task for parole selection would be to identify men for whom the tiny fragment of the past called criminal *will not* be a good guide to the presence or absence of similar fragments in their future: a parole board that addressed itself realistically to this task would collect and use different information from one whose activities were solely bureaucratic.

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- (iii) I did not myself put the data on the computer
  - (iv) I did not write the programmes for the computer
  - (v) The people who did these things had no great knowledge, and even less concern, about the feelings that my critic asks to express more mildly.
  - (vi) I expressed these feelings *after* these analyses were done

The whole point of standardized scientific techniques is to prevent the researcher's emotions from influencing the results he obtains. Thus a scientific researcher can admit his emotions. He is not like a judge whose justice may depend on his being cool when really he is boiling.

But secondly, suppose that my findings *are* riddled with subjectivity. How will this be improved by toning down the sentence in question? This might lull the careless reader's suspicions, but it would not improve the objectivity of my findings. If these are vitiated by writing emotional comments, they are equally vitiated by deciding not to write them. All science would be undermined by what the scientist might have been feeling.

is, broken down into questions answered by yes or no. These dichotomies are listed and defined in Table 93. We note that for *every* member of the sample an answer either of "yes" or of "no" is possible for every question in Table 93. Thus when we wish to dichotomize Table 44 (Time that first offence of present batch occurred or began, according to police forms) we do not ask "according to police forms, was the time of the beginning of his first offence in the period 9 p.m.-3 a.m." since this would carry the *three* answers "Yes" (88 cases), "No" (74 cases) and "Not known" (244 cases). Instead we ask (Table 93 row 91) "Do the police forms state the time of the first offence to be in the period 9 p.m.-3 a.m." with the answer "Yes" (88 cases) and "No" (318 cases). We form other dichotomies from Table 44 also (see Table 93 rows 89, 90), but we never allow an ambiguous answer to arise. Of course framing dichotomies to avoid "neither-yes-nor-no" responses involves a large number of choices and decisions, such as what to do about missing information, and some of these decisions may seem arbitrary; but there are often guiding principles which we can follow and in any case, even the most arbitrary decision will be made by default later if not by intention sooner; all we have done here is to prefer arbitrary choices to arbitrary accidents.

Even apart from this the act of dichotomizing involves us in certain choices. Table 38, for example (most serious conviction in past career recorded on RCMP criminal record form) contains 9 possible responses (and of course the defining of these involved acts of choice). Now where there are  $n$  possible responses there are  $2^{n-1} - 1$  possible yes-no questions, so we could have extracted 255 dichotomies from Table 38. In fact we used five of these; which five was a subjective but not arbitrary choice.

Given our set of dichotomies, we can examine the association between the answer yes or no to any dichotomy and the answer yes or no to the question "Did this inmate receive parole?" The measure of association that we use is the well-known  $\chi^2$  with one degree of freedom (without Yates' so-called correction). Although one often has reservations about comparing the strength of two associations by comparing two values of  $\chi^2$ , here, where all the comparisons relate to 143 men granted parole and 263 refused, the comparisons are valid. Our data yielded 219 dichotomies which are listed in Table 93 and for whom the basic data of their association with the granting or refusing of parole are set out in Table 94.

Now in attempting to find to what extent one or the other of our two hypotheses is supported by these data, let us ignore dichotomies t1 to t5 (for reasons which will appear soon) and also the dichotomies whose low value of  $\chi^2$  in Table 94 indicates that they have very little association with granting or refusing parole. Such data will pile up on any general-purpose file, and although it is very interesting to consider what information the Parole Board does not collect, or having collected does not use in granting or refusing

parole, at the moment we are concerned with the data that *do* relate to this decision.

Let us for the time being, consider then only those 51 remaining dichotomies whose values of  $\chi^2$  in Table 94 are greater than 6.635 (the value of  $\chi^2$  which would be exceeded only once in a hundred samples if there were no association present in the underlying population). These 51 dichotomies are listed on Table 95, with their  $\chi^2$ 's.

Now in many cases we find that several of our dichotomies were obtained from the same underlying variable (the same Table in Tables 3-92) and represent very similar information; in these cases we select the dichotomy with the largest attached  $\chi^2$  and ignore the related dichotomies. We also ignore dichotomies 147-148 which represent information which must *necessarily* be in an artifactual relationship with the granting of parole. Semi-artifactual relationships have been allowed to remain, however, as these do not arise by logical necessity and thus they contain information about the decision-model of the Parole Board.

The 32 dichotomies that survive this process of elimination are then ranked in order of their  $\chi^2$ , counting 1 for the largest  $\chi^2$  and 32 for the smallest that is greater than 6.635. Now these 32 dichotomies are based on statements drawn from seven sources.\*

- |                            |                                                                                                               |
|----------------------------|---------------------------------------------------------------------------------------------------------------|
| A Admission forms          | (Penitentiary records departments)                                                                            |
| B Criminal record forms    | (R.C.M.P.)                                                                                                    |
| C Various police forms     | (Other police forces)                                                                                         |
| D Parole application forms | (Inmate)                                                                                                      |
| E Community enquiry etc.   | (Mainly Parole Service)                                                                                       |
| F Classification reports   | (Penitentiary records departments based on statements by classification officer)                              |
| G Custodial reports        | (Penitentiary classification officer collating his own and a number of other custodial officers' statements). |

Thus all the sources of dichotomies are represented in our "top 32" except the Parole Board itself and a miscellaneous group which we called "all sources" and which dealt with discipline reports, recommendations for psychiatric examination, etc., within the penitentiary; the source in fact of dichotomies 185-193.

Table 95 shows then the 32 dichotomies with their rank order, divided into groups representing their seven sources. Table 96 summarizes these results by giving the number of these dichotomies from each source, and the

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\*This is the reason for ignoring dichotomies t1 to t5; they do not come from any specific source in this sense of the term.

average of the rank-order in each group. The number of these dichotomies in each group is not very important as the amount of paper generated from any one source will depend on many causes not related to granting or refusing of parole. However, the average rank-order is an important measure; if this figure is much lower in one group than in another it indicates that of our 32 dichotomies those in the group with the lower average rank relate to the granting or refusing of parole more strongly on average than those in the group with the higher figure. (The higher the rank-order, the lower the  $\chi^2$ .) Looking at the data by means of average rankings of the more strongly associated dichotomies was chosen to eliminate two problems, the problem of the source that generates both important and unimportant dichotomies (since we are only considering a selection of 32 of the most "important") and the problem of different amounts of data from different sources, since we are considering the average ranking within each group.

So the first thing to do is to decide whether the apparently very large differences in these seven average rankings are larger than we should expect to find occurring just by chance; and if so, then to interpret these differences.

The techniques for testing whether the differences in average rank order is likely to have arisen by chance is Kruskal and Wallis' Non-Parametric One-Way Analysis of Variance (Kruskal and Wallis, 1952). There is a good account of this in Siegel (1956). Here it is sufficient to note that it does the same job as an ordinary analysis of variance as described in almost any introductory book on statistics, but is a more appropriate method when the figures to be analyzed are rankings and not measurements.

Accordingly Kruskal and Wallis' method was applied to Table 96. This method produces a statistic distributed like a  $\chi^2$  with (in this case) 6 degrees of freedom; we obtained a value of 17.06, which reaches the 0.1% significance level.

So we conclude that the differences between the average rank orders in the seven groups did not occur by chance. In other words, among our 32 dichotomies there is a systematic tendency for some sources to provide dichotomies more strongly related to the granting or refusing of parole than do other sources. The sources, in descending order of "importance" in this sense, are:

E	Parole Service, etc.	Mean Rank	4.00
G	Custodial reports	Mean Rank	15.25
B	R.C.M.P.	Mean Rank	16.85
F	Classification reports	Mean Rank	17.00
A	Admission Forms	Mean Rank	22.00
D	Application by inmate	Mean Rank	28.50
C	Various police forces	Mean Rank	29.50

With the apparent exception of G (Custodial Reports), to which we shall return, we can hardly imagine a better fit at first sight (we shall consider objections later) to the hypothesis that the Parole Board operates bureaucratically and without reference to the external realities. Their decision to grant or refuse parole is related firstly to statements made by their own employees; secondly, to the group G, which needs separate explanation; thirdly to records of past decisions of judges; fourthly to the most professionalized papers from the penitentiary; fifthly to lower-status papers from the penitentiary; sixthly and seventhly to the people of sufficient lowliness and remoteness from the Parole Board actually to have some contact with the events that are supposed to be the reasons for convicting and imprisoning the applicant in the first place.\*

In this steady descent from the Parole Service, its high-status connections and their servants, down to those "outsiders" from the "correctional" point of view who actually participated in the relevant events, group G appears misplaced. At this point our thinking is forced to become more speculative and cannot be so firmly based on inescapable facts. However, it seems to us that the explanation that we offer, while not strictly provable, fits the observed facts and is inherently reasonable. Our line of thinking would have led us to expect these "custodial reports" to fall probably between the classification reports and the admission forms. If we except the "best" dichotomy, No. 199 ("Was custodial recommendation for parole favourable?" which has  $\chi^2 = 100.98$ , rank order 3) from this group we find that the mean rank order of the remainder is 19.33, exactly where we should expect it. Now is there any justification for pulling out this one dichotomy? At the moment, doing this has a strong appearance of "cooking the results" and we should need several guarantees before we should consider that taking dichotomy 199 out of group G was appropriate and justified and explained anything rather than "explaining away" a result that we didn't like.

Firstly, does this item fit its group notably worse than other items appear to fit theirs? The difference in rank order between dichotomy 199 (rank order 3) and its companion in group G which has the nearest rank (dichotomy 197, rank order 15) is 12. The nearest approach to this is the difference of 8 in group A between dichotomy 15 (rank 18) and dichotomy 16 (rank 26). If we removed dichotomy 15 from Group A the "average" of the one remaining rank order in this group instead of the present 22.00 would be 26.00 which would leave group A in position between groups G and D and so would not involve any change in our interpretation. Neither would removing dichotomy 16. Nor are there any extreme outlying dichotomies in the other groups. Thus

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\*The idea may occur to some readers that the less used information might in fact be summarized by the more used information, which is to some extent more recent, and this might justify ignoring the less used information. We shall see later in this section and in Section 13 that in fact this was not the case.

a completely impartial researcher working blind, that is to say, not knowing what groups A to G represented, and knowing nothing of our interpretation of the Parole Board's activity as bureaucratic, might very well remove dichotomy 199 from group G on purely statistical grounds; and the only other possible change, and that a very doubtful one, would be to remove dichotomy 15 or 16 from group A; and if he removed one or both dichotomies (or even a few from other groups as well) the only change he would bring about that affected the interpretation would be the reassignment of group G, the custodial reports, to the position we should have expected.

So we have made a part of the case for treating dichotomy 199 as separate from group G. But we shall not be satisfied to do this unless this dichotomy is in some clear sense different from the others, unless in fact it *requires* removal instead of merely permitting it. In fact it does. It is "Was the custodial recommendation for parole favourable?" Now this custodial recommendation is the first suggestion that parole should or should not be granted to be systematically made on every applicant. (There is also provision for considering police recommendations but these are not usually made; see Table 54). Now in any shared, visibly recorded yes-or-no decision-making process the first official recommendation carries enormous weight. For an extreme case of this in a very serious area see Scheff (1966). Thus we are not surprised to find that this one item is far more strongly associated with granting or refusing parole than other items drawn from custodial reports.

So if we accept that dichotomy 199 may be regarded separately from the rest of group G, we find the following sequence of average strength of association with granting or refusing of parole:

The Parole Service itself, then the first recommendation received, then past penalties imposed by judges, then documents from the penitentiary in three groups in order of degree of professionalization, then lastly those people who have had some contact with the social phenomenon which triggered off the existing situation.

Of course, this is not to say that this order of importance was consciously held by the members of the Parole Board, nor even that any particular item that associates strongly with granting or refusing parole was consciously considered by the Board. It does however show whose opinions relate in practice (whether deliberately considered by the Parole Board or not) to the granting or refusing of parole.

A number of objections can be offered to our interpretation of this decision process as bureaucratic and unrealistic. The results of the data-analysis are inescapable, but it can perhaps be argued that items are treated as important, not in terms of who supplies them but in terms of what they say, and that if the relationship between the Parole Board's decision and the sources of various items leads to the sort of interpretation we have



advanced, this indicates who address themselves to the most relevant questions and it does not necessarily indicate a bureaucratic interpretation of the decision process.

This claim is susceptible to analysis. We divide our 32 dichotomies once more into groups, this time not according to source but according to the subject-matter treated. However, whereas in the earlier analysis our seven sources were clearly defined and each dichotomy came indisputably from one of these sources, the same cannot be said of our categories of subject-matter. We obtained our groupings as follows. All items were taken at face value; for example, we did not consider the possibility, of which up till now we have been very much aware, that an official's opinion of an inmate told us as much about the official or the official-inmate relationship as about the inmate. (After this part of this section we shall return to our usual point of view on this, as set out in Section 7.) We divided the items into those which referred to the time before the offences for which the prisoner was presently under sentence, those which fell in the period from those offences up to the present, and those which referred to the future. In some cases the decision was not obvious or indisputable. Then we categorized each item by asking the question "Whose behavior has probably been different in the case of a yes and a no response to this item?" Thus dichotomies dealing with past sentences received were put in the category "Past: judge" while the question "Is the inmate's family expected to be supportive" was classified as "Future: people (not officials) in contact with the inmate."\* The people fell into six categories: judges, police, correctional staff, parole service, the inmate, and people (not officials) in contact with the inmate. Table 97 shows the dichotomies in the categories so formed. There are eighteen possible categories, but seven contained none of our 32 dichotomies and six contained only one dichotomy each. These six were combined into two as shown in the brackets in Table 97, leaving 7 categories in all.

Now it is obvious that other categorizations could have been formed, and that in the extreme one could say that each dichotomy referred to a different topic. But in the present context this last claim would be illegitimate. At present we are investigating a finding that dichotomies from some sources are more closely related to granting or refusing parole than those from other sources; and to seek to *explain* this finding in terms of the greater relevance of some topics than of others implies that the topics involved can themselves in some way be ordered or categorized in groups of different degrees of relevance. This relevance must be definable *independently* of the observed strength of association of each item with the granting or refusing of parole that is the starting point of these analyses. Otherwise the statement that some

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\*This is not to say that terms like "Past: judge" etc. *describe* the topic of the dichotomy in any detail, but merely that this is a convenient way to *classify* these topics in a general way.

topics are more relevant than others contains no further information beyond what we are seeking to explain, and our "explanation" becomes circular.

However, the particular categorization that we have chosen is very much open to challenge. We can say in its defence that it is clear and definable, and the groupings it yields appear to fit in reasonably well with the groupings that might be suggested, though under different names, by common sense, although we are limited by the need to keep the total number of categories small. Also, it appears neither to be too closely a repetition of the categorization shown in Table 96, nor on the other hand to go to artificial lengths to avoid resemblance between the two tables. But all this is bound to be subjective, and we have provided the necessary data for any reader to make and analyze other categorization if he wishes, though further support for the appropriateness of our set of topics appears in Section 13.

The analysis of Table 97 by the method of Kruskal and Wallis yields a  $\chi^2$  of 9.80 with 6 degrees of freedom, a value low enough to occur purely by chance in the absence of any underlying relationship.

To summarize so far, we have found strong evidence for a clearly interpretable relationship between the source of a dichotomy and the strength of its association with the granting or refusing of parole. We have found some evidence, but not quite enough to amount to proof, of a weaker (and as we shall see later, not easily interpreted) relationship between the topic to which a dichotomy refers and its "importance" for the parole-granting-or-refusing decision. But of course these two relationships are interconnected; items on the same topic tend in general to come from the same source. So it becomes important to see whether the effect of either of the factors that we have been considering, is "explained away" by the other. If we accept the relationship between source and importance, should we at the same time believe in a *separate* relationship between topic and importance, (and *vice versa*) or are these two relationships merely different ways of expressing the same underlying facts?

Accordingly we carried out a two-way analysis of variance on the data. The fact that in our 32 dichotomies some sources had supplied no, or very little, data on certain topics meant that the analysis was what is termed non-orthogonal; that is to say, the amount of the variance that we can regard as "explained" by one factor depends on the amount that is regarded as explained by the other factor. Thus we carry out the analysis twice. Firstly, we explain as much as we can of the variance between the ranks of our dichotomies in terms of their source; then we see how much of the *remaining* variance can be explained by topic. Secondly, we go the other way round, seeing how much of the *total* variance can be explained by topic and then how much of the remaining variance can be attributed to the difference between the sources. (In an orthogonal analysis of variance, as described in most

elementary statistical text-books, these two analyses become identical.) The two analyses are set out in Tables 98 and 99.

From these two tables we see that there is no adequate evidence that any effective attention is paid at all to the topic to which an item refers (of course this does not prove that such attention is not paid at all) though we have good evidence that its source is treated as important.

If we find this conclusion somewhat extreme, we can assume for a minute that the differences in mean rank for different topics did not arise by chance. Then we shall want to know which topics (on this assumption) are the most closely related to the granting or refusing of parole. Table 100 lists the seven topic categories in order of average ranking. Apart from the same concern with their own activities and their employees (including their own employees' estimates of the applicants probable future) that we saw in the analysis of sources of dichotomies, there is little in this list to suggest that it is arranged in any discernible order of relevance.

To summarize so far: if we take the 32 dichotomies based on reasonably independent items of data, whose association with the granting or refusing of parole is beyond the level that could arise even by a one per cent chance, and group them by source of origin, we find differences too great to be explained by chance in the average strength of this association in the different groups; and if we list the groups in order of average strength of association we find a "steady descent from the Parole Board's own employees, its high status connections and their servants, down to those "outsiders" from the "correctional" point of view who actually participated in the relevant events". We also find a very heavy reliance on the first official yes-or-no recommendation to be systematically made to the Board. We have sought but failed to find any evidence that these results can be explained in terms of the greater relevance of some items, and we have very little evidence that any topic is treated as more relevant than any other; but even if we *assume* that they are so treated, very little pattern of relevance can be traced in this.

However, two further objections can be advanced to our interpretation of the decision to grant or refuse parole as strongly bureaucratic. The basic measure used in the section was a  $\chi^2$  with 1 degree of freedom, and each of these  $\chi^2$  refers to the relationship between one single dichotomy and the decision to grant or refuse parole, taken over our whole study sample. It may be suggested that a less bureaucratic picture might be found if we analyzed our dichotomies in groups instead of singly, and for sub-groups of our sample instead of for everybody lumped together. The appropriate place to deal with this point will be in Section 11, but we will anticipate that section just sufficiently to say that this attempt to make the parole-granting decision appear to be realistically based will vanish on close inspection.

The second objection is an appeal to results. All the analyses of the present section, however interesting, would be in one important sense overridden if it could be shown that in some sense the "right" decisions were made, by whatever process, and that the "parolable" applicants were granted parole and the "unparolable" refused. This will be considered in Sections 12 and onwards, specifically in Section 13, but again we will anticipate to say again the same thing: we see no way of regarding the decision to parole or not to parole as being realistically based on benefit to society. We see much evidence that, whatever values are held by individual members of the Parole Board, or proclaimed on behalf of the Board as a whole, at the period under study the decision to continue a man's imprisonment or to grant him a limited freedom was determined almost entirely by bureaucratic considerations.

## Section 10

### The nature of prediction

So far, at each new stage in the research, we have usually begun by describing the problem before us in very general everyday terms before proceeding to a more specialized account of how a researcher might tackle this particular situation. Now we have arrived at the part of our study where we wish to discuss the aims and methods of "prediction". This time it will be simpler to begin with some account of the logic and activities that a statistician embarks on when he "predicts"; then, with this explained, we can consider how far this logic and these activities are precisely or approximately relevant to the situation that we are studying.

Let us begin then by supposing in the most general terms that we have four sets of people A, B, C, D. Later on we shall drop C and D from our discussion, but they are very important to an understanding of the logic of prediction. Set A is known to possess a certain attribute, set B to lack it. This may be any attribute we like, such as having fair hair, or being married, or receiving parole, or losing it, or even such conditionals or intangibles as wanting a toy train for Christmas if he can't have a bicycle.\*

Suppose that set C also possess this attribute, but that we don't yet know this; and similarly that set D lack this attribute but that we don't yet know this

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\*To keep this account reasonably simple we only consider the case where the subjects of the enquiry are people, and the attribute whose possession or lack is to be predicted can be regarded as the answer: "Yes" to a question admitting only yes or no answers. (In our example above, "Have you got fair hair?" or "Are you married?", etc.). Of course, the mathematical techniques remain exactly the same whether the subjects are people, animals, things, groups, ideas or in fact anything identifiable and partly describable; and these techniques can be generalized to questions requiring answers more complicated than yes or no; e.g. "How tall is this tree?" or "In what category does this idea fall?" or many others.

either. Thus at present we cannot distinguish sets C and D and will refer to them together as set C + D.

To introduce some further useful fictions, let us suppose that set C + D is a random sample from an underlying infinitely large population P, and that A and B were obtained by drawing a further random sample A + B also from P and then sorting it into its constituent parts A and B.

Let us call this attribute, whose possession we wish to predict, "the criterion". Let us now suppose that on each individual member of A, B, C, D we have a set of further observations; we shall call these "predictor variables". These variables will be the same for everybody, though of course their values will differ. (If we measure height we will measure everybody's height but not everybody will be the same height. "Height" is the name of a variable; "73 inches" is a *value* of the variable.) Now we may by means of various mathematical techniques\* be able to identify useful differences between the value of certain predictor variables in set A and set B, or perhaps differences in patterns of values, if our techniques are sophisticated enough. Then we hope for two things:

- (i) That these differences are characteristic of those members of P who possess or lack this criterion; i.e. they are not merely chance sampling characteristics of samples A and B. (This will partly depend on the mathematical techniques used to discover these differences); and
- (ii) that if supposition (i) is true, C and D are also distinguished by these differences, i.e. that these differences have not faded away by chance in the samples C and D.

The logician may wonder why we do not collapse (i) and (ii) and merely hope that C and D may be distinguished by the same differences that distinguish A and B. In fact the usefulness of introducing P into the argument will appear later.

Now the act of prediction may take place. We may take any member of C + D and examine the values of the predictor variables to determine whether he possesses values or patterns of values more typical of A or of B. We can then predict that he possesses the criterion (belongs to C) if his values or patterns of values of the predictor variables more closely resemble those of A, or we predict that he lacks the criterion (belongs to D) if his values or patterns of values of the predictor variables more closely resemble those of B.

Whether a member of C + D is said to resemble A more or less closely than B depends not only on the predictor values involved but also on our definitions and procedures in seeking resemblances and differences. For

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\*Or by hunch, hypothesis, crystal-gazing or clinical prognosis; but irritatingly enough maths. seems to work better. (See Meehl, (1954).) Besides, you can throw mathematical tasks to a computer; but humans have (so far) to do their own crystal gazing.

example, we may define two sets of values as "alike" for our purposes if they have certain characteristics in common, no matter what other differences. Or we might calculate a "score" by applying some formula to the values, and regard any two individuals who have the same score as being "alike" in this sense. Whatever set of definitions and procedures we choose is called our "model" of the relationship between the criterion and the predictors. We shall discuss this more fully later, but first we must make some further general remarks about prediction.

We cannot usually say of a member of C + D that his predictor variables enable us to assign him certainly to C (or D); more usually we find that his predictor values sufficiently resemble (however we define sufficiently resemble) those of say 50 individuals of whom perhaps 10 are members of A and 40 members of B; so we estimate his *probability* of belonging to C as  $10/50 = .20$ , or 20 per cent.\* Or we may have made certain mathematical assumptions about some regularity in the relationship between the predictors and the criterion (such as linearity) which enable us to estimate this probability even though we have defined our patterns of predictors in such a precise way that almost no member of A or B is said to resemble our candidate very closely.\*\*

Now we are considering prediction as a two-stage process; firstly we assign members of A and B to sub-sets of individuals who resemble each other according to our chosen definition of resemblance. Depending on our model the sets will be real or hypothetical. Then by a parallel method we assign members of C + D to corresponding, but this time hypothetical, sets. Now strictly speaking any individual can be assigned to any set, and with very few exceptions a logical definition can be formed to justify his inclusion; in this sense it follows that any individual has logically any probability between 0 and 1 of possessing any attribute. But this has the air of being a logician's paradox rather than a useful finding of probability theory. The reader does not seriously expect to find a \$100 bill between the next two pages of this book simply because he has "ascribed" a probability of one to that event.

Corresponding to the reader's common-sense realization that there will not be a \$100 bill between the next two pages of the book, however, logically defensible may be the assignment of a high probability to this event, is the

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\*But see Good, (1965) for the argument that a sample of 10 A's and 40 B's leads to an estimate *other* than .20 of the probability of belonging to C.

\*\*For example, suppose that we calculate a score for each individual and regard two individuals as similar if they possess similar scores. Suppose further that an individual (or a hypothetical individual) has a score of say 10. We might in some cases (depending on how we had obtained our score) find it reasonable to suppose that his probability of possessing a certain criterion was greater than that of people with scores of less than 10, and less than that of people with scores greater than 10. By using a number of such assumptions we might arrive at a reasonably precise estimate of this probability even though very few individuals (or even in the case of the hypothetical individual, perhaps no-one) had a score of exactly 10.

statistician's realization that in a given case, the model that he sets up for calculating probabilities must also follow common-sense, and that it does not alter the case if a "sillier" model is equally logical. Unfortunately common-sense only gets us half way. The reader knows all about the regrettable lack of \$100 bills between the pages of this book; but common-sense will not tell him how to estimate the probability that the next page contains exactly one misprint. He does not know, I imagine, how to predict variation in the type-setter's or proof-reader's vigilance. Thus he has to guess which of several possible models best reflects reality.\*†

But if in the face of an apparently simple phenomenon like misprints we find ourselves uncertain what model to choose, how much more uncertain we shall be in predicting phenomena such as the granting or loss of parole, which we may suppose to be the outcomes of complex sets of causes not necessarily reflected in our variables. (We shall see that a variable does not have to be a cause to be a predictor.) In general we use Occam's razor, and keep our models as simple as reasonably possible, and only increase their complexity when we have good reasons to suppose that it is worth while to do so, but we may still find that we have a choice between equally simple models, and in any case statisticians do not yet know how to measure exactly the simplicity or complexity of a model.

In the present study we have used two predictive techniques, each based on its own particular model. The first of these techniques is multiple linear regression, and the second is predictive attribute analysis (see Macnaughton-Smith, (1965)). Both of these will be described below, but for the time being we must return to the adventures of our four sets A, B, C, and D.

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\*If vigilance remains constant so that errors are independent, the number per page will presumably follow a Poisson distribution; but if vigilance is variable and the presence of an error is associated with a period of lower vigilance, errors may come in bunches and the distribution of the number of errors per page will have a higher variance than that of a Poisson distribution. On the other hand if making an error fulfilled some need, each error might be followed by an error-free period, and this negative association between errors would result in the distribution of the number of errors per page having a lower variance. Which assumption we make (or even our refusal to make any assumption) will influence our estimate of the probability that the next page contains exactly one error.

†The whole problem is an example of the well known problem of the "Justification of induction" (see e.g. Strawson, (1952) p. 248 et seq.). We assume that the present and future will show certain resemblances to the past (e.g. that the "laws of science" will continue) and that sound judgment based on experience is possible. There is no logical need for this to be always so, and it is not necessarily illogical to expect anything to happen at any moment; but it may often be "silly". (So of course can over-confidence that the world will go on "just as it always has", i.e. just as we think it always has.) But we save the continuity of the laws of science by the following trick: if some law which we have formulated appears to have held in the past but to have ceased to hold any longer, we *assume* that it was wrong in the past as well as the present, and we try to formulate a new more inclusive version of the law which fits both the old and the new data, so that we can once again say "this law has worked in the past and works in the present and I judge that it will work in the future; the laws of science and nature do not change." In fact what does not change is our determination to seek patterns that cover the whole of the known past and present.



Firstly, we must note that we have supposed that we do not know whether an individual really belongs to C or D, but only the probability with which we have estimated that he belongs to either set. Yet to measure the success or power of a prediction we must compare our estimated probabilities with the reality. Thus at some stage or other this hidden information must be revealed. Often of course it was always available, but simply treated as hidden for the purposes of the study. This depends on the reasons for undertaking a prediction study; see Section 11.

We have defined the "act of prediction" as being applied to members of C + D, yet in fact the majority of so-called prediction studies published are in a sense incomplete; they stop when the analysis of sets A and B have been carried out, leaving the selecting and treatment of sets C and D to be done later, if ever. Some authors refer to the tables that often summarize the results of such studies, not as prediction tables but as experience tables. This is not solely a question of the reasons for undertaking the study, nor is it merely a question of terminology. In some cases statistical theory is capable of estimating the accuracy and precision of a set of estimates without recourse to a second set of data. In this case replication may be reassuring but is not logically necessary. But most prediction techniques likely to be useful in practice possess the quality that they lead in the first instance to biased and unreliable predictions which need correcting on a second set of data, from which their accuracy and precision can also be estimated. That is to say, with these techniques our group A + B should itself be broken into four groups, (let us call them A<sub>1</sub>, B<sub>1</sub>, A<sub>2</sub>, B<sub>2</sub>) regardless of whether groups C and D are being considered in the same study, or later, or never. Again, replication will not in this case be necessary once A<sub>2</sub> and B<sub>2</sub> have played their part.

Now all this is very confusing to the reader of such studies. In fact many of the researchers seem to be far from understanding what they have done. The point to grasp is that all research is in some sense incomplete, and that a "project" or "study", no matter how imposing, always leaves undone more than it achieves. The point at which a researcher breaks off a study is determined by many things, but it is always in some sense a mid-point and never an end. "It is not a matter of approaching a fixed limit: absolute knowledge or the happiness of man or the perfection of beauty; all human effort would then be doomed to failure, for with each step forward the horizon recedes a step; . . . Science . . . finds its truth if it considers itself as a free engagement of thought in the data, aiming, at each discovery, not at a fusion with the object, but at the possibility of new discoveries; what the mind then projects is the concrete accomplishment of its freedom" (de Beauvoir, (1948) )\*. So since no science, and thus no prediction study, can achieve

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\*I have changed two words from Bernard Frechtman's translation, in which the words "data" and "object" are not used, but in their place "given" and "thing" respectively.

completeness, it is reasonable to have studies with two, four or six groups, though I know of none published which uses all six. Those using two only may be perfectly proper studies of sets A and B, but unfortunately they are often invalid exercises in analysing sets  $A_1$  and  $B_1$  without even considering any sets  $A_2$  and  $B_2$ , let alone C and D. Such studies are of course perfectly valid if it is made clear what is and is not being done; the invalidity arises from consciously interpreting our analyses of sets  $A_1$  and  $B_1$  as though our results came from sets  $A_2$  and  $B_2$ .<sup>\*</sup> Similarly, a study using four sets may use A, B, C, and D or  $A_1$ ,  $B_1$ ,  $A_2$  and  $B_2$ , or, unfortunately, four hybrid groups where it is not clear what has occurred. Again, any of these studies will be valid or invalid according to the interpretation of what has and has not been done.

The present study uses sets  $A_1$ ,  $B_1$ ,  $A_2$  and  $B_2$ . Since further replication is not logically necessary and the predictions are not designed for further applications with the Parole Service, we shall not be concerned with sets C and D.

This will be a convenient point to make an observation that will have very important implications in Sections 11 and 14. In the set of predictor variables there may be many which are in a strong relationship with the criterion. Yet in the pattern or score finally set up to predict the criterion, very few predictor variables may be involved. The point is that the predictor variables are not only related to the criterion but also to each other; so that the predictive value of two or more strong predictors may be very little different from the value of one of them alone.

- Example.* (a) His health is bad because he's overweight.  
(b) His health is bad because he's too fat.  
(c) His health is bad because he's overweight and he's too fat.

a and b may express strong relationships, yet c expresses a relationship so little stronger, if at all, that it has a faintly comic sound, more like an insult than a serious remark. Thus the smallness of the set of variables in a prediction equation says nothing either way about the relationship of the other predictor variables with the criterion.

Now that we have described the logical structure of the activities which constitute prediction, we ask what real life situations sufficiently correspond to this to justify undertaking predictive studies, or at least to tempt us to do so.

The commonest example of statistical prediction is of course insurance. (Prediction here refers only to the process of attaching probabilities to

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<sup>\*</sup>M. Walker (1969) claims that this is justified (in claiming that valid significance tests and confidence limits can be obtained) when using techniques for which likelihood statistics can easily be calculated (they can in fact be calculated if desired in conjunction with almost any predictive technique). She advances no reason for making this strange and fallacious claim, and presents her own unreplicated study as though it fulfilled all the requirements of a replicated one.

*individual* outcomes; the forecasting of national economic trends and similar block measurements is sometimes called prediction, but is not what we are discussing here.) The price of an insurance policy to an applicant is partly a function of his predicted probability of making a claim. In this case, policies held in past years on which claims have been made are treated as set A; those on which claims have not been made are treated as set B, and currently valid policies are treated as set C + D\*. The reader may consider to what extent this exactly meets or approximates to the logical structure described earlier in this section.

Because insurance is so widespread all statistical prediction is sometimes referred to as actuarial. However, other applications exist which raise important questions. Research goes on into the use of prediction techniques in medical and psychiatric diagnosis and prognosis (e.g. Armitage, McPherson and Copas, (1969)).

In diagnosis, A is a group who have been found to have a certain disease and B a group found not to have it; C + D is a group to be diagnosed. In prognosis A, B and C + D are all agreed to have the disease, but a certain outcome (recovery, death or some other event of interest) has occurred to A and not to B. Already in these examples certain points of logical interest arise, but we will pass them over to consider one frequent extension of the prognostic model.

The prediction of the outcome of their illness for C + D depends on the assumption that A + B and C + D are (or may be regarded as approximating to) two random samples from the same underlying population; and this implies that the frequency of any treatment applied is, apart from sampling variations, the same for C + D as for A + B; and the same is true for any other attribute apart from treatment. But suppose now that for C + D (or some part of it) we artificially produce some point of difference from A + B. Suppose for example that we apply some treatment of their illness to C + D that was not available for A and B. Then if the frequency of the outcome (recovery) is significantly different from that predicted, we may infer that this difference was due to the treatment. The logic is as follows. If C + D were left to the same probabilities of receiving the same treatments as A + B, then the logical structure discussed earlier in the section would apply and we could predict the probability of recovery of each member of C + D. But we have raised the probabilities of receiving various treatments, and we assume that we have varied nothing else. We may then ascribe any variation in recovery rates that is too large to be due to chance to the variation in the treatments.

Now criminologists, especially those who interest themselves in so-called corrections, sometimes apply this last model to contra-criminal

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\*One policy can fall in several sets since it is a policy-year rather than a policy as such that is treated as an "individual".

activities. There is a double impertinence here, since they imply that the police, judges, prosecutors, jailers, and other professional inflictors of compulsory annoyance deserve some comparison with those who try to use relevant knowledge and skill to restore health and relieve suffering; and they assume that your decision that you have a cold is logically parallel with somebody else's decision to call you a criminal. However, it might be worth making these assumptions once or twice on a "let's pretend" basis, to evaluate and thus explode the correctional-treatment myth; but one can only wonder at the willingness of some criminologists to devote their careers to dignifying, by seriously evaluating instead of ridiculing, every new-fashioned quirk or old-fashioned nastiness devised or re-discovered by our professional annoyers.

When this model is applied in this way, the technical name coined for the predicted probability that someone will reconvict a member of C + D who does not receive the new "treatment" is "base expectancy". I assume that the pun is unintentional.

Other uses of prediction in criminology vary mainly in the use to which the results are to be applied rather than in any point of logical interest. One might suppose that an expensive and apparently harmful public activity like contra-crime would at least keep some account of the outcome of its activities, some rudimentary acceptance of quality control if not of responsibility for the results of its operations; but when such things are mentioned at all it is in terms of "a need for research", though an industry that diverted its researchers on to what should be routine quality control would soon lose its market or be in ruins. But the contra-crime industry does not fear loss of market since it protects its monopoly by force.

Some researchers propose prediction models as aids to decision-making. For once one can praise the contra-crime industry for its lukewarmness, except when overpersuaded by the salesmen of research. A prediction model evaluates the probable outcome of a state of affairs or a single action; a decision model compares the different probable outcomes of all available courses of action. Prediction models are not decision models though there may be some overlap. If the direct, objective outcomes of the actions of the contra-crime industry mattered at all to that industry they would have developed the use of decision models years ago.\*

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\*One reader suggests that this last comment is a little too harsh, since many other comparable industries have made almost no use of formal decision-models.

## Section 11

### Prediction in criminology. Predicting the granting or refusing of parole

Since, as we said in Section 10, the probability to be ascribed to an event depends not only on the data available to predict it but also on the model fitted to these data; and since almost any probability for any event can be derived from some logical (but possibly far-fetched) model, so that the criterion for a good model is that it shall be sensible as well as logical; then in choosing our model or models for the predictions in the present study it will be worth considering a little further the mechanisms underlying the events we wish to predict, and the models that have been fitted in other similar studies in the past. Later in this section we shall only be concerned with the granting of parole, but in Section 15 we shall be predicting the loss of the partial freedom that parole confers, a subject in the past much more studied, perhaps because of the negative views on man and society implicit in much main-stream criminology. We shall also be relating these two predictions. Thus it will be convenient in this first part of the present section to consider both the processes (granting and losing parole) that we hope to predict.

The earliest prediction studies in criminology\* were closely linked to the question, once thought to be fundamental but now looking somewhat of a distracting option, "what makes them do it?" It was hoped that by quantifying the predictors of "doing it" (delinquency) or "doing it again" (recidivism) certain administrative decisions about which children or criminals to annoy and how much to annoy them would be facilitated (this was

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\*In this section we do not give references to examples of very general statements. The reader interested in pursuing the matter further should start with Simon (1971), where he will find sufficient guides to further reading.

seen as a doubtless worth-while goal)\* and also that concise summaries of differences observed between those who "do it" (or "do it again") and those who don't would lead towards solving the supposedly central question,\*\* (though as we have pointed out a variable can be a predictor without being a cause. A high consumption of milk and sugar may be a predictor of a liking for cornflakes.) The increased emphasis we are placing in the present work on conviction and punishment as things known to be done to "criminals" rather than as things supposed to be caused by criminals makes much less attractive the idea of predicting these events solely from data about the criminal, ignoring data on all the other people who contribute to convicting and punishing him, and the situations, relationships and contexts also involved. Such a prediction remains a logical possibility, just as, to take an even more extreme case, we might reasonably predict what telephone numbers were liable to be dialled in error from characteristics of the numbers and not of the people who were making errors, but our approach makes it seem less plausible than it has seemed in the past that conviction etc. will ever be either accurately or meaningfully predicted as a function solely of data about the convicted man.

When people were more optimistic about predicting delinquency, crime, recidivism or what-have-you, two alternative sets of assumptions were made, or at any rate implied in their work. The first was that since what was being predicted was, they thought, some sort of badness or lack, not necessarily entirely moral, but usually in some way stigmatic, the problem was to identify and measure the relevant badness or lack. That is to say, the probability of possessing the bad criterion would be related to a score formed by adding points for each quantum of each relevant bad attribute possessed or necessary good attribute lacked. The "relevant bad attributes" could be chosen either *a priori* in terms of what looked bad in the light of certain experience, assumptions or theories, or they could be derived by analysis of the data itself. The mathematical techniques involved will be discussed later.

The alternative assumptions were that there were not necessarily in the data any attributes whose possession or lack could be regarded as uniformly

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\*But see Section 10 for the difference between prediction models and decision models.

\*\*A praiseworthy exception to this state of mind was shown by Warner (1923) in what is sometimes called the first prediction study in criminology. Interestingly, he studied, as we do, the relationship between the granting of parole and subsequent re-criminalization. Although with the benefit of nearly fifty years' hindsight we may smile at some of his recommendations it is clear that he anticipates much of what we shall find about the objective irrelevance of the parole selection process, though Hart (1923) disputed this point. Hart also pointed out that Warner had not made what we should nowadays call a prediction study.

Between Warner's study and ours, no-one, as far as I know, has made the same sort of detailed study of the relationship between decisions of contra-crime agencies, their imputed goals and their actual achievement. The myth of the criminal as the man who is different and on whom we must therefore focus attention has been very successful in insulating the contra-crime industry from any serious danger of evaluation.

bad in all cases; "badness" was associated with certain patterns of attributes which went badly together, rather than with individual attributes or additively formed scores. The problem of identifying such patterns could also be approached from an *a priori* position or by pattern-seeking techniques of data analysis.

Many people confronted with these two approaches intuitively prefer the second, as being more realistic. This may be so; on the other hand it may merely sound more realistic because it is vaguer. The phrase "pattern of attributes" can mean almost anything until we make it more specific; when we do this it depends on *what* we specify whether our model remains more realistic than a simple score, or less so; after all, a score itself is merely one specific form of pattern. Some pattern-seeking techniques have been enthusiastically praised by Grygier (1966) and condemned by M. Walker (1969). In practice, in the problems in crime and contra-crime so far analysed both by pattern-seeking and score-building methods there has not yet appeared any conclusive advantage one way or the other. (See Section 14.)

The technique of constructing a score that we have used is that of multiple regression, as described in standard statistical texts. One point to note is that when a variable (whether it is the criterion or a predictor) takes the values "yes" and "no" these are scored as 1 and 0 respectively. It can easily be shown mathematically that any two different numbers could be used for yes and no without affecting any predicted probability, but the numbers 1 and 0 form a convenient conventional representation of "yes" and "no".\*

If we apply the most obvious techniques of multiple regression to our data we shall obtain an equation containing an enormous number of variables, most of which import more sampling "error" than useful information. We need a technique which selects a smaller sub-set of predictors, and for this purpose we use what is called stepwise multiple regression. This commences by selecting the best single predictor variable. Then it selects the variable which, taken together with the one already selected, forms the best two-variable predictor set. It then continues at each step adding the best new variable, or deleting a variable of which the information-content is so duplicated by a group of later variables that it no longer earns its keep. Thus we arrive finally at a good sub-set consisting of a manageable number of predictors; not necessarily the best sub-set since there is no reason why the best set of say eight predictors should be reachable by this process of stepwise improvement of the best set of seven, but nevertheless usually a set nearly as good as the best.

The "pattern-seeking" technique used is Predictive Attribute Analysis (Macnaughton-Smith (1965) ). This is described in detail in Appendix A. For

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\*We discuss certain objections to the use of linear regression to predict probabilities, and to the use of dichotomies in regression equations, in appendices B and C.

now it is sufficient to note that this method divides the members of the sample into two groups, those possessing and those lacking the best predictor attribute. (All the predictor variables in this method are dichotomized.) Each group is now treated independently; it is either further divided into the two sub-groups possessing and lacking the best predictor attribute for that particular group, or left alone and defined as a "final set" if no more good predictor attributes can be found. Obviously the new predictor attribute cannot be any of the attributes which already distinguish the group from the rest of the sample. This process of declaring a group to be a final set or continuing to subdivide it continues until all the members of the sample are in final sets, which are defined by the possession or lack of certain attributes.

Both techniques, multiple regression and predictive attribute analysis, involve certain decisions; how good does a predictor have to be before it is good enough? How many variables or final sets enter into an equation before it is said to be too elaborate? We shall discuss these decisions when we come to our actual applications of these techniques to our own data. Both techniques also share the feature that they result in predictions based on far fewer variables than the large number put into the analysis. Techniques which select those variables which appear best on the basis of sample-based data usually work "too well"; that is, by exploiting chance variation in the sample data they produce predictions which are systematically more precise when applied to the original sample than when applied to a second sample. This is why, in the language of Section 10, we used sets  $A_2$  and  $B_2$ , or why in the present study our sample was divided at random into two, so that predictions based on analysis of the study sample can be corrected and their precision measured from the replication sample.

The reader will have noticed that we began this section with a fairly critical account of the assumptions underlying certain past prediction studies, and then proceeded to derive from these same criticized assumptions the two models to be used in the present study; and he may well ask how far has more recent thinking departed from these ideas or provided new ones which we can use here. As mentioned earlier, there is a tendency nowadays, which I welcome, to show an interest in conviction as something done to the convicted person rather than caused by him, and this has changed our view of the worthwhileness of predicting conviction solely from data about the convicted man, and perhaps also our ideas on what data to collect; but it has not had any effect on the mathematical models proposed. Our ideas about guessing the people liable to suffer from the propensities of other people to convict them are still limited to adding up a "perceived badness score" or identifying patterns. Perhaps this will less often be done *a priori*, since we are less certain of what attributes or patterns will be perceived as worse than others.

However, even if the newer ways of thinking have not affected the mathematical models, they have widened our ideas of what might be worth



predicting. If data on an individual are to be used to try to predict what other people will do to him, then the thing that other people will do that we decide to study doesn't have to be conviction or similar "failing". On a population of convicted men one could attempt to predict the sentence that a judge will impose. (Hogarth, (1971).) In our present study we attempt to predict two things that will be done to our sample; they will be granted or refused parole, and, among those granted it, some will in some way or another lose the partial freedom that parole confers. The idea of putting both these events on the same footing, predicting both, and studying the relationship between the two derives, I think, from this relatively new viewpoint, which like all "new" viewpoints is probably better seen as a return to traditions and normalities which have become hidden beneath an overlay of petrified professional habit. Our way of viewing the data themselves also derives from this viewpoint. But in spite of all this, the actual models used, though hopefully more correctly treated than in some earlier criminological studies (Glueck and Glueck (1950)) is a notorious example of a number of elementary mistakes the stubborn defence of which still provides a unifying factor in several careers) do not reflect any new knowledge or hypothesis about the structure of social or individual behaviour.

We have now sufficiently covered the generalities of prediction in criminology to turn at last to the predictive analyses carried out in the present study. For the rest of this section we shall be concerned with the granting or refusing of parole; predicting other criterion variables and relating these different predictions will be undertaken in later sections.

After deciding who to include in the analysis (see Section 3 and Table 2) the first decision was how many different prediction equations to develop. Ideally we would have chosen two: a regression equation and a classification by predictive attribute analysis. Once one has accepted the idea that more than one probability of possessing the criterion can be assigned to the same individual (see Section 10) there is no logical objection to developing as many different predictions of the same event as we wish, except when these predictions are to be used as the basis for decisions, when a "best" prediction for this purpose would have to be chosen; but see Section 10 for the objections to using prediction in this way. But all the same we saw no advantage in the present study in developing larger numbers of predictions than necessary.

However, we did not succeed in keeping the number of our equations down to two. It early became apparent that certain rather important predictors were to be found among the variables suspected of being in an "artifact-dependent" relationship with the criterion (see Section 9 for a discussion of this). The footnote to Table 59 points out that "The Information in Tables 59 - 62 represents statements on the Parole Board's Community Enquiry and Pre-Release reports, or other similar statements on other documents prepared for the Parole Board's consideration." Obviously this

information (and thus dichotomies 127 to 146 which are derived from the same data) is affected by whether a community enquiry into a particular man's case was held or not, and we have already seen (Section 9) that this depends on a decision already partly formed as to the provisional probability of granting or refusing parole to that man. So strictly, any use of dichotomies 127 to 146 to predict the granting of parole involves some circularity. But we could not simply reject these variables, since we had reason to believe that they were also predicting the granting of parole partly for non-artifactual reasons. (Our reasons for believing this are discussed later in this section.) So first we carried out predictive analyses including these variables,\* and then we carried out corresponding analyses, but this time excluding them; and as we had already decided to use two different forms of analysis we wound up with four prediction equations for the granting of parole.

In fact, in the course of developing these four equations we produced still others, but these can be regarded as intermediate steps *en route* to our final four, and not as serious competitors. The reason for these intermediate steps was twofold.

Firstly, we had far more variables than could be included in one computer-analysis using the programmes available to us. We had to handle the variables in batches so as to decide by preliminary analyses which variables to include in later analyses. The manner of doing this will be described below. But secondly, even if our programmes had been capable of handling enormous numbers of variables at one time, it would be undesirable to do so.

We have already explained that techniques which produce equations based on a small selection of "best" variables, chosen by the analysis itself from a larger set of potential predictors, usually work "too well" on the original sample, and "shrink" (lose predictive power) when applied to a second sample. Now we have kept our replication sample precisely to prevent this from rendering our equations invalid. But it will not be much comfort to know that our equations are valid if in spite of this they don't predict very well. A part of my argument later will turn on the fact that the loss of the partial freedom conferred by parole is less predictable than the decision to grant or refuse it in the first place. But to discover this we must not only have valid predictions of these two events, we must have predictions that are as powerful as we can make them. And this "shrinkage" or loss of power is at its greatest when a very large number of variables go into the analysis (just as

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\*One of these variables, dichotomy No. 136, Family Expected to be Supportive, correlated so highly with the criterion that the risk of this being partly for artifactual reasons became a near certainty: in terms of its relationship with other variables we could see no reason for its being a good non-artifactual predictor and the damage we would do in including it was also large. So it was excluded from all these predictive analyses. Also, dichotomy 127, Community Enquiry Carried Out, was obviously inappropriate to include.

the chance that a horse-race will be won by an outsider who will do badly in his next race are greater when the first race has a very large starting-gate.)

So in a sense we are caught in a cleft stick. If we only put a few variables into the analysis we may have left out precisely the ones we need. If we put in too many our prediction equation may include "chance outsiders" who have eliminated the variables which would continue to predict well on further analysis.

To minimize this problem as far as possible we divided our variables into batches. First we ran our two analyses, regression and classification, on batch A. Then we eliminated all those variables that had not shown signs of being good predictors in either analysis, and ran a further pair of analyses on the surviving variables together with batch B. The surviving variables of this second pair of analyses were now combined with batch C and so on until all the variables had had their turn and the final equations included the ultimate survivors. In this way we hope to have reduced the chance of including in the final equation groups of "outsiders" who by chance happened to form a predictive combination in this sample only, without too often having excluded variables of genuine predictive value.

In our analyses we took the decision to use dichotomous variables throughout. Our criteria were the granting or refusing of parole, and (in Sections 12 and onward) the loss or keeping of the partial freedom so conferred. These events are most easily regarded as dichotomous variables to which probability-estimates can be attached, though one could imagine a researcher studying the same subject by using as his criterion such variables as length of time in prison before grant of parole, amount of reduction of incarceration, etc. But the criterion-variables as we defined them seemed to be the most relevant forms in which to express those aspects of the phenomena that we wanted to study. Also, time-duration variables raise grave technical and logical problems when applied to samples observed for differing lengths of time. Given that the members of our sample received or were refused parole at different points of time in the course of sentences of different length, so that some men who were refused parole were imprisoned for less time than some others who received it, the treatment and interpretation of a time-duration criterion variable would be very difficult and the only available solutions would be artificial.

In Predicative Attribute Analysis there is no problem about the variables being dichotomies, since the method was designed for this sort of data, but there are logical, technical and strategic problems associated with our decision to use only dichotomies in the regression analyses. We will deal with one of these now and defer the more technical ones to Appendices B and C.

The objection may be made that it is a waste of information to dichotomize a variable that could be used in a fuller, multi-category or

continuous, form. This is true; but the loss of information is less than commonly supposed. (About two-thirds of the information in a normally distributed variable is kept when we dichotomize about the mean value. Dichotomizing most of the variables in Tables 3-92 will lose much less information.) We are more concerned in this study with the information which one variable contains *about other variables* where the relationship between the two does not necessarily follow any known or simple functional relationship; and in this case the gain in practicality in using clearly defined dichotomies will probably outweigh the loss of information. Also, if we do not dichotomize we face complicated scaling problems, since for any variable with three or more categories we must choose numerical values to attach to these categories; with the sort of data used in the present study any such scaling is extremely arbitrary and can lead to even greater loss of efficiency than dichotomizing does. (See for example the unfortunate experience of Simon (1971) who certainly attacked this sort of scaling problem competently but with rather painful results.) Another reason for using only dichotomies in the regression was that we wanted to see which of our two models, the one based on scores or the one based on patterns, best approximated the decision-making of the Parole Board, and later whatever process deprived the parolee of his partial freedom; and to make these comparisons our data had to be comparable in the analyses by the two methods.

We divided our dichotomies (apart from the criterion, and apart from some purely artifactual predictors that we excluded) into six batches.

First there were those that we decided not to use. To be excluded a dichotomy had either to be in an artifactual relationship with the granting or refusing of parole, or else to possess two properties. It had to show (in Tables 94 and 103) no sign of having predictive value for either criterion variable, granting or refusing of parole and keeping or loss of the partial freedom conferred by parole. Also it had to be derived from the same information (the same table in Tables 3 - 92) as other dichotomies which were included in the predictive analyses. (As stated above, dichotomies 127 and 136 were rejected for different reasons.) The rejection of dichotomies of this sort further reduced the chance of our predictions "working too well".

The remaining dichotomies were divided into five batches. Batch A contained those possibly highly predictive dichotomies (as suggested by high values of  $\chi^2$  in Tables 94 and 103), that reflected how many different steps the inmate had taken, or was seen as having taken, along the path of repeated official criminalization. In other words, was this his first conviction? Was it his first incarceration? Was it his first penitentiary incarceration? In this batch we did not include any account of what the inmate was recorded as having done, but only whether certain specific things were said to have ever been done to him. The dichotomies in Batch A were numbers 31, 32, 33, 41, 42, 43, 44,

46, 48, 50, 51, 53, 54, 55, 56, 58. Batch B contained other possibly good predictors (again as indicated by high value of  $\chi^2$  in Tables 94 and 103), but we excluded from this batch those where we were suspicious of artifact-dependent relationships, and we also (for convenience) excluded dichotomies based on time-intervals. The dichotomies in Batch B were numbers 15, 34, 37, 39, 61, 65, 66, 155, 156, 176, 178, 181, 194, 195, 197, 198, 199. Batch C was the batch of dichotomies previously discussed, based where possible on statements made on the community enquiry and suspected of being in an artifact-dependent relationship with the criterion. However, if the only association between these dichotomies and the criterion had been artifactual and dependent on what happened to the "not known" and "does not apply" categories, we should have expected two things; that these dichotomies would be very highly associated *with each other* (as measured by  $\chi^2$ ) and that when the "not known" and "does not apply" cases were excluded the dichotomies would no longer be highly associated with the criterion. Neither of these anticipated results was found, and the most reasonable interpretation was that these dichotomies were associated with the criterion both for artifact-dependent reasons and also for less circular reasons. The only reasonable course was thus to run two sets of prediction, with and without this batch. The dichotomies in Batch C were 128, 129, 131, 132, 133, 134, 135, 138, 145. When Batch C was excluded, dichotomy No. 145 was included in Batch D. As explained above, dichotomies 127 and 136 were entirely excluded from the analysis. Batches D and E included the remaining dichotomies, which had relatively low associations with both the criteria. Batch E had lower values of  $\chi^2$  than batch D, except that a group of dichotomies based on time periods calculated by us from recorded dates (as opposed to those recorded on the files as time periods) were included in Batch E. To reduce the size of Batch E a few dichotomies with very low associations with both criteria were excluded.

The dichotomies in batch D were numbers 6, 16, 20, 30, 49, 52, 64, 73, 87, 92, 122, 126, 23, 55, 157, 165, 171, 14, 59, 65, 66, 67, 108 and No. 145 when batch C was excluded.

Those in batch E were numbers 5, 8, 40, 113, 117, 123, 140, 142, 158, 162, 170, 193, 25, 45, 62, 68, 80, 83, 115, 116, 146, 166, 168, 174, 175, 188, and the five time-interval dichotomies t1, t2, t3, t4, and t5.

As we said, these analyses led to four predictions of the granting or refusing of parole. However, they still needed replication and adjusting, by application to the replication sample. (Since this is the first occasion in this study that we have used the replication sample, we should mention that various checks showed that it was satisfactorily comparable with the study sample.) The four equations in their final form are set out below as equations 1 and 2 and Tables 101a and 101b. (Predictive attribute analyses are more easily represented as tables than as conventional equations.)

What can we say about these equations? How well do they work? The effectiveness of the regression equations is best measured by  $r$ , the correlation coefficient. For the predictive attribute analyses the corresponding measure is  $\phi$ . Both  $r^2$  and  $\phi^2$  measure what proportion of the original variance is "explained" by the equation, so that  $1 - r^2$  or  $1 - \phi^2$  represents the proportion of whose origin we are still in ignorance. Thus, although our values of  $r$  and  $\phi$  are higher than usually found in criminological prediction, we must remember that in our four equations, from one-half to seven-tenths of the variance is still unexplained.

The predictive attribute analyses are easy enough to understand. In each row we can see how many men were granted or refused parole. To give a comparable picture of the regression equations we show, in Tables 102a and 102b, a summarized form of the distributions of predicted probabilities (scores) for the men granted and refused parole in the replication sample. To make this summary reasonably comparable with the predictive attribute analyses we have divided the range of scores into 5 bands and show the numbers of men granted and refused parole in each band. Note that these bands are for convenience of explanation and summary only; the members of a band in no way form a "type" nor should a man's individual probability ever be replaced by a central value for his band. Furthermore, from what we are about to say in the next paragraph but one, we do not regard people in the middle bands as being "the unpredictable group". The tables are solely to enable the reader to form the same sort of overall impression of the results of the regression analyses that he can form of the predictive attribute analyses.

It is difficult to say whether to regard our analyses as yielding a good level of prediction or not, since as far as I know there have been no other attempts to predict the same sort of phenomenon from comparable data. Ballard and Gottfredson (1964) in "Estimating Prison and Parole Terms Under an Indeterminate Sentence Law" obtained replicated correlation coefficients of which the greater ones were of similar size to ours (theirs ranged from .34 to .74) for two non-dichotomized criteria (length of time to be served before parole, and on parole) in an indefinite sentence system where all releases are on parole. These figures relate to a task basically different from ours, but I know of no other remotely comparable studies. Certainly our figures are higher than those obtained in studies that predict reconviction, the favourite target for predictive efforts.

Apart from  $r$  and  $\phi$  there seems to be (especially in criminology) a whole host of measures of the "goodness" of a prediction which are based on the number of individuals "correctly or incorrectly classified". That is to say, if you possess the criterion and your predicted probability of possessing it is greater than .5 (or if you lack it and the predicted probability is less than .5) you are said to be "correctly predicted to possess (or lack) the criterion" or, more shortly, to be "correctly classified". Now if your aim is correct classification, defined as allocating possessors and lackers of the criterion to

two sets, "predicted possessors" and "predicted lackers", then you can devise techniques to do it as nearly as possible, but prediction theory as discussed in Section 10 is not necessarily the best approach. Also, these measures of goodness assume that if an event occurs, any estimated probability of less than .5 is equally useless. But think of the worker who learns that one-third of his factory are being laid off, or the married pair who learn that if they have a child it has a one-in-eight chance of being seriously handicapped, and it becomes obvious that the numerical value attached to a probability may be very important apart from the questions of whether it is larger or smaller than some magic number such as .5.\* All dealing in probabilities, including prediction, involves the acknowledgment of uncertainty, usually large amounts of uncertainty. Prediction is not meant to be infallible prophecy, and (apart from simple technical mistakes) people only judge it as though it should be when, like some of our contra-criminals, their role in life is partly that of the false god who cannot afford to admit he does not know and to act accordingly; or when, like certain researchers, they have (perhaps unwittingly) allowed their role to be defined as including the service of such idols.

We may regard our equations as giving a summarized version of the qualities of a man seen as parolable (or, at least, most likely to be treated as parolable) by the Parole Board. It would seem that parolability in this sense may better be seen as achieving a good score on an implicit scale of uniformly applicable ideas of alleged goodness than as the fact of belonging to one of several types each with its own *distinctive* virtues. We infer this not so much because the regression equations work slightly better than the predictive attribute analyses, as because even the latter look more like assemblies of generally "good" qualities rather than a "typology" in any less primitive sense. Whether the qualities regarded as constituting parolability are appropriate or not we shall see later (insofar as *any* quality should be seen as constituting "prisonability") but here we simply note that nothing in this section would cause us to modify our findings in Section 9 about the apparently goal-less bureaucratic orientation of the Parole Board. These findings were based on analyses of individual dichotomies applied to the whole study sample, and we had to suspend our assent until we had seen whether they were reversed or modified by our analyses in the present section of groups of attributes and sub-sections of the sample. Our findings were not so reversed or modified.

This is as far as we can conveniently take these matters without reference to an external criterion. The search for this criterion and the study of it, and of its relationship to the matter we have already considered, will form the third part of this book.

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\*For example, all the political and public concern about the alleged "increase in crime" is a concern about events whose *individual* probabilities are all *greatly* less than fifty per cent. So, usually, is concern about epidemics.

## PART III

Which men do they shut up again,  
and why?  
What is this all about?



## Section 12

### The idea of success in losing or keeping the partial freedom conferred by parole

Part III of this book follows a similar plan to parts I and II combined. There we proceeded from general considerations to a study by certain specific techniques of the granting or refusing of parole. Our interpretation of our findings was constantly limited by the need to wait until we could compare them with what we shall find in this third part. Here we shall again proceed, though more rapidly this time, through a few further general considerations to a study by the same specific techniques, this time of one aspect of the "success" or "failure" of parole. The aspect we have selected, for reasons discussed below, is simply whether a paroled man keeps or loses the partial freedom conferred on him by parole. Our analyses and interpretations will be not only of our findings on this subject but also of their connection with the findings on the granting or refusing of parole that we studied in part II.

Most of the general ideas on which this third part depends have already been discussed in parts I and II, so that this present relatively short section corresponds to the whole of sections 1-8. After that the correspondences are as follows: Section 13 corresponds to Section 9, Section 14 to Sections 10 and 11, Section 15 relates Part III more fully to Parts I and II, and Section 16 considers the implications of the whole study.

In Section 5 we considered the aims of what is commonly called justice and corrections but which I prefer to call contra-crime. In Section 6 we concluded that it was "unlikely that we shall seek any opportunities to examine "causes of crime" or "effects of treatment", those two will-o'-the-wisps that have for ninety years led most criminologists to drowning. Instead we shall in all situations look at processes that can be observed to

occur, stating very clearly the rare occasions on which acts or processes are assumed by consent rather than observed. And our studies of values and criteria will either be of those which we assert on our own responsibility, or of those which we can infer from the acts and processes we observe; but not usually the assumed "aims of society" or officially claimed functions of institutions." So we must explain how our decision to study the keeping or losing of partial liberty arises from applying the outlook just quoted to a study of the Parole Board.

One common and simple measure of performance in any undertaking is to what extent it succeeds in doing those things that it visibly attempts to do. Thus we may study whether a police department actually arrests those people it is seeking to arrest, or whether a prosecutor wins his cases; but I see no useful application of this concept to our present study of parole. The number of cases in which the Parole Board decides to release a man but finds that by *force majeure* this does not occur is probably negligible if not zero; and the contrary case where they refuse parole to a man who is then released by some other power (or successfully escapes and eludes recapture) contrary to their wishes may equally well be ignored. There are more complicated cases such as that of the parolee who disappears from the sight of his supervising officer, and similar frustrations of the Parole Board's or Parole Service's actual physically observable operations regarding the continuation or withdrawal of parole, but these cases are too complicated to be studied here. (I do not include in this last sentence the Parole Board's own *reversals* of *previous* intention; this we shall come to next.) So we cannot set up as our criterion variable this particular value (the achievement of the visible direct goals of visible activities) and as we have said, we do not wish to import the "assumed aims of society or officially claimed functions of institutions." What does this leave us?

The most obvious feature of parole is that men are released from prison into partial liberty.\* It does not seem far-fetched to regard the subsequent loss of that partial liberty by any reimprisonment, however caused, and whether brought about by the Parole Service itself or some other agency, as contrary to the intention that can be assumed to have existed when the decision to grant parole was made. This is not logically inevitable. Parole could be granted on what was intended to be temporary basis, or as a short-term "good" in total indifference to how long it lasted; or what the mental hospitals' apologists call a "revolving door" philosophy might be adopted, though there is a big difference between a door which revolves at the touch of the man who wishes now to go out, now to come back in, and one, be it in prison or in mental hospital, which revolves while rival or collaborating agencies hurl or coax their human cargo backwards and forwards. But in any event, in the cases of

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\*But see Section 9 p. 53 et seq.

parole that we are studying we have no reason to suppose that any decision to grant parole was originally intended to be reversed.

Nevertheless it frequently occurs that a man on parole loses this partial freedom, and for the reasons we have indicated in this section and in Sections 5 and 6 we shall choose this event as the main phenomenon to be studied in the third part of this book.

One point needs to be made clear that in discussions during the writing of this book has sometimes been misunderstood. The loss of the partial freedom conferred by parole is a fairly easily defined and observed event. Other concepts such as "degree of rehabilitation" or "reduction in criminal activity", frequently proposed as criteria in studies of "effects of correctional treatment", are vague in nature and measures of them are hard to define or develop. It has been suggested that this greater clarity is our main reason for having chosen loss of partial freedom as the criterion variable in the second part of this study. Now our actual reasons for rejecting phoney criteria, such as the rehabilitating results of selecting and annoying people, have been set out often enough in this book already, and difficulty of measurement was not one of them, except in so far as this difficulty might arise from a self-contradictory element in the proposed criteria themselves. We should have enjoyed trying to give precision to a vague criterion if it had been a reasonable one to choose on other grounds.

Nor was the re-imprisonment of a man on parole selected for study merely on grounds of its being a reversal of an earlier decision, the only criterion that survived logical analysis when all the more interesting and relevant criteria had succumbed to our debunking tendencies. As I have argued in many places in this book, but especially in Sections 5 and 6 it is hard to see any essential feature in "justice and corrections" other than choosing, labelling and officially annoying certain members of society. The reasons alleged for these activities vary from age to age, and I see no great reason to do research into topics such as rehabilitation or reduction of crime (social defence) suggested purely by the latest rationalizations of why society officially annoys people, when these rationalizations are direct contradictions of other equally plausible ones offered at other times and places, or even at the same time and place, to justify the same activities. But a tradition is still faintly remembered even in some "justice-and-corrections" quarters, that freedom is a good thing not only for its possessor but for the society in which he lives and that deprivation of freedom is bad for the deprived, the depriver and the society of which they are a part. This implies that the questions of whether certain people are at liberty or not, and who is depriving them and why, may be among the most relevant questions in criminology. Now it would be ridiculous to suppose that a person "serving a sentence in the community" or "being required to accept help and support" possess liberty

in anything but a degenerate, politician's *cliché*, sense of the word;\* nevertheless, these people are almost certainly less inherently deprived of liberty than are prison inmates; and so it is not purely for its logical value that the avoidance of, or return to, incarceration of parolees was chosen as the object of study in the second part of this project.

Since most of "what the researchers did" was described in Section 4, a very few further notes will be necessary. In the study sample defined in Part I, 143 men out of 406 were granted parole. These 143 men form the study sample in Part III. A corresponding 147 men were granted parole out of the 426 applicants in the replication sample in Part I, and these 147 men form the replication sample in Part III.

Now parole can last a long time. Some of these men are still on parole at the time of writing. We decided purely from practical necessity to study the full period of parole whenever it was less than three years, but to impose a cut-off to our observations after three years no matter how long the intended period of parole. In any such situation there will be a number of men who remain partly free on parole for a long time and then are deprived of this, but they are likely to form a small part of the whole, and however interesting from the personal point of view their stories may be, they probably add little to what our research can discover without them.

So we set the necessary time limits to our observations at three years. We must note that this means that different men were observed over different lengths of time to see whether or not they lost their partial freedom. In most prediction studies the follow-up period is as nearly the same for each man as can be managed, and departure from this is usually a sign of the researcher's incompetence. In the present study this variability was an essential part of the phenomena being studied, and we had to adjust our analysis to cope with it. Now we have to decide what events we shall treat as loss of partial freedom. Such a decision cannot avoid being to some extent arbitrary. We endeavoured to keep as close as possible to what would be experienced by an ordinary citizen, not on parole, as a marked reduction in liberty, but we were limited by the incompleteness for this purpose of the information on the file and by the logical requirement to *define* reduction of liberty rather than to decide intuitively whether to impute it to each man's pieced-together story. Finally we decided to treat *any* suspension, revocation or forfeiture of parole, any arrest or any incarceration, or any "taking in" for questioning as a suspect, as loss of liberty, and no other event whatsoever. Border-line cases arise when orders removing parole are made but not carried out, usually because a man has moved his address, for good or bad reason, but before he has received his "supervisor's" permission. In such a case he will spend some time at some risk of arrest. Another form of border-line case is when parole is suspended or

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\*See Section 6, especially footnote 2 to page 27.

revoked and then restored, or forfeited and a new parole granted. But in all these cases, even a suspension pending an enquiry, the man will spend some weeks or months incarcerated while the parole service generates paper about his liberty.

The third problematic case is that of arrests not followed by sentences of imprisonment nor by any loss of parole. However, at exactly what point society's official treatment of suspects and petty offenders amounts to an important deprivation of liberty is a question of degree. Turk (1969) makes a strong case for treating the fact of arrest as a punishment in itself, and since this chimed with the availability of data, we have followed him in this. If we had been measuring "rehabilitation" or "success of treatment" we might have made a different decision, but we were not, and certainly being arrested constitutes some loss of liberty. We included for this purpose being "taken in" as a suspect.

The problem with all these definitions is that the phrase "loss of partial freedom" is a short phrase covering a wide range of complex phenomena. Indeed we need full-scale phenomenological studies of freedom and its absence before we can make much further progress in this area, or appropriately set the limits of the criterion variable in the second part of the present study. But these phenomenological studies do not yet exist, and we have to settle the detailed definition of our criterion very arbitrarily. It appeared from analysis made on alternative definitions of the criterion that the decision we in fact made was certainly reasonable and probably the best from a technical point of view.

In our study sample of 143 men, 39 lost parole (all those whose parole was suspended finally had it revoked or forfeited) and 13 others were arrested at various times, so that we counted 52 people as losing the partial freedom conferred by parole. In the replication sample, out of 147 men 31 lost parole (one on suspension only, the others, sooner or later, by revocation or forfeit) 12 were arrested and two were taken in on suspicion by the police; this amounts to 45 who lost partial freedom.

Thus permanent or temporary loss of parole, arrest, taking in for questioning as a suspect, and the variable that summarizes all of these, "loss of partial freedom conferred by parole" as we have just defined it, are the only new variables introduced into Part III of this study; and the vast majority of our analysis are in terms of loss of partial freedom conferred by parole.

## Section 13

### The relationship between the loss or keeping of parole and individual items of information, and a comparison with Section 9. More about purposeful and bureaucratic activities

In Section 9 we broke down our data into a set of dichotomies and measured the relation of each to the granting or refusing of parole by means of  $\chi^2$ . We now (Table 103) do exactly the same for the relationships between these same dichotomies and the keeping or loss of partial freedom.

Note that in Section 9 we were dealing with a study sample of 406 imprisoned men, whereas now our study sample contain only the 143 who received parole. Thus, if the relationship between any given dichotomy and our second criterion is of the same strength as that between the same dichotomy and the first criterion, examined in Section 9, we should expect our new  $\chi^2$  to be only about one-third as large as in the former table. Since the value of  $\chi^2$  required to reach a given significance level is the same in all cases, this means that a relationship nearly three times as strong is needed to exclude the possibility that it has arisen purely by a chance of a given magnitude in the sampling.

Table 103 shows results similar to those found in many other studies. In very general terms, the best predictors of loss of partial freedom are similar sorts of events (convictions, incarcerations, etc.) in the past. However, almost any alleged fact will relate more strongly than measures of opinion or of prognosis. Among opinions, very general ones amounting almost to simple unexplained "good-bad" labels (such as dichotomy 138, "anticipated to have problems",  $\chi^2 = 8.446$ ) will be more strongly associated than more focussed judgments (dichotomies 139-146, on particular anticipated problems).

The question obviously arises, to what extent are the same dichotomies associated both with the granting of parole and with loss of partial freedom. To answer this we compare the  $\chi^2$  for each dichotomy in Table 103 with the corresponding  $\chi^2$  in Table 94. If we call values of  $\chi^2$  of less than 3.841 (the value at the 5% significance level) "small", those between 3.841 and 6.635 (the value at the 1% significance level) "medium" and those greater than 6.635 "large" we find that the dichotomies fall into 9 groups. (See Table 104.)

Of the first group nothing is remarkable save its size. These are, roughly, the items on the file which, whatever other purposes they may serve, bear very little relation to the main topics of the present study, either because they are about something else entirely, or because the extraction of this particular dichotomy from a wider question has hidden the relevance of the wider question. In this latter case some other dichotomy based on the same variable will usually turn up on one of the other groups in Table 104.

Groups 3, 7 and 9, the groups where both the  $\chi^2$  are either large or small, are the easiest to interpret. Very roughly, (and we shall discuss the limitations on these interpretations shortly) group 3 are those dichotomies representing information relating to a man's chance of keeping or losing his partial freedom, but nevertheless not considered, or not effectively considered by the Parole Board in deciding whether to grant him parole or not. Group 7 is the contrary group, those items which are closely related to the Parole Board's decision to grant or refuse parole, though not at all strongly related to the paroled man's chance of keeping or losing his partial freedom. Lastly, group 9 is the group of items importantly related both to the granting or refusing of parole and to keeping or losing partial freedom.

In spite of all I have said in this book, I suppose there will still be some readers who regard group 9 as the most important, as relating to the "aims" of this corner of the "justice and corrections" industry and most likely to lead to an understanding of its "problems". So it is pleasant to record that this group is extremely dull, though revealing in a negative way. Out of the vast mass of data sifted in the present study, we find 15 items clearly predictive both of whether a man will be released into society or kept incarcerated, and of whether or not he will be re-incarcerated by that society (or by the people that released him). Of these 15 items, all but four are simply measures of the extent to which he has been incarcerated, or at least tried, in the past; and most of these mainly separate first "offenders" from others. There are four remaining items. From dichotomy 14 we observe that men on long sentences are more likely to receive parole (their case comes up for consideration more often) and also more likely to lose their partial freedom (perhaps simply because of their being on parole, and thus at risk of losing their parole, for a longer period. From dichotomy 65 we observe that people whose past criminal records include robbery among their three most "serious" types of

conviction are less likely to receive parole and more likely to lose their partial freedom; from dichotomy 138, that in the very select small group on whose files no-one has recorded their anticipation of any problem while on parole, there is a very high probability of receiving parole and a very low probability of losing partial freedom. Lastly, from dichotomy 176 we observe that men recorded by the classification officer as unemployed at the time of their offence are less likely to be granted parole and are more likely to lose partial freedom.

That is all. All the information that is clearly relevant and also clearly used can be put on the famous ‘‘half-sheet of foolscap’’. The whole process of enquiries, interviews and deliberations can, so far as any question of protecting society is concerned, be summed up by saying that the best guess how the so-called guardians of society will treat this man in the future is based more on how they have treated him in the past than on any other factor.

But of course, there is no evidence and no logical reasons to believe that the Parole Board, or any other part of the counter-crime system, exists to protect society or to achieve any other objective goal. Thus we find a considerable proportion of our total dichotomized information falls in group 7, dichotomies related to the granting or refusing of parole but not to the subsequent keeping or losing of its partial freedom.

Because the sample of paroled men is smaller than the sample of applicants (we cannot with certainty assert any other reason) there are fewer dichotomies in group 3 (good predictors of loss of parole’s partial freedom which are not related to the granting or refusing of parole). Since they are so few we can dispose of them at once. From dichotomy 23 we observe that the two groups of people applying for parole who were over and under 35 years old at the date of sentence have a roughly equal chance of being paroled, though the under 35’s are more likely to lose their partial freedom. However, as we shall see in Section 15, in spite of the lack of association with granting parole, we cannot assume that the Parole Board ignore this fact in reaching their decision. In the same way we shall see that the apparent ignoring by the Parole Board of two measures of past gaol experience (dichotomies 55 and 57) which are strongly related to later loss of partial freedom may well be misleading.

This leaves dichotomy 61, from which we observe that although in our sample people whose earlier convictions include as the most serious a charge of an offence against the person (in the sense of the terms ‘‘most serious’’ and ‘‘offence against the person’’ already used in this study in Table 38 row 3) have a lower chance of receiving parole than other offenders, this difference is not large enough to be statistically significant, although the greater chance that the paroled men among them have, compared with other men on parole, of losing their partial freedom is statistically very significant indeed ( $p. < .0001$ ).



One could imagine a very good defense for the custom of paroling such groups of men in the face of their being "worse risks" than others, if one saw the Parole Board's activity as a protest against a savage over-reaction of contra-crime against this group of people, who may in more extreme cases have "committed horrifying crimes" but quite often have simply at some point in their lives reached a tedious but all too visible crisis in a dreary chain of personal relationships; but the Parole Board goes to immense trouble to deny having such a protesting role\*, and nothing in any of our other analysis prompts us to take them at anything but their own evaluation as collaborators in the "aims", by which we mean the aimlessness, of the contra-crime industry. Still, the possibility remains that the Parole Board is in this instance making decisions for humanitarian reasons.

Let us return to group 7, those dichotomies not related to the loss of partial freedom conferred by parole, but nevertheless strongly related to the granting or refusing of parole. At this point two objections may be raised to the logic of the approach I have been taking. With few exceptions I have been treating dichotomies with low  $\chi^2$  when cross-tabulated against loss of partial freedom as irrelevant to this issue, and those with low  $\chi^2$  when cross-tabulated against granting of parole as not being effectively used in making this decision. Now let us imagine an attribute (say, having a cloven hoof) which *rightly or wrongly* the Parole Board considered prognostic of a speedy return to prison. Then, their defenders might say, they might react in two ways. They might grant parole to a far smaller proportion of men with cloven hoofs than men with toes; and they might only parole cloven-hooved men who possessed a higher level of other good prognostic signs than they demanded from toed men. Now even if they were right in their views of the evils of cloven hooves, if they reacted in either or both of these ways, we might find a low  $\chi^2$  when we examined the association between cloven hooves and loss of partial liberty. Thus my treatment of such a dichotomy as "irrelevant" to loss of partial freedom might be inaccurate.

The first of these two reactions, granting fewer paroles to men possessing a certain attribute and thus reducing the second  $\chi^2$  of a relevant dichotomy, can occur, but when it does we can easily see it, since Table 94 shows the numbers of paroled men possessing and lacking a given attribute. In general it does not explain away many of our low  $\chi^2$ . When several dichotomies appear to convey very similar information but to have different associations with loss of partial freedom, this sort of phenomenon may underly this variability. But

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\*c.g. "It is not a question of interfering in any way with the sentence of the courts. If we are to have a parole system to help reform offenders, we must let them out on parole . . ."

Street, G. (1960) p. 1

"The Board is definitely not a reviewing authority. It does not review the propriety of the conviction or the length of the sentence . . . Parole is not designed to shorten the sentence of the court . . . Parole is not a matter of mercy or clemency. It is not granted on compassionate or humanitarian grounds".

Ibid, p. 2.

most of our low  $\chi^2$  in Table 103 are based on figures large enough to produce large  $\chi^2$  if there had been any strong relationship between this attribute and loss of partial freedom.

The second reaction mentioned above to cloven hooves, etc., of demanding a higher level of other good prognostic signs before granting parole, is also logically possible, and it is for this sort of reason that some things we say are modified in this section in anticipation of Sections 14 and 15. But because of the nature of human decision processes this reaction is unlikely to happen often, and it will occur mainly in very clear simple cases. The reader may be assured that we do not in this section offer interpretations that have to be withdrawn in later sections. However, if he so wishes he may suspend his assent to (or dissent from) the present section until he has read Sections 14 and 15.

Because of these and similar factors, we only wish to make one comment on the fact that dichotomies 15, 42, 43, 49, 51, 64, 147 (artificial relationship with granting parole) 178, 194, 198 fall in the group that we are discussing (group 7). In some of these cases so few of these men are released on parole that there can be very little evidence in favour of refusing it to them. In our imaginary example of the men with cloven hooves we have to ask, if very few of them are ever paroled, why was it ever supposed that this category was not fit for parole. Sometimes, more rarely than we suppose, there may be external evidence, but frequently, as in the case of data relating to guesses about industriousness, future occupation and so on, the supposable reasons for refusing parole are contradicted by such slight evidence as they do not succeed in blocking off.

However, this leaves the rest of group 7, the dichotomies 16, 37, 39, 53, 73, 87, 123, 126, 127, 128, 129, 131, 132, 133, 134, 135, 136, 145, 156, 181, 195, 197, and 199 to consider.

Table 105 summarizes the entries in Tables 94 and 103 for these 23 dichotomies. For each dichotomy, it gives the probability that a person for whom the reply was yes would receive parole, and, for those of them who did, the probability that they would keep the partial freedom thus gained. The same two probabilities are given for those for whom the dichotomy's reply was no.

Such a summary table must be treated with great care. As far as possible the original data in Tables 94 and 103 should be consulted. But two things can be said. The dichotomies in the table all come from group 7 in Table 104. This means that the difference in probability of receiving parole between the yesses and the noes for any dichotomy in this table is always too large to be attributed to chance, but that the corresponding differences in probability of keeping partial freedom for those of the yesses and noes who receive it are always small enough to be chance effects.

A set of results small enough to be due to chance can be interpreted in two ways. Firstly we can regard them as not providing much evidence, and in this case we shall have no adequate reason for supposing that the possession or lack of *any* of these attributes by a parolee makes any difference to his probability of keeping or losing his partial freedom. Yet we note that people recorded as having previously had parole, or not having been the subject of a community enquiry, or not being estimated (in the course of the community enquiry or similar investigation) to have a "supportive family", whatever that undefined term may mean, those for whom it is *not known* whether their family has a criminal record (this is apparently more damning than its being *known* that your brother *has* such a record, see dichotomy no. 157 in Table 94) those for whom it is *not known* whether before his incarceration he had received trades training, and those who do not receive on the custodial report a favourable recommendation for parole, have very little chance indeed (less than 15%) of receiving parole; and that people whose files include the "wrong" answers to any other dichotomy in this list will have a substantially reduced chance of receiving parole, without any sufficient evidence that they would be less "suitable" in terms of any greater risk of losing their partial freedom.

There is a second way of regarding the statistically non-significant second columns of Table 105. Instead of regarding it as insufficient evidence of any differences in the probabilities of keeping or losing partial freedom, we can accept the data at their face value, estimating for example that parolees who have been fined (dichotomy 37) have a chance of keeping their partial freedom 8.7% less than those who have not been fined. But if we accept this difference it is logical to accept *all* the differences in Table 105, not just some of them. If, because of past beliefs, we accept some of these differences and reject others, we should admit that our choice is neither supported nor refuted by the present study, and should then consider specifically what other evidence or mythology gave rise to it.

Here we are concerned almost entirely with the interpretation of the data in the present study.

If then we accept all the differences in the second column of Table 105, how shall we interpret them? Of course, they do not affect what we have already said about who has a greatly diminished chance, or very little chance at all of receiving parole. We find that only in four cases do the paroled men in the group with less chance of receiving parole have a chance as much as 10% greater than the other parolees of losing their partial freedom; in other words, if there is any case at all for refusing parole to a large number of applicants, then such a case could perhaps be made out (but not strongly supported; see above) for the Parole Board's practice of granting parole less frequently to people who are not said by the community enquiry to be going to live with a wife (dichotomies 122 and 131) or who were anticipated to have personality

problems (dichotomy 145) (though this is said of more than two-thirds of the sample) or who did not receive favourable recommendations on the custodial report (dichotomy 199) but even in these four cases the observed *large* differences in chance of being paroled do not seem justifiable. In the case of the other dichotomies, the difference in chance of losing partial freedom is small, and even accepting these non-significant differences no case can be made out for the reduced chance of receiving parole. In 11 dichotomies (marked with an asterisk in Table 105) the Parole Board even shows itself more given to releasing those with a higher chance of losing their partial freedom; in five of these cases this "difference in the wrong direction" is greater than ten percent. It is interesting to observe which values of the Parole Board are thus brought into doubt.

However, in general we prefer the first of the two viewpoints given above, the one which doubts the "reality" (in the statistical sense) of the non-significant differences, and thus avoids the detailed interpretations of the last paragraph; but since this viewpoint perhaps leads to some contempt for the Parole Board's decision process, it seemed necessary to set out the alternative mode of interpretation, which however, while changing very slightly certain details, left the overall picture much the same.

Returning to Table 104, the dichotomies in remaining groups (groups 2, 4, 5, 6, and 8) give rise to at least one medium value of  $\chi^2$ , and thus we cannot conveniently make any clear interpretation of what if anything they reveal about the relationship between receiving or being refused parole and keeping or losing partial freedom.

There is one dichotomy in group 1 on which we have already implicitly undertaken to comment. In Table 5, (row 2) we observe that 41 percent of our sample were in the Quebec penitentiary of Saint Vincent de Paul, and in the footnote to the table we say ". . . there is already ample evidence that people are more harshly sentenced for equivalent convictions in Quebec than in the rest of Canada (see Jaffary, (1963) ). Thus this figure of 41% would be expected to contain "better parole material", or at least "less criminal types" than in some other provinces, if by this we mean people who in other provinces would have been seen as "deserving" a lighter punishment than a penitentiary sentence. What treatment is accorded these people will be seen later in the study".

We see from dichotomy 1 in Table 103 that, of the 59 men in the study sample paroled from Saint Vincent de Paul, 18, or 30.5 percent lost their partial freedom. Of the 84 men paroled from other penitentiaries, 34, or 40.5% lost their partial freedom. This difference could well occur by chance ( $\chi^2 = 1.488$ ,  $p = .12$  on a 1 - tailed test since the difference is in an already predicted direction) or could equally well be support for the hypothesis that

the paroled Quebec members of our sample were "better parole risks" than the others. We see from the tables that of the 165 men of our study sample in Saint Vincent de Paul, 59, or 35.8% were paroled. Of the 241 men from other penitentiaries, 84, or 34.9% were paroled. This difference is negligible, which is fair enough if the men of Saint Vincent are in fact equal "risks" to the others, but might at first sight appear to be politicians' "quota justice" rather than "deal(ing) with offenders as individuals, not as members of a group" (Street, (1960), Handbook on Parole, which may be regarded as a statement of official policy) if, since we know that they include victims of harsher sentencing habits we accept the hypothesis, (lightly supported by our data) that they include "better parole potential". In this case, if each one were really to be treated "as an individual" they would between them "deserve" a higher proportion of paroles. However, this situation probably arises merely because, as we submit later (Section 15) the Parole Board, in granting or refusing Parole, does not in general succeed in distinguishing between good and bad risks in any province.

The rest of this section is the appropriate place to take up a topic left open in our earlier discussions. In Section 9 we wrote of the orientation of the Parole Board. In the absence of any real goals for contra-crime we thought that such an orientation would be very bureaucratic. In fact we found from our data that the Parole Board regarded (or acted as if they regarded) the source of an item as more important than its topic, (which we saw as a bureaucratic characteristic)\* and further, that they set up a particular ranking for the importance of their sources which fitted very well what we should expect from such a bureaucratic orientation. But our argument was not conclusive because two lines of reply were left open. The first was the simple "appeal to results", the claim that to a reasonably successful degree the Parole Board had paroled those people that it ought to have paroled and had left unparoled those people that it ought not to have paroled. We shall see what happens to this idea in Section 14.

A second criticism of our interpretation would be that we had classified the dichotomies into topics in a way that was irrelevant to the aims of the Parole Board, and that a more realistic classification into topics would reveal these to be treated by the Parole Board as more important than the sources of the dichotomies. What we have already seen in this section does not inspire confidence in this line of argument, but a more direct examination of its merits is possible.

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\*We ourselves in the present study have paid more attention, and have recommended other researchers to pay more attention, to the existence and origin of data than to the alleged "information about the criminal" that it contains (see Section 7). The reader may consider why this is a profitable attitude for a researcher but is bureaucratic and revealing of a lack of real goals when found in the Parole Board.

Accordingly we carried out a further set of analyses of the type discussed in Section 9, (see Tables 95 - 100) but this time using loss of partial freedom as our criterion. That is, we took all the dichotomies whose association with loss or keeping of partial freedom produced in Table 103 a value of  $\chi^2$  of greater than 5.412 (the value at the 2% significance level). There were 27 such dichotomies. (In Section 9 where our criterion variable was the granting or refusing of parole, we took the 57 dichotomies whose  $\chi^2$  in Table 94 were greater than 6.635, the value at the 1% significance level. If in the present section we had taken a lower limit of 6.635 for  $\chi^2$  we should not have had enough dichotomies to carry out a reasonably worth-while analysis; on the other hand, if we had taken approximately 57 dichotomies we should have included some very low values of  $\chi^2$ . The selection of the 27 whose values of  $\chi^2$  were greater than 5.412 seemed a reasonable choice.)

Next, as before, we eliminated those dichotomies which might duplicate information in other members of our list, retaining each time the one with the higher  $\chi^2$ . Five dichotomies were eliminated. For technical reasons we also had to eliminate dichotomy No. 138. It was the only one of our 22 dichotomies in that particular topic category *and* the only one in that source category. Thus it was impossible to distinguish between the importance of source and of topic for this item. The remaining dichotomies were then ranked, a rank of 1 being given to the dichotomy with the largest  $\chi^2$  and 21 to the one with the smallest  $\chi^2$  that was still on our list. They were also allocated, as in Section 9, to groups representing their sources and their topics. As far as possible an exact parallel with the analysis in Section 9 was preserved, but in the amalgamation of the very smallest groups slight changes had to be made.

The results of this analysis up to this point are shown in Table 106 where our 30 dichotomies, the  $\chi^2$  measuring their association with keeping or loss of partial liberty, the ranking of the 21 dichotomies left in the analysis, and their source and topic group are shown. We also give the number of dichotomies and the mean ranking for each source-group and topic group. Note that out of the seven sources and seven topics in the previous analysis only four of each are now represented. The reader may occupy some moments by interpreting where possible the order of importance of the topics and the groups, but what there is of importance in this occurs sooner or later in other sections of the book and in any case is not our main concern here.

All the analyses of variance carried out in Section 9 were now repeated on the data of Table 106. The results are shown in Tables 107 and 108, from which we obtain a very different story from the corresponding analysis in Section 9. As far as relationship to loss of liberty is concerned, the importance of a dichotomy is strongly related either to its topic or to its source; but the relationship to source disappears if we consider topic first, and the relationship to topic falls well below significance level if we consider source first. Thus the reader may interpret the value of a dichotomy as a predictor of

loss of partial liberty as depending largely on topic (more than 60 percent of the sum of squares in our second analysis (Table 108) is accounted for by this one factor) in which case he will certainly see its source as unimportant; or he may with slightly less justification regard its importance as depending on source (50 percent of the sum of squares accounted for in our first analysis (Table 107) ) in which case he will probably see its topic as having little or no importance; or he may adopt an intermediate position.

This establishes that our classification of dichotomies by topic has been reasonably well done; the distinctions between topics that we have made cannot be purely arbitrary since we have shown them to be relevant to the relationships between our dichotomies and the keeping or loss of partial freedom. Thus we close the last defence but one against the finding in Section 9 that the process of selection for parole was almost purely bureaucratic; the defence that we had consciously or unconsciously chosen an irrelevant categorization of the contents of the data on the Parole Board files and then used this to accuse them of ignoring the content of these items. But now we have shown our categorization of content to be relevant, and only one defence is left. As we said in Section 9 "all the analyses of the present section, however interesting, would be in one important sense overridden if it could be shown that in some sense the "right" decisions were made, by whatever process, and the "parolable" applicants were granted parole and the "unparolable" refused". This question will be one of our main concerns for the rest of this book, but more especially in Section 15.

## Section 14

### Predicting the keeping or loss of partial freedom

The first step in predicting the loss of the partial freedom conferred by parole was to define this criterion exactly. How this was done has already been described in Section 12, where we also discussed why we chose this particular criterion. After this, the next stage was to produce four prediction equations, following exactly the procedures already followed in predicting the granting or refusal of parole (Section 11).

It may be asked why we still need four prediction equations. Our need both for linear (regression) and for classificatory (predictive attribute analysis) equations is clear enough (or if it isn't, then see Sections 10 and 11); but why should we wish once again to develop equations both including and excluding the dichotomies in batch C, those whose relationship to the granting or refusing of parole was thought to be partly but not wholly artifactual? Surely, unless we think that these dichotomies are also in a semi-artifactual relationship to the losing of the partial freedom conferred by parole\*, we can without any hesitation include them in our predictions of this criterion. But we shall need comparability between our results in this section and those in Section 11, and so, for each of the earlier equations, we derive a new one from an analysis by the same techniques from the same data (except for the change of criterion). In fact, neither in the new predictive attribute analysis nor in the new regressions did any of the previously questionable data from batch C enter the final equations, so that this problem solved itself very happily by default.

The longer a person is on parole, the longer he is at risk of losing this partial freedom, and this increased risk had to be allowed for in our analysis.

\*Which may or may not be the case; but this is not a study of parole supervision and we will rest content here with the simpler assumption that there is no such semi-artifactual relationship.



But the exact form of the relationship between greater length of parole and increased risk was not known, and could not be observed from our data since length of parole is itself associated with other factors. So we divided our sample into those with follow-up periods of up to 295 days (43 out of the 143 members of the study sample) and those with longer periods. This dividing-point was chosen after a rough inspection of several such divisions. We considered dividing the sample into three roughly equal groups with short, medium and long follow-up periods, but there appeared to be little difference between the medium-period and long-period groups; of course, our longest follow-up period was limited to three years. This new dichotomy (length of parole was included in all the analyses made in producing our two new prediction equations. In the stepwise regression analysis the new dichotomy was forced into the final equation whether it would have "earned its keep" or not (in fact it always did) but it is not desirable to force variables into Predictive Attribute Analysis and so it was merely included in the set of available dichotomies, without having any special priority attached to it.

The results of these analyses are given below in equation 3 and Table 109. As in Section 11, we see that the Predictive Attribute Analysis in Table 109 is extremely easy to understand, but for some readers Table 110 may make the results of the regression equation (equation 3) easier to picture. This Table should be treated with all the cautions that we attached to Tables 102a and b in Section 11. The regression analysis at first rapid glance appears to have "behaved itself" and to have produced a linear equation such as we might expect to see. But the Predictive Attribute Analysis has produced the most primitive prediction possible, short of producing no prediction at all. The sample is simply divided into two groups, those answering "yes" and "no" to dichotomy 41, "not listed as having previously been in a penal institution."

This division into two groups on the basis of a single dichotomy produces a value of  $\phi$  in the replication sample of 0.324, which is about the level of predictive power commonly found in equations predicting reconviction or similar criteria. The interesting point is that this value of  $\phi$  is greater than the value of .261 produced by the regression equation. It is frequently found in criminological prediction that a single item, usually of the "has it happened before" type, predicts as well as or better than an attempt to use a greater range of information. Simon (1971) lists her own study and those of Power, Shoenberg and Alderson (1967), Duncan, Ohlin, Reiss and Stanton (1953) and Glaser and Hangren (1958) as examples of this.

So how do we interpret the fact that one more study has proved able by statistical survey of a vast mass of data to account for just 10 percent of the variance in the question "will this man lose his partial freedom or not?" leaving 90 percent of this variance not accounted for, and that almost the only relevant information about the parolee that relates to this question is our

answer to the question "has he already lost his freedom before the most recent occasion?"

Anyone who has read this book so far can foresee my reply. Loss of freedom is a thing that *happens to* a criminalized man, not something *caused by* him (or not necessarily so, and certainly nowhere near wholly so). So there is no reason to be surprised if in fairly similar nations with fairly similar contra-crime industries study after study shows that the information about the man's past and present recorded by collaborators with the man-caging professions is only one-tenth of the relevant information. (To a first approximation variance serves as a measure of information.) It could have been some other proportion, even a very high proportion indeed (e.g. in a country that repeatedly criminalized Jews) but it happens to be about ten percent, which is as credible a proportion as any other.

Simon (1971) has a very good discussion of this problem, unfortunately too long to quote here, but necessary to an understanding of the subject. The only point on which I would differ from her is that she sometimes writes as though there were a phenomenon called "real criminality", which by unspoken definition is a quality possessed in greater or lesser degree by "real criminals", and of which events like convictions are a crude and unreliable measure. However, this shadowy "real world" does not seem to interfere much with the observable, superficial "unreal" world of cops who arrest people and judges who call them robbers. She concludes reasonably enough that the information logically and practically required to predict criteria like reconviction or loss of partial liberty is not merely information about the inmate's past or present; my only disagreement is when she writes as though the hypothesized personal attribute of "real criminality" is the central concern of the criminologist and that such crude real-life concerns as what other people do to him are secondary issues.

Now Turk (1969) has exploded the myth that criminality is in any important sense a question of the behaviour of a man called a criminal; and Kuhn (1962) argues that a science cannot exist until the generality of its practitioners have reached some working agreement on what *not* to study. Without claiming that there could ever be a distinct science called "criminology" we might extend Kuhn's argument and suggest that the explanations offered by criminologists of the phenomena that they study will remain in an unscientific confusion until a relevant choice of what not to study has gained a reasonable number of adherents. Therefore, although it is legitimate to study the behaviour of any man, and, if we so wish, to study any determinants of that behaviour that may inhere in him, as well as those external to him, it is almost certainly necessary that we abandon the whole enterprise of seeking qualities supposed to inhere in men called criminals and to differ from those inhering in another group not so called, and then trying to explain criminal status in terms of specific inhering qualities.

If we persist in this habit we are likely to continue as we seem to have done throughout civilized history, with neither scholars who understand criminality nor ruling groups who know what they are using it for.

It remains to consider certain reasons sometimes advanced for weak predictions where someone has expected a more powerful result. In effect, a prediction equation is a relationship between a criterion and a set of predictive data. We have just resisted criticism of our choice of criterion, which we have justified at various points of this book. This leaves the predictive data or the relationship to take the blame. We have just reiterated our view that the predictive data are irrelevant to our criterion; but other people not sharing our basic orientation have criticised this sort of data on other grounds which we cannot accept.

Some of these grounds are frankly ridiculous, and most of these have been patiently and politely, too politely, mopped up by Meehl (1954) in his excellent little book. But two features remain which distinguish our sort of data from certain other sorts of information sometimes used to guess the future.

The first of these is that the bulk of the data in this sort of study is sometimes said to be "objective" rather than "clinical", "intuitive" or "subjective", using or abusing all four of these terms in a regrettable but common way. Now Meehl points out that when one uses relatively objective forms of data-analysis which require the data to be tidily recorded, it is easy to suppose that these tidily recorded data are themselves objective. Anyone who looks at Table 1, the list of data and data-sources for this study, will soon disabuse himself of the idea that we are excluding relevant subjective judgments by experts because of a bias towards imitating the physical measurements that form the basis of the natural sciences. What we find about certain objective and subjective data and their relationship to the granting or refusing of parole and to the loss of the partial freedom that parole confers has been a major concern throughout much of the book, and we cannot recapitulate it here; but this study can give no comfort to those people who argue that the failure of criminologists to predict things like reconvictions is a failure to make proper use of so-called clinical insights. Even Sawyer's (1966) claim that such clinical "evidence" improves predictions based on objective data seems to rest on a confusion between having more types of data and simply having more data. On an unreplicated study sample, the more data the better; on a replication sample this is often but not always so. (Thus in the present study the prediction shown in Table 109 is based on one predictor variable and gave  $\phi = .383$  on the study sample and  $\phi = .324$  on the replication sample. Equation 3, using the same predictor variable and six others, gives the better prediction represented by  $r = .651$  on the study sample, but has fallen behind the one-variable equation to give merely  $r = .261$  on the replication sample.)

The other criticism often levelled against criminological prediction is that the predictor variables do not include information on the treatments that have been applied to the inmates with the aim of affecting their future behaviour. Now in the present study we included any information that met the conditions listed in Section 3; if our data contain very little information on "treatments" this is not because of indifference or bias by the researchers, but because very little such information is recorded on the files in a systematizable form. We do not know whether this is because the Parole Board are not interested in such things or whether there are very few of such things to record about our sample.

At the more general level, the reason why most prediction studies make no use of "treatment variables" is that the overall finding of competent research, whether in the field of crime or of mental illness, is that the things that we do to change behaviours seen as problematic do not in general produce a greater "improvement" in these behaviours than would occur if we did something different or if we did nothing at all. This is of course a gross oversimplification (see the literature listed by Truax and Carkhuff (1967)\* for mental illness and that listed by Turk (1969) and by Robison (undated) for crime) but the necessary qualifications to this overall approximation still do not amount to a finding that prediction can be improved by including treatment variables.\*\*

In any case, we have already argued that criminality is not a problem of behaviour and that the things done to criminalized people, even if at some times and places these things happen to be called treatments, do not in fact exist for the purpose of changing behaviour, nor for any other objective benefit to society, but even a reader who rejects this point of view should consider the evidence that *repeated* criminality is not a problem of *persistent* behaviour. We turn once more to the ever-valuable Turk (1969) for a list and discussion of the evidence that "variability rather than homogeneity is characteristic of the recorded careers of offenders . . ."; he concludes that "If the records of most persons who have at times been assigned the status of criminal show that the act attributed to them varied, then one may doubt that there is a detectable pattern in the officially recognized behaviour of most "sometime" criminals. If there is no "career line" in the records of most of

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\*Truax and Carkhuff would not agree with my previous sentence; they claim that some therapists cure people and others make them worse, and that these two effects mask each other for groups but not for individuals. Even if this were true (and their interpretation of their evidence does not convince me) it would not affect my main line of argument here. But Truax and Carkhuff, early in their book, give a very useful list of the evidence of lack of impact of "treatment" on problematic behaviours.

\*\*There have been occasional contrary findings. The most famous is that of Mannheim and Wilkins (1955) but this seems to be an exceptional case. Grygier's (1966) finding cannot be relied on as it is not based on a replicated or otherwise validated analysis.

these people, then there is little reason to expect theories of behaviour to account for records of criminality".\*

In these circumstances it is hardly surprising that "treatment" of criminals does not "work" and the question arises why after trying it intermittently for thousands of years we still expect it to work. At the level of the individual judge or magistrate this may be simple ignorance. In almost no part of the world is a judge selected, his performance evaluated, nor his level of reward or esteem determined by any measure of his ability to produce any results, since contra-crime is not a result-seeking activity. Thus ignorance of the effects of his actions is no disability. But such an excuse is not open to those politicians and their hangers-on who still crow about their success, or whine about society's failure, to "beat crime" by methods that were never designed to. Ministers and governmental advisers who announce further efforts (whether "firm" or "progressive") to "fight crime" are either cheating or else taking refuge in an avoidable ignorance that appears to be perilously close to active bad faith.

To recapitulate briefly before continuing, we have found that the best prediction of keeping or loss of partial freedom is obtained from one single dichotomy (No. 41, not listed as having previously been in a penal institution) giving a value of  $\phi$  of .324 on the replication sample. By comparison with other similar studies this is quite good, but in fact, rather like these other studies it only accounts for 10 percent of the variance. So we considered prediction studies in general to see why this should be so. We regarded a prediction analysis as a relation between a criterion variable and a set of predictor variables. We rejected certain criticisms of the criterion variable, but claimed that predictor variables based mainly on data about the convicted man were basically somewhat irrelevant. However, we rejected other criticisms of these predictor variables, and drew certain conclusions from this rejection.

However, we still have to consider whether weakness of prediction may not arise from faults in the choice of relationship between the criterion and predictor variables. This choice falls into two parts: the decision to use systematic, mathematical techniques rather than intuitive clinical ones; and, given this decision, the choice of which particular mathematical models to use.

First let us consider the possibility that a non-mathematical relationship between the predictors and the criterion might work better than a

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\*Obviously there is an epistemic problem here. If we see "the carrying out of any act later judged to be criminal" as a homogenous behaviour-category, then of course criminal careers will be homogenous. On the other hand, any two not-quite-identical acts differ, and in this sense the most repetitive obsessive psychotic is showing heterogeneity. The practical force of this objection to Turk's statement is greatly diminished when we see that in general, in the studies that he quotes, one man's subsequently criminalized behaviour on a number of occasions shows nearly as much variety as do a number of subsequently criminalized acts ascribed to a number of different men. There is of course the problem of selective perception; we may be readier to suspect, and therefore more likely to observe, a behaviour that conforms to our past expectations of a particular man than an act which represents a new departure for him.

mathematical one; that the way to predict reconviction, etc. is to consider the data and then make a "clinical" prognosis.

The answer to this position is simply that it is known to be false. Mathematical prediction of this type of criterion may be weak, but non-mathematical prediction is even weaker. Lists and discussions of the evidence are to be found in Meehl (1954) and in Sawyer (1966) but no one has listed the special pleadings and smoke-screens that represent the case for non-mathematical (or combined mathematical plus clinical) prediction and which in total amount simply to anti-intellectualism in defence of an entrenched position masquerading as science. It is the imperturbable politeness of Meehl that hides the banality of many of the arguments he so patiently refutes.

But even granted that criminology's failure to predict reconviction, etc. is not due to the use of mathematical techniques, we have to consider whether the two particular mathematical models that we decided to use, described in detail in Section 11 and Appendix A, were the best available or whether we could have obtained a better prediction by using a different model. Simon (1971) used a large number of different models for comparative purposes, and surveyed not only these but some of those used by other workers. In all she considered twelve models and concluded that ". . . in practice all of them work about equally well . . . In spite of the different theoretical advantages and disadvantages of each type, it appears that when put to the test on a validation sample they are likely to come up with prediction instruments of roughly equal power (provided that the construction sample has been large enough to avoid gross instability). Moreover, this power is low. Several investigators, including the present writer, had the experience of finding that a single variable gave as much, or nearly as much, power as a sophisticated combination of several . . .; more generally, it seems that, while the most predictive individual variables are likely to have correlations with the criterion of .2 to .3, there is no method of analysis which will succeed in combining them into an instrument with a correlation much above .4. While the methods cited in this discussion are not an exhaustive list, they cover a wide range (points scores, multiple regression, hierarchical configurations, Lance's centroid sorting, Floyd's pattern recognition technique), and it is the opinion of the present writer that there is probably little to be gained by searching for others in the hope of combining low-power predictors into something of much greater power".

The two models used in the present study were chosen more on logical grounds than in any hope of obtaining notably more powerful predictions than by other methods. But Simon's study seems to confirm the position that we have taken, that failure to predict reconviction or similar events is not due to technical inadequacies in the methods used but to the implicit self-deception in regarding criminalization as something mainly caused by the criminalized man.

## Section 15

### The relationship between granting or refusing parole and keeping or losing this partial freedom. Confirmation of the findings of earlier sections

This section will be short, partly because what we now have to say is fairly simple, partly because at this late stage in the book there are not too many new ideas needing explanation; but however short it may be, it contains what are in a sense the central findings of the whole study, on which many of our arguments and many of our interpretations of our other findings finally depend. Up to now we have examined the relationship between the bulk of our data and the granting or refusing of parole. Separately from this we have examined, in the case of those members of our sample to whom parole was granted, the relationship between this same bulk of data and the keeping or the loss of the partial freedom that parole confers. The two parts of the study were made strictly comparable, and certain comparisons of our two sets of findings, certain similarities, contrasts and relationships, sprang to mind. But we have made up till now no formal investigation of the "triangular" relationship between the bulk of our data, the granting or refusing of parole, and the keeping or loss of partial freedom.

There are certain inescapable limitations to the study of this triangle. We can only discuss the keeping or loss of partial freedom in the case of those who possessed it, since you can neither keep nor lose what you do not have. Thus, for the 263 men in our study sample and 279 in the replication sample who were refused parole, we cannot directly observe whether they kept or lost partial freedom, since they did neither. Whether, after their ultimate release from prison, they kept or lost such freedom as they then enjoyed is a different question but not one on which the Parole Board records systematic information on its files, since they are under no pressure to evaluate the effects of

refusing parole, which was at that time their usual ultimate response to an application. Because of this lack of information we could not ourselves study the fate of these men without seeking a large amount of data elsewhere, which would have enlarged the project beyond what was practical. However, one part of our present tasks was to consider how many of these men refused parole would have kept or lost their partial freedom if it had been granted them, and one of the things that will concern us in the rest of this section is the degree of certainty that can be attached to such apparently hypothetical estimates.

By a similar logic one can see that we cannot compare the paroling or non-paroling of the two groups of men who subsequently kept or lost their partial freedom, since of necessity these were all men who had received parole. However, for the paroled men we can calculate each man's probability of receiving parole, estimated without reference to our knowledge that he did really receive it, and we can then compare this probability with whether he later kept or lost his partial freedom. If this probability of being paroled represents not merely a statistical summary of the facts, but some variable representing his "parolability as seen by the Parole Board", then a man with an estimated probability of being paroled of .8 who then receives parole can be termed "more parolable" (in terms of Parole Board customary practice) than a man with a probability of .2, even if the latter also receives parole. In this case we face the possibility (and the problem) of relating a man's chance of receiving parole to the question of whether he later keeps or loses his partial freedom, and of attaching a meaning to the relationship so found.\*

What relationship should we expect to find between predicted probability of receiving parole and subsequent observed keeping or loss of partial freedom, or between the actual granting or refusing of parole and the predicted probability of losing this partial freedom? We have to return to the ideas of success and failure discussed in Section 12. Now nothing in this book is to be taken to imply that the act of letting a man out of prison is ever a failure. But we have seen that the Parole Board will almost certainly wish to release those inmates who are least likely to lose their partial freedom in preference to those who would rapidly lose it. In these terms, if the Parole Board grants parole to a man who soon loses his partial freedom while at the same time they refuse it to a man who would have kept his partial freedom, we can see this as a failure (of their selection activity only); on the other hand, if the men granted parole are less likely to lose their partial freedom than would have been those refused parole, this difference will be a measure of the success of the selection activity; but this does not imply that refusing parole to an applicant is ever a success.

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\*If we consider 5 men with parole probabilities of .8, it is likely that four of them will be paroled and one refused. We cannot say which or why, so it is reasonable to regard them all as more paroleable than 5 men with parole probabilities of .2, among whom it is likely that only one will be paroled and four refused.



The difficulty is to keep clear the distinction between "success in doing something good" and "success in achieving something attempted" where the thing attempted is not necessarily entirely good. In this study we are concerned with a third concept, "success in achieving a goal, not necessarily good, but nevertheless implied by the activity under review if we suppose that this has a goal at all."

Table 111 sets out the correlations (measured by  $r$  and  $\phi$  as appropriate) between the various events and probabilities that concern us here. In this table we have arranged the directions of the variables so that if two of these events or probabilities are perfectly and linearly related in the so-called "successful" direction (i.e. where people with a higher chance of gaining it are also those with a greater chance of keeping it),  $r$  or  $\phi$  will equal 1. If they were perfectly and linearly related in the so-called "unsuccessful" direction (i.e. if the worse parole risks were those with the greatest chance of receiving it),  $r$  or  $\phi$  would equal minus 1.

The interpretation of this table is relatively simple. From the row numbered six we see that the single question "Has this man been in a penal institution before his present sentence" is for the paroled man a weak but not negligible predictor of whether he will keep or lose the partial freedom that parole confers; and that it is for the whole sample a slightly weaker predictor of whether parole will or will not be granted. From line five we see that the attempt to introduce further variables into the prediction of keeping or loss of partial freedom weakened the relationship that it tried to improve, and also weakened the relationship with whether parole would or would not be granted.

So far it would appear that we have evidence that is compatible with (but not proof of) the possibility that parole is granted to those who are most likely to keep their partial freedom. But when we look at lines one to four in Table 111 the picture changes drastically. For we see very simply that we can very greatly improve the prediction of who will be granted or refused parole; and that the more we improve this prediction, the less it predicts whether a paroled man will keep or lose his partial freedom, until our best predictor of the granting or refusing of parole is in an almost zero relationship with whether a paroled man keeps his partial freedom or not. If we turn to the original equations the reason becomes clear. Whether there have been past convictions or incarcerations or not is the only element entering simultaneously into the prediction both of granting or refusing parole and of keeping or losing partial freedom. The more we improve the prediction of the granting or refusing of parole the more we move away from this sort of item and from any other that might predict the keeping or loss of partial freedom.

Now this result has one feature needing explanation. Earlier in this section we have distinguished sharply between the paroled man's keeping or

loss of partial freedom and the unparoled released prisoner's keeping or loss of whatever freedom he enjoys after his imprisonment. But in the absence of other evidence, the loss of freedom or partial freedom in the two cases would appear to be a similar process. There is no immediate reason to suppose that the recriminalization of a parolee by way of arrest, trial, etc. is very different from that of an ex-prisoner.

Yet it is well known that the recriminalization rate of released non-paroled prisoners is much higher than that of paroled men. A direct comparison of Street's figures for parole and McLeod's for penitentiaries (Standing Committee on Justice and Legal Affairs, 1968) would be rather misleading, but unpublished analyses by the Dominion Bureau of Statistics also reveal this difference, though at a lower level than Street's and McLeod's figures appear to imply. We have to ask, does this mean that parole is granted to people with a lower chance of recriminalization and that there must be something wrong with the analysis we have just given?

We have to remember that the two groups whose predicted recriminalization we compare in the present study are both groups who have *applied* for parole. At that time about 40 percent of men sent to penitentiary did not apply for parole. In very rough general terms it seems that there is an actual disadvantage in being paroled if you are going to lose the parole (see Section 6). Thus in view of the findings in Table 111 it seems very likely that the difference in recriminalization is between parole applicants and non-applicants, not between applicants who were granted parole and those who were refused it. The point of view developed in this book, and the findings of this section, are perfectly compatible with the idea that the prediction of recriminalization is better carried out by those in danger of it and with most to lose by it than by bureaucrats who have collected a heap of irrelevant data, especially as receiving and then losing parole not only submits you to the humiliations of day-to-day life under supervision, but also lengthens your total stay in prison.

But this is hypothesis. In the present study we have no data on non-applicants. If our hypothesis turns out to be true, well and good. If it turns out to be false, there may be problems in reconciling this with the analysis in this section. Even so, such problems would only arise if we held to the assumption that the recriminalization of a given man is the same process with the same causes whether or not he is on parole at the time; and this in turn is an assumption in an area where we know nothing. So we see no necessary conflict between our findings and the often-quoted but mis-named "success of parole" figures comparing parolees and released non-parolees.

There is one point here that needs to be cleared up. It can be argued that our prediction of the granting of parole, though good, still leaves half the variance unaccounted for, and that the prediction of loss of partial freedom is

weak enough to leave 90 percent of the variance unexplained. It may be asked whether it is not possible that the Parole Board in selecting for Parole are using information not used by us which predicts loss of partial freedom better than we can.

The answer is that it is logically possible but for technical reasons it seems so unlikely that we almost certainly need not let it cause any doubts. The logic of a similar problem is well discussed by Mannheim and Wilkins (1955), though we must note that their finding of a "treatment effect" is divergent from most findings and is still causing controversy. But they say in effect that we must consider what would be required of this hypothesized extra information alleged to be used by the Parole Board. Firstly it would have to be an efficient predictor of future criminalization which no known information save past criminalization is. Secondly, its worth would have to be clearly and correctly appreciated by the Parole Board, available in nearly every case, and yet not recorded on their own files. Thirdly, these unlikely data would have to be only weakly related to the vast mass of other data that is on the file, or else we should find that we could predict as well without it. No such miracle-data, invisible, unrelated to anything else, but effective, have ever been reasonably suggested. So our finding about the lack of connection between the probabilities of granting and of keeping parole still stands, with all its implications.

With this point cleared up, we see no need to discuss the findings of this section very much. If we had written this book as a guide to parole selection we could devote pages to the consequences of granting parole to certain groups who were in fact refused it; the reader concerned with this may easily undertake such studies from Tables 102a, 102b, 110, 112, 113, and 114, which give the number of people granted or refused parole, and the numbers keeping or losing their partial freedom, for various values of the three regression equations. For the Predicative Attribute Analysis the same games may be played with Tables 101a, 101b, 109, 115, 116 and 117. But this is not the point. The point is that yet one more analysis has failed to reveal any orientation towards, or achievement of, any other goal than purely bureaucratic ones. The Parole Service would appear to subscribe to a myth that shapes their selection activities in a way that may or may not be comfortable for them but which shows no relevance either to their stated goals or to any objective benefit to prisoners or to society.

## Section 16

### Conclusions and inconclusions

It is not the main purpose of this section to repeat the findings scattered over the rest of the book, but for clarity's sake we begin by briefly listing a few of them. All of these findings are seriously distorted by being listed in this drastically abbreviated form, and reference should always be made to the section indicated.

In Section 6 we estimate that the direct effect of parole on male penitentiary inmates not serving life or indeterminate sentence is a reduction of the total man-days of imprisonment of that set of people by about 6 percent, or three weeks per year on average.

In Sections 7, 8, and 9 we discover a number of facts about our sample, and about the information collected or not collected and used or not used, by the Parole Board in its decisions to grant or refuse parole.

In Section 6 we discover that parole is not freedom but an uncertain, highly conditional and in some ways degrading "permission to be slightly free", and that the Parole Board frequently makes men serve the same sentence twice, and even more than that.

In Section 9 we discover that in using items of information to decide whether to grant or refuse parole, the Parole Board is more influenced by the source of an item than by the topic to which the item refers; though for various reasons this point cannot be regarded as adequately established until a number of points have been cleared up in a number of sections up to and including Section 15.

In Section 11 we find that the granting or refusing of parole can be predicted with a correlation (on a replication sample) of .710. In other words,

half the variance in the reply to the question "will this applicant be granted parole or not?" can be explained in terms of information on the file and our techniques of analyzing it. It is seen that this decision leans very heavily indeed on the custodial authority's recommendation.

In Section 13 we find many things about the relationship between individual items of information and the loss or keeping of the partial freedom conferred by parole. From these we proceed to an analysis (which we do not complete until Section 15) of the relevance, or much more frequently the irrelevance, of the information mainly used to decide whether to grant or refuse parole.

In Section 14 we find that the keeping or losing of the partial freedom conferred by parole is predictable with a correlation (on a replication sample) of .324. In other words, only 10 percent of the variance in the reply to the question "Will this man keep or lose his partial freedom?" can be explained by the information available to us and our techniques of analyzing it. This level of 10 percent of the variance is close to that found in many studies of similar situations, and we conclude that since loss of partial freedom, reconviction and other similar phenomena are all cases of something being *done to* the individual concerned rather than being necessarily or wholly *caused by* him, it is not surprising if some low figure such as 10 percent represents the proportion of the information on whether these things will or will not be done to him that can be extracted from information about his own past and present.

This is consistent with our finding in Section 14, also shared with many other workers, that this 10 percent is all contained in the question "Has he lost his freedom before the present occasion?" Other information available to us adds nothing to the predictive value of this one item. In other words the best, practically the only, predictor of whether they will "do it to him" in the future based on personal data about himself is if it has been "done to him" in the past.

In Section 15 we pull together our other findings by observing that previous loss of freedom not only predicts, although weakly, future loss of partial freedom, but also predicts, although weakly, a refusal to grant parole. However, our very much stronger predictors of the granting or refusing of parole bear no relation to subsequent loss or keeping of partial freedom. This leads us to the conclusion foreshadowed in Section 13 that the Parole Board's criteria for granting or refusing parole are irrelevant to the aims implied by such an activity, if it has aims at all other than purely bureaucratic ones.

These are the main findings arising from direct analysis of the data collected for this study. Later we shall deal with other findings of a more general sort, based on wider considerations.

The findings that we have just listed raise in my mind six main questions, which group themselves conveniently in three pairs.

Firstly we remember that all these findings are based on parole decisions taken in one country within a three year period. It is natural to ask firstly whether these findings have been true in Canada at other times, in particular whether they are true today (though tomorrow we shall see nothing special in today) and secondly whether they are true for other nations and other jurisdictions.

Furthermore, we have throughout this book adopted a definite point of view, not only towards parole but towards the whole of contra-crime and also how to study it. So our third question is whether the things we assert about contra-crime (the "other findings of a more general sort, based on wider considerations" that we have just mentioned) are true or not; and our fourth question is whether our viewpoint on how to study crime and contra-crime is worthwhile and profitable or not.

Next, we may guess with reasonable confidence that very few readers will find the state of affairs described in this book satisfactory, and no-one will find it entirely satisfactory. So our fifth question is what can researchers be doing in face of this situation; and our sixth question will be what can we, researchers or not, be doing about it in our "role" of human being, of which the role of researcher is a partial expression.

The answer to the first question, whether our findings apply to Canadian parole at other times, and particularly today, must be that strictly we do not know, but that probably they do so apply. In estimating whether something observed at one time is true at another, we have to consider the nature of the thing observed. If we meet a man with one leg, he will not have two the next time we meet him. On the other hand, if he has a cold it may well be gone the next time we meet. The activities described in the present book seem to me to suffer from a state more like one-leggedness than like some temporary disorder such as a cold. Parole is a part of contra-crime, controlled by the same parts of government for the same alleged reasons as the rest of contra-crime. We have argued that the two universal features of contra-crime throughout world history have been the official selection of certain people and the official practice of nastiness against them, and that this is done without a consistent philosophy, not even an implied one, to give it goals, though there is no shortage of philosophizing to serve as rationalizations of these activities. Now if this is true it would lead to states of affairs very like those found in the present study, and they would be extremely stable, although (and this would depend on the spirit of the age) they might well be half-hidden behind a ceaseless flurry of quasi-reforms. But in this present case we must be more specific. Since the date of this study the Parole Board *has* in fact introduced

large changes, which are either real reforms or the "ceaseless flurry of quasi-reforms" we have just mentioned.

Over the years the number of prisoners paroled has greatly risen, and this is welcome, though it is doubtful whether the man-days in prison saved has risen so much, and it will probably fall sharply, even below zero, in the future when the mandatory parole provisions of the amended Parole Act begin to apply on a large scale. Many of the other changes centre around these amendments to the Parole Act (see Appendix E) designed to make it easier for the Parole Board to re-imprison people without fuss and without appeal. The Act also imposes compulsory parole on people who under the old system would have been treated as having finished their sentence. Loss of this parole results in re-imprisonment for an offence for which (apart from legal quibbles about remission) the full sentence has already been served. Thus parole reveals itself even more fully as a part of the official nastiness called contra-crime.\*

The other main change is that the Parole Board is enlarged, and parole decisions are taken, not by the whole Board, but by travelling teams of two. This means that decisions are made faster (we do not know if *final* decisions are made faster, or only the "decision not to decide" mentioned in Section 5) and are announced in person to the prisoner instead of in writing. Enough is known about selection of people to meet a criterion to know that if the Parole Board ever had or imagined it had a criterion for parole selection, it abandoned all hope of relating its decisions to this criterion when it adopted this new selection process (see for example Meehl (1954) and Sawyer (1966)). The process is sometimes said to have resulted in a large increase in the number of men paroled, but there had been a steady trend towards granting and withdrawing more paroles for a long time and it does not appear that this was affected by the new arrangements. One member of the Parole Board claimed to me that this change humanized the situation. I believe that for him it did; he resigned from the Parole Board. But in general, face-to-face contact accompanied by a non-symmetrical balance of power can dehumanize as easily as it can humanize. Judges easily look their victims in the face. To take this point further, we observe as a historical fact that one of the main objections to Christianity has been the apparent absurdity of the concept of the condemning judge who loves the accused. The usual defences of Christianity have been either to play down one or the other aspect or to assert both as a paradox beyond human power of understanding. But as we point out in Section 5, our present age does not balk at paradox or self-contradiction in rationalizing about contra-crime.

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\*The revolt that destroyed most of Kingston Penitentiary on April 14th, 1971 (See footnote 2, p. 32) was in part against these new parole provisions.

Our second question, whether the findings of this study are true for other nations and jurisdictions, will receive a somewhat similar answer. We have plenty of reason to suppose that such findings are true elsewhere, and no reason to suppose that they are false. But this time we have rather more facts to support us. Enormous numbers of studies exist on every aspect of contra-crime, studies undertaken often by researchers thoroughly committed to the so-called treatments or other processes that they were studying, often even undertaken by members of the institutions practising such treatments; sometimes studies undertaken by researchers more critical of what they studied. When we have eliminated those studies and those interpretations that are by normal research standards incompetent, we are left in general with findings that parallel, or at least are compatible with, our own. Read any standard text in this way (good examples are Mannheim (1965) for completeness, Turk (1969) for conciseness, or Nigel Walker (1965) for a position halfway between the two) and little will be found to contradict this view. A good documentation of the objective uselessness of that branch of contra-crime called corrections in a part of the world with one of the best reputations for success is to be found in the polite, patient but devastating report of Robison (undated, *circa* 1970). Examples of how hypothetical is nearly all our thinking about deterrence, and of how little of it points, either at the logical or the empirical level, to any support for the objective usefulness of contra-crime, are found in Zimring (1971); although it is not clear how far Zimring would accept this interpretation, it seems an inescapable conclusion from his text.

The third question is whether the things we assert about contra-crime in general, not on a basis of the facts collected in the present study, but as a part of our general viewpoint, are shown to be true or false by our more specific findings. Now our data tell us little about that part of contra-crime that consists of officially selecting people to be subject to official actions. Tables 3-92 may perhaps explode a few myths *en passant* and Section 14 shows to some extent that people who have been so selected and have been deprived of their freedom more than once in the past are more likely to be so treated again than those to whom this has only happened once so far. But this is all. True, there is a selection process by contra-crime operators as one of the main phenomena of our study, but it is a selection process in too ambiguous or even paradoxical a relation to the main contra-criminal selection activities to give us much guidance as to these (see Section 9).

But when we come to our view of the official actions to which these officially chosen people called criminals are subjected, our data here bear much more relevance to our claim that the main essentials of these contra-criminal actions is that they are nasty and without any objective benefit to society. For we find that the Parole Board's actions are predictable and irrelevant to any such objective benefit; in fact that the Parole Board does not



even attempt to collect the information necessary to estimate the objective consequences of their actions or inactions.

Of course, no evidence ever proves conclusively the value of a particular viewpoint, and no viewpoint contains final truth, in the sense that all viewpoints are ultimately replaced by new ones that lead to better explanations of the phenomena of interest to the workers of that particular epoch. For a fuller account of this oversimplified statement see Kuhn (1962). Thus even if the viewpoint advanced here comes to be generally accepted, it will ultimately be replaced by others which offer more hope of profit to the workers of that day. But I would urge that our commencing with contra-crime and seeing it as an objectively goal-less activity is vastly more useful than those older approaches that force themselves to seek for behaviours in the men selected to be called criminals, behaviours that are then assumed to be basic causes of a rational, contra-criminal response, and thus to be the rightful starting points of criminology.

Of course, the claim that in the present study we have found much to support our viewpoint and little to cause us to reject it provokes the very obvious retort that one sees what one is looking for and not its contrary. Goethe says "We see only what we know." It is often pointed out that what will be found in the study of any human phenomenon is practically determined when we have decided what to look for.

I have utilized such techniques for reducing the unwanted aspect of this of as were available to me. The original lists of the contents of a National Parole Board file, were made largely by a student employed one summer by the Centre of Criminology, who had not previously met me and to whom I tried to avoid communicating my orientation. Changes in this list were made on grounds of feasibility, never consciously on theoretical grounds, though of course there is inevitably a large amount of personal choice and professional myth that plays its part in deciding how to change a list of contents into a coding schedule. We brought in no data from other sources, and the techniques of analysis used were all relatively objective, pre-chosen ones that had been used in other studies. Finally, in this report we have kept as close as we could in such a large study to the scientific tradition of full reporting, not merely to meet as far as possible normal scientific criteria but also to give others the chance to observe and to counterbalance the inevitable subjective limitations inherent in all such work. So the support offered to our views on contra-crime is, I think, not to be totally discounted as merely one more self-fulfilling prophecy.

The answer to the fourth question, whether the viewpoint we adopt leads to more worthwhile research than other viewpoints, is better provided by the reader than by me. He will judge this partly on his evaluation of the present book, and his decision whether such work would be undertaken and such

analysis of the data and interpretations of the analysis would be likely to be made under those older approaches which lead so often to the fruitless search for causes of crime and effects of treatment. But also he will judge which of the studies listed in say, the first one and a half sections of Turk (1969) share to some extent this approach, and whether they are of greater value than more traditional ones. (I do not disagree with the later parts of Turk's book; but he turns his attention to other interest less close to mine.)

The fifth and sixth questions may be taken together, since they both amount in different ways to "what should we do about it?" One thing a person distressed by the findings of the present study and the many studies cited in these pages can do is to reject such findings. Another reaction may be to escape the situation by describing it as complicated and suspending all judgment until that never-to-come day when we shall have done enough research. Now the situation *is* complicated and we *do* need more research, but some general features of this complicated situation are clear enough to make certain judgments possible now.

A third reaction is to accept the specific findings and call for a clean-up, possibly involving changes of personnel, to change the state of affairs. This is only a legitimate action for people who do not realize that the findings in this book are typical and inherent in all contra-crime organizations; taking one to pieces and rebuilding it will achieve nothing new since the causes of the undesirable features of the old will still be present in the new.

Some people go further, and wish not only to re-make but to re-design the contra-crime system according to a new plan; or, if they are researchers, to direct their studies to support groups trying to do this. There are two points to consider here. Firstly the relationship between changes sought and changes achieved is often extremely obscure. A reform brought about in response to pressure to make a law less "punitive" may equally well result in fewer, the same or more people being subject to less, the same or more nastiness, and all this as the apparent result of fairly small-seeming semi-accidents beyond the wit of any man or reformist group to foresee or prevent. See for example the Canadian and English examples quoted by Gigeroff (1968). But more generally we must realize that contra-crime reform and reformist pressures have always, except perhaps for the Medes and Persians, been an integral part of contra-crime, changing none of its essential features but securing its respectability and our assent to it by making it appear capable of adapting in face of varying needs. But it doesn't need to adapt any more than the ant needs to evolve. In spite of its objective uselessness (contra-crime's, not the ant's) it has achieved stability in essentials.

At this point in the discussion many readers may become somewhat frustrated and irritated as I seem to block off one after another possible response to the inherent nastiness of contra-crime. It has been asked if this

rejection of political attempts at reform leaves anything to advocate except anarchism, and at this point the reader will either turn away in disgust or ask seriously whether anarchism is in fact a solution.

I do not think that it is. If over the whole literate world for six thousand years there have been governments practising contra-crime, such a phenomenon has reasons for existing. This is not denied but emphasized by its objective uselessness and by the variety of philosophical rationalizations used to justify it. When I speak of rationalizations I mean this at the societal level; because of ignorance there may be genuine reasons (even apart from the purely personal) as far as the particular arresting policeman, condemning judge, imprisoning warder, interfering supervisor, or citizen deprived of saner remedy is concerned. So if we abolish contra-crime forcefully without removing its reasons for existence, the probability is that it will arise again in a new guise but very little changed. If we try to use force to prevent it arising, we are ourselves a government (and incidentally, I suppose, no longer anarchists) and unless the reasons for contra-crime have been removed we shall soon find ourselves practising it. Of course, it is just possible that the main support of the continued existence of contra-crime is a mistaken belief that we need it, and in this case if we successfully abolished it we should at the same time automatically be abolishing its cause. But I doubt it.

The individual reader has to come to terms with the fact that contra-crime exists, is objectively useless, is nasty, and is beyond his power to reduce or abolish. In this sense it is like war. It is like war in three other ways. We should like its abolition but cannot really believe in this. It forms one of the largest bases of both child and adult recreation; and when we are introduced to an eminent practitioner, however much we despise the man-killing and man-caging trades, nevertheless we treat him, be he judge or general, with an unthinking respect and even make him what in our quaint language we term a guest of honour. So although we must all reach our own decisions, either as researchers or as whatever we may be, about what to do in face of the findings of this book, I would ask those readers who agree with my general position but do not know how to come to terms with it one, I hope helpful, question: What are we doing about war?

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“It is the custom to ornament every scientific work with a bibliography, a list of earlier books on the same subject. The aim may be to test the reader’s competence by laying out for him an awe-inspiring course of reading; it may be to prove the author’s competence by showing the mountain of dross he has sifted to win one nugget of truth.”

*Peter and Hull (1969)*

I have obviously leaned very heavily on the reference listed as Turk (1969) for my discussions of criminality and on the reference listed as Simon (1971) for my discussions of prediction. These are two splendid introductory texts. My constant reference to these works explains some otherwise surprising gaps in the list of references. There are several important sociologists who have adopted positions to some extent supportive of my own in this present work, and whom I have not mentioned, since what I would need to say about them Turk has said better. Similarly, the very small mention here of the Californian prediction researchers, Ballard, Beverly, Gottfredson and others, should not disguise the importance of their work nor my admiration for them. But again, what I would say about them has been better said elsewhere, this time by Simon.

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## APPENDIX A

### Predictive Attribute Analysis

The most detailed description of this technique is in Macnaughton-Smith (1965). In the form in which it is used in the present study we begin by defining an information statistic. Consider a  $2 \times 2$  contingency table where the observed frequencies are as follows:

		Criterion Dichotomy		TOTAL
		YES	NO	
Predictor Dichotomy	YES	a	b	e
	NO	c	d	f
	TOTAL	g	h	n

Now write  $A = a \log a$ ,  $B = b \log b$ , etc.

If  $a = 0$  or  $1$ , put  $A = 0$  and so with  $b$ ,  $c$ ,  $d$ , etc. (Use natural logs (to base  $e$ ) not Napierian (to base  $10$ ).)

Then we define an information statistic

$$I = A + B + C + D - (E + F + G + H) + N.$$

When  $a$ ,  $b$ , etc, are fairly large  $2I$  takes a value very close to  $\chi^2$  for the same table. When some of  $a$ ,  $b$ , etc. are small  $2I$  has rather more desirable properties than  $\chi^2$ . For a more detailed account see Kullback (1959).

For the study sample, calculate  $2I$  for each of the  $2 \times 2$  tables formed by the criterion dichotomy taken together with each of the predictor dichotomies in turn. Choose the predictor dichotomy which yields the largest value of  $2I$ . If this is less than some arbitrary figure (in the present study we took  $6.635$ , the value of  $\chi^2$  at the  $.01$  significance level; this is a

convenient figure but must not be regarded as a significance test of  $\max 2I$ ) then the analysis stops. If  $2I > 6.635$ , divide the study sample into the two subsets consisting of those who possess and lack the predictor dichotomy respectively. Repeat the operations so far described on each of the subsets so formed. We continue in the same way; at each stage a subset is either split into two or declared a "final set". If a subset is split into two final sets, and one of these contains fewer than a certain number of individuals (in this study, 10) then the split is rescinded and the parent subset is declared a final set.

Now take the replication sample and calculate  $2I$  for the  $2 \times 2$  table formed by the criterion and the same predictor as was chosen for the first subdivision in the analysis of the study sample. Choose a desired significance level, .05 in the present study, giving a minimum value of  $2I$  of 3.841.\* If  $2I$  is greater than or equal to this lower limit, we divide the replication sample into two and repeat the process, using every time the predictor chosen in the corresponding part of the analysis of the study sample. When  $2I$  is less than our lower limit, the subset is declared final.

A  $2 \times k$  table may now be set up showing the numbers of individuals possessing and lacking the criterion in each of the  $k$  final sets. Any suitable measure of the predictive value of this subdivision into  $k$  sets may be used. Usually I would recommend the appropriate information statistic for a  $2 \times k$  table (which has the convenient and interpretable property of being partitionable; that is to say, it is equal to the sum of the information statistics from all the  $2 \times 2$  tables created by our subdivisions of the replication sample). In the present study we wished to compare these analyses with corresponding multiple regressions, so from the  $2 \times k$  table we calculated  $\chi^2$  with  $k - 1$  degrees of freedom, and then  $\phi = \sqrt{\frac{\chi^2}{N}}$ .  $\phi$  is directly comparable with  $r$  from a multiple regression.

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\*In one case a value of  $2I$  of less than 3.841 was accepted because it fulfilled the conditions of the corresponding 1 - tailed test;  $2I > 2.706$  and the association between the criterion and the predictor was in the same direction in the replication and study samples.

## APPENDIX B

### The use of linear regression to predict a dichotomized criterion

There are two objections to this procedure.

(1) In linear regression the predicted value of  $y$  is an estimate of the population mean of  $y$  corresponding to a given value of  $x$ . Given that the observed  $y$  is equal to 1 when a criterion event occurs and 0 when it does not, the predicted value of  $y$  is therefore an estimate of the probability of the criterion event. As such it lies between 0 and 1. But in general the range of  $x$  and the gradient of the regression line of  $y$  upon  $x$  is such that predicted values of  $y$  less than 0 or greater than 1 occur. Mannheim and Wilkins (1955) treated all predicted values of  $y$  of less than .15 as indicating a probability of .15, and all predicted values of greater than .85 as indicating a probability of .85, but this is purely arbitrary and to some extent undermines the logic of the formulae from which the linear equation is calculated in the first place.

(2) The least-squares technique used to calculate the equation of a regression line rests on the assumption that the variance of the observed  $y$ 's about the expected  $y$ 's is independent of  $x$ . Yet a binomial event with probability  $p$  has variance  $p(1 - p)$ , and since in our case  $\Sigma(y|x) = p$  and is a linear function of  $x$ , this condition of independence is not met. The usual technique in a case of non-independent variance is to use weighted sums of squares, products, etc.,  $\Sigma wx^2$ ,  $\Sigma wy^2$ ,  $\Sigma wxy$  where  $w = \frac{1}{\text{error variance of } y}$ . Unfortunately in our case this would lead to infinite weight when expected  $y = 0$  or 1, so that any straight line passing through one success and one failure would in general become "perfect", and thus

the method degenerates. Ways around this difficulty could probably be, but so far as I know have not been, devised.

In practice these two objections do not seem to matter much. A logical and in many ways attractive way out would be to use the logit transform  $z = \log \frac{p}{1-p}$  with  $z$  instead of  $p$  as a linear function of  $x$ , fitted by maximum likelihood. Unfortunately, a stepwise multivariate programme for the heavy iterative calculations required by this approach was not available and would have been complicated to develop. Ironically after what we have just said, but fortunately nevertheless, the "good old" unweighted least-squares method is a reasonable first approximation to the maximum likelihood solution of the equation using the logit transform. The use of strictly invalid methods that approximate to logically justifiable techniques is a very small part of the reasons for the comparative inadequacy of so much criminological prediction. See the discussion of this in Section 14.

## APPENDIX C

### The use of dichotomous predictor variables in linear regression

It is often wrongly held that linear regression and multiple linear regression are invalid unless the values of  $x$  are approximately normally distributed, and that special problems arise when dichotomous predictors are used. This misapprehension seems to arise from the fact, correctly summarized for example by Snedecor (1938, 4th edition 1946) that "If random samples are drawn from a normal bivariate population with correlation,  $\rho$ , the statistic,  $r$ , though not unbiased, is an appropriate estimate of  $\rho$ . Errors in either  $X$  or  $Y$  will further bias the coefficient. Unlike regression samples, neither  $X$  nor  $Y$  may be selected. You see, then, that in [some] samples . . . the correlation coefficient provides only a doubtful estimate of  $\rho$  because  $X$  is selected". (Snedecor writes  $X$  and  $Y$  where we write  $x$  and  $y$ .)

This leaves us in the position that if in our sample  $x$  is limited to two values only, either the population is non-normally distributed or the dichotomous values are very crude measures of what is really a normally distributed continuous variable. Many behavioural scientists incline to the second view and their analysis become enmeshed with complicated, inefficient measures such as tetrachoric correlations, on which it is very hard in practise to carry out significance tests, and which yield estimates about hypothetical variables, not analysis of the data that we actually have.

Now undeniably a dichotomy is often *related* to a hypothetical continuous variable. The division of our sample into "first convictions" and others undoubtedly relates to "degree of past criminalization". But equally, in terms of the information it gives us, it may be treated here as a dichotomy in its own right, not as an approximation to something else. A recorded

conviction is either the only one recorded on that particular piece of paper by that particular bureaucratic system, or it isn't. So we may validly regard our population as having a bivariate binomial (or in multiple regression, multiple binomial) distribution without inherent measurement error. The tabulated values of  $r$  for given significance levels work quite well here. For example, in the bivariate case, we see from the simple formula for  $\chi^2$  in a  $2 \times 2$  table that for two dichotomies  $r = \sqrt{\frac{3.841}{N}}$  at the 5% level and  $\sqrt{\frac{6.635}{N}}$  at the 1% level ( $r$  here becomes identical with the measure more commonly known as  $\phi$ ). These formulae give for varying values of  $N$  values of  $r$  very close to those found in statistical tables for random samples from bivariate normal populations.

Thus we see that the use of dichotomous predictors is legitimate. The question remains, is it efficient. This has been well reviewed by Simon (1971), who, in discussing the effect on predictive power of using dichotomized predictors, says "The few studies which bear on this question seem to indicate that in practise it may not make much difference. Gottfredson and Ballard (1965)\* mention in their eight-year follow-up study that in the construction of one of their tables all variables were dichotomized, while in another (based on the same sample) they were not; but both analyses (done by multiple regression) gave very similar results. On a two-year follow-up the two tables had M.C.R.s\*\* of .33 and .31 respectively; after an eight-year follow-up these values were reversed. In the present study [i.e. the one that Simon is reporting] one equation which used solely dichotomized variables had an  $r$  on validation of .13 while four other equations [using mixtures of dichotomies and other variables] had  $r$ 's between .16 and .18. In another sample analysed in the same study the regression analysis using scaled variables produced a very similar result to the predictive attribute analysis using their dichotomized forms." She points out that in general the data used in criminological prediction seem to yield up their useful information equally readily to quite crude and to more elegant methods of scaling and analysis. The problems of such prediction seem to be criminological, or rather, logical and common-sensical, more than statistical. Thus in the present study where we wanted to keep the multiple regressions as comparable as possible with the predictive attribute analysis, it seems reasonable to put all predictors in the dichotomized form required by the latter.

\*In this quotation I have added references where they were given in other parts of Simon's text. Similarly I have changed the wording slightly where it would be difficult to follow out of her context.

\*\*The M.C.R. is a measure of predictive efficiency developed by Duncan, Ohlin, Reiss and Stanton (1953) which tends in many cases to take a value slightly larger than  $\phi$ . But its relationship to other measures and its probability distribution are not easy to deduce.

## APPENDIX D

### An Act to provide for the Conditional Liberation of Persons Undergoing Sentences of Imprisonment

[Assented to 6th September, 1958]

**H**ER Majesty, by and with the advice and consent of the Senate and House of Commons of Canada, enacts as follows:

#### SHORT TITLE

1. This Act may be cited as the *Parole Act*. Short title

#### INTERPRETATION

2. In this Act, Definitions
- (a) "Board" means the National Parole Board established by this Act; "Board"
- (b) "inmate" means a person who has been convicted of an offence under an Act of the Parliament of Canada and is under sentence of imprisonment for that offence, but does not include a child within the meaning of the *Juvenile Delinquents Act* who is under sentence of imprisonment for an offence known as a delinquency; "Inmate"
- (c) "magistrate" means a justice or a magistrate as defined in the *Criminal Code*; "Magistrate"
- (d) "parole" means authority granted under this Act to an inmate to be at large during his term of imprisonment; "Parole"
- (e) "paroled inmate" means a person to whom parole has been granted; "Paroled inmate"
- (f) "parole supervisor" means a person appointed by the Board to guide and supervise a paroled inmate; and "Parole supervisor"
- (g) "regulations" means regulations made by order of the Governor in Council. "Regulations"

### BOARD ESTABLISHED

Board established	<p>3. (1) There shall be a board, to be known as the National Parole Board, consisting of not less than three and not more than five members to be appointed by the Governor in Council to hold office during good behaviour for a period not exceeding ten years.</p>
Chairman and Vice-Chairman	<p>(2) The Governor in Council shall designate one of the members to be Chairman and one to be Vice-Chairman.</p>
Temporary members	<p>(3) The Governor in Council may appoint a temporary substitute member to act as a member in the event that a member is absent or unable to act.</p>
Quorum	<p>(4) A majority of the members constitutes a quorum, and a vacancy on the Board does not impair the right of the remaining members to act.</p>
Rules of procedure	<p>(5) The Board may, with the approval of the Governor in Council, make rules for the conduct of its proceedings and the performance of its duties and functions under this Act.</p>
Head office	<p>(6) The head office of the Board shall be at Ottawa, but meetings of the Board may be held at such other places as the Board determines.</p>
Seal	<p>(7) The Board shall have an official seal.</p>
Remuneration	<p>4. (1) Each member of the Board shall be paid such remuneration for his services as is fixed by the Governor in Council, and is entitled to be paid reasonable travelling and living expenses incurred by him while absent from his ordinary place of residence in the course of his duties.</p>
Staff	<p>(2) The officers, clerks and employees necessary for the proper conduct of the business of the Board shall be appointed in accordance with the provisions of the <i>Civil Service Act</i>.</p>
Chief executive officer	<p>(3) The Chairman is the chief executive officer of the Board and has supervision over and direction of the work and the staff of the Board.</p>

### POWERS AND DUTIES OF BOARD

Jurisdiction of Board	<p>5. Subject to this Act and the <i>Prisons and Reformatories Act</i>, the Board has exclusive jurisdiction and absolute discretion to grant, refuse to grant or revoke parole.</p>
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**6.** (1) The Board shall at the times prescribed by the regulations Review of cases

(a) review the case of every inmate serving a sentence of imprisonment of two years or more, whether or not an application has been made by or on behalf of the inmate, and

(b) review such cases of inmates serving a sentence of imprisonment of less than two years as are prescribed by the regulations, upon application by or on behalf of the inmate.

(2) Upon reviewing the case of an inmate as required by subsection (1) the Board shall decide whether or not to grant parole. Decisions

**7.** The Governor in Council may make regulations prescribing Regulations

(a) the portion of the terms of imprisonment that inmates shall serve before parole may be granted,

(b) the times when the Board shall review cases of inmates serving sentences of imprisonment, and

(c) the class of cases of inmates serving a sentence of imprisonment of less than two years that shall be reviewed by the Board upon application.

**8.** The Board may Powers of Board

(a) grant parole to an inmate if the Board considers that the inmate has derived the maximum benefit from imprisonment and that the reform and rehabilitation of the inmate will be aided by the grant of parole;

(b) grant parole subject to any terms or conditions it considers desirable;

(c) provide for the guidance and supervision of paroled inmates for such period as the Board considers desirable; and

(d) revoke parole in its discretion.

**9.** The Board, in considering whether parole should be granted or revoked, is not required to grant a personal interview to the inmate or to any person on his behalf. Personal interview

**10.** Where the Board grants parole it shall issue a parole certificate, under the seal of the Board, in such form as the Board prescribes, and shall deliver it or cause it to be delivered to the inmate and a copy to the parole supervisor, if any. Parole certificate

Effect of parole                   **11. (1)** The sentence of a paroled inmate shall, while the parole remains unrevoked and unforfeited, be deemed to continue in force until the expiration thereof according to law.

Idem                                   **(2)** Until a parole is revoked, forfeited or suspended the inmate is not liable to be imprisoned by reason of his sentence, and he shall be allowed to go and remain at large according to the terms and conditions of the parole and subject to the provisions of this Act.

**SUSPENSION OF PAROLE**

Suspension of parole           **12. (1)** A member of the Board or any person designated by the Board may, by a warrant in writing signed by him, suspend any parole and authorize the apprehension of a paroled inmate whenever he is satisfied that the arrest of the inmate is necessary or desirable in order to prevent a breach of any term or condition of the parole.

Apprehension of paroled inmate   **(2)** A paroled inmate apprehended under a warrant issued under this section shall be brought as soon as conveniently may be before a magistrate, and the magistrate shall remand the inmate in custody until the Board cancels the suspension or revokes the parole.

Review by Board                   **(3)** The Board shall forthwith after a remand by a magistrate under subsection (2) review the case and shall either cancel the suspension or revoke the parole.

Effect of suspension               **(4)** An inmate who is in custody by virtue of this section shall be deemed to be serving his sentence.

**FORFEITURE OF PAROLE**

Forfeiture                           **13.** If a paroled inmate is convicted of an indictable offence, committed after the grant of parole and punishable by imprisonment for a term of two years or more, his parole is thereby forthwith forfeited.

**APPREHENSION UPON REVOCATION OR  
FORFEITURE OF PAROLE**

Apprehension                   **14. (1)** If any parole is revoked or forfeited, the Board may, by warrant under the seal of the Board, authorize the apprehension of the paroled inmate.

Recommitment                   **(2)** A paroled inmate apprehended under a warrant issued under this section, shall be brought as soon as conveniently may be

before a magistrate, and the magistrate shall thereupon make out his warrant under his hand and seal for the recommitment of the inmate as provided in this Act.

#### EXECUTION OF WARRANT

**15.** A warrant issued under section 12 or 14 shall be executed by any peace officer to whom it is given in any part of Canada, and has the same force and effect in all parts of Canada as if it had been originally issued or subsequently endorsed by a magistrate or other lawful authority having jurisdiction in the place where it is executed.

Warrants for  
apprehension

#### RECOMMITMENT OF INMATE

**16.** (1) Where the parole granted to an inmate has been revoked he shall be recommitted to the place of confinement to which he was originally committed to serve the sentence in respect of which he was granted parole, to serve the portion of his original term of imprisonment that remained unexpired at the time his parole was granted.

Place of  
recommit-  
ment

(2) Where a paroled inmate, upon revocation of his parole, is apprehended at a place not within the territorial division to which he was originally committed, he shall be committed to the corresponding place of confinement for the territorial division within which he was apprehended, to serve the portion of his original term of imprisonment that remained unexpired at the time his parole was granted.

Idem

**17.** (1) When any parole is forfeited by conviction of an indictable offence the paroled inmate shall undergo a term of imprisonment equal to the portion of the term to which he was originally sentenced that remained unexpired at the time his parole was granted plus the term, if any to which he is sentenced upon conviction for the offence.

Effect of  
forfeiture

(2) The term of imprisonment prescribed by subsection (1) shall be served as follows:

Term to  
be served

(a) in a penitentiary, if the original sentence in respect of which he was granted parole was to a penitentiary;

(b) in a penitentiary, if the total term of imprisonment prescribed by subsection (1) is for a period of two years or more; and

(c) in the place of confinement to which he was originally committed to serve the sentence in respect of which he was granted parole, if that place of confinement was not a penitentiary and the term of imprisonment prescribed by subsection (1) is less than two years.

Conviction for offence committed during parole

(3) Where a paroled inmate is, after the expiration of his parole, convicted of an indictable offence committed during the period when his parole was in effect, the parole shall be deemed to have been forfeited on the day on which the offence was committed, and the provisions of this Act respecting imprisonment upon forfeiture of parole apply *mutatis mutandis*.

#### ADDITIONAL JURISDICTION

Revocation or suspension of certain punishments

**18.** (1) The Board may, upon application therefor and subject to regulations revoke or suspend any sentence of whipping or any order made under the *Criminal Code* prohibiting any person from operating a motor vehicle.

Clemency

(2) The Board shall when so directed by the Minister of Justice make, any investigation or inquiry desired by the Minister in connection with any request made to the Minister for the exercise of the royal prerogative of mercy.

#### MISCELLANEOUS

Order, etc. final

**19.** An order, warrant or decision made or issued under this Act is not subject to appeal or review to or by any court or other authority.

Evidence

**20.** Any order, decision or warrant purporting to be sealed with the seal of the Board or to be signed by a person purporting to be a member of the Board or to have been designated by the Board to suspend parole is admissible in evidence in any proceedings in any court.

Expenditures

**21.** All expenditures under or for the purposes of this Act shall be paid out of money appropriated by Parliament therefor.

Superannuation

**22.** The members and staff of the Board shall be deemed to be employed in the Public Service for the purpose of the *Public Service Superannuation Act*.

Transfer of staff

**23.** Notwithstanding subsection (2) of section 4, the Governor in Council may by order transfer persons who prior to the commencement of this Act were members of the staff of the Department of Justice to the staff of the Board.

**24.** (1) The *Ticket of Leave Act* is repealed.

Repeal  
R.S. 1952,  
c. 264

(2) Every person who at the coming into force of this Act is the holder of a licence issued under the *Ticket of Leave Act* to be at large shall be deemed to have been granted parole under this Act under the same terms and conditions as those under which the licence was issued or such further or other conditions as the Board may prescribe.

Licence under  
former Act  
deemed  
parole

(3) Every person who was issued a licence to be at large under the *Ticket of Leave Act*, whose licence was revoked or forfeited and who at the coming into force of this Act is unlawfully at large may be dealt with under this Act as though he were a paroled inmate whose parole had been revoked or forfeited.

Revoked or  
forfeited  
licence

(4) A reference in any Act, regulation or document to a conditional liberation or ticket of leave under the *Ticket of Leave Act* shall be deemed to be a reference to parole granted under this Act.

Reference

(5) The powers, functions and duties of the Minister of Justice under section 666 of the *Criminal Code* are hereby transferred to the Board, and a reference in that section to permission to be at large on licence shall be deemed to be a reference to parole granted under this Act.

Habitual  
criminals

**\*25.** This Act shall come into force on a day to be fixed by proclamation of the Governor in Council.

Coming into  
force

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\*Note – Proclaimed in force as of February 15, 1959.