calculate the chance of observing exactly, or at least, n "sixes", then binomial equations are relevant. In this kind of experiment we impose a linear sequence on the trials and we may observe successes, say, at the 3rd, 7th, and 15th throws (trials).

When, however, we come to examine a patient of age t years for the presence of 3 independent somatic mutations, we do not take each of his L cells at risk and expose them in turn (L trials) for t years. We actually examine the whole individual. Suppose, however, that by some histological technique we could number these L cells systematically, and suppose we could show that cells numbered 3, 7 and 15 were mutant at t years. From these observations we would be unable to determine the *sequence* in which these three cells became mutant. This contrasts with "independent trial" experiments where the sequence of successes—at the 3rd, 7th and 15th trials—is known. In fact, the three cells could have become mutant in any one of 3! (that is, 6) different sequences and each sequence is equally probable. When kt is small (kt being the probability of any one mutation at t) the probability of each of the 6 distinctive sequences of any three mutations is equal to the probability of finding three successes in the independent trial experiment, that is, $(kt)^3/3!$. It follows, that the probability, at low kt, of observing in a patient *any* one of the 3! equally probable sequences is $(kt)^3$. Provided $L >> n$, the probability of observing any sequence of n mutations at low kt is, by analogy: $(kt)^n$.

This result can be obtained with equal facility from the law of independent probabilities. A set of events is said to be *independent* if the occurrence of any one of them is not influenced by the occurrence of the others. These are the conditions postulated for my somatic mutation model. L is large ($L >> n$) and the occurrence of a mutation in any one cell is not influenced by the occurrence of mutations in other cells. (In the independent trial model, one success follows another in a known *sequence* and this constitutes a *dependent* relationship.) If the probabilities of occurrence of a set of n independent events are: $p_1, p_2 \ldots p_n$, then the probability, P, that all of the set of events will occur, is defined by: $P = p_1 p_2 \ldots p_n$. This is the law of independent probabilities. At low kt, the probability of observing a single mutation is kt, and because mutations are independent, the chance of observing any other similar mutation is also kt. It therefore follows from the above law of independent probabilities that the chance of observing a set of n mutations at low kt is $(kt)^n$—the result obtained above.

When kt is not small it can readily be shown by solving the correct differential equation (Burch, 1964a) that the probability, $P_{\not<n}$, of finding at least n independent mutations is given by:

$$P_{\not<n} = (1-e^{-kt})^n \qquad (1)$$

The probability, P_n, of finding exactly n independent mutations (Yule, 1924; Burch, 1964a; Irwin, 1964) is given by:

$$P_n = e^{-kt}(1-e^{-kt})^n \qquad (2)$$

It would be disconcerting if the age-specific prevalence, or age-specific initiation-rates of "spontaneous" idiopathic diseases in appropriately homogeneous populations never conformed to equation (1) or its relatives. Happily, very good agreement is in fact observed (Burch, 1963, 1964b, 1965; Burch and Rowell, 1965). In the course of many studies I have never found conformity between such data and the logically inapplicable Poisson formalism, except of course when $n = 1$, where no issues of sequence arise, and where the equations for independent trials and independent events coincide.

P. R. J. Burch,
Deputy Director and Reader.
M.R.C. Environmental Radiation Research Unit,
Department of Medical Physics,
University of Leeds,
The General Infirmary, Leeds, 1.

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BEHAVIOUR THERAPY FOR TRANSVESTISM

Dear Sir,

I was interested to read again reports of the cases of transvestism treated by Barker with aversion therapy (*Brit. J. Psychiat.* March, 1965, 268–276). Though I am in general agreement with much that he says, there are a few points with which I disagree.

I first treated a transvestist with aversion therapy in 1956, and since then my colleagues and I have treated five more. We have used apomorphine, and contrary to what Barker says, it is perfectly possible to utilize the actual process of dressing up in female clothing. It is certainly time-consuming, and the degree of control is obviously much less than with

electrical techniques. The obstacles in timing are a good deal less formidable than is generally believed. The treatment is unpleasant, but in my own experience more patients object to being given electric shocks than to the nausea following apomorphine. The apomorphine regime which we currently use, and which I have reported elsewhere (1964), is certainly not one which may result in the breakdown of a patient's health. Whether or not there is any "personal degradation and humiliation" will depend almost entirely on the attitudes of the therapists. In any case, having to indulge in or to simulate perverse acts of which the patient is often ashamed may be degrading and humiliating in itself, and it is difficult to see why it is less so if he is wired up to a mat and more so after apomorphine. We do not find that the central depressant properties of apomorphine interfere with conditioning, whatever the theoretical considerations may be. It is unfortunate that Barker has constructed a comparison of the two techniques around his two cases. The result of the first is that after 18 months the patient was abroad and presumed symptom-free. The other suffered relapse less than a year after treatment by the "better" technique.

My colleagues and I have been attempting to compare electrical and pharmacological techniques in an anti-smoking clinic. Controlled comparison is extremely difficult, but the pharmacological technique seems to us to be a good deal more effective. We have also been combining the two approaches. I recently saw a mackintosh fetishist whom we successfully treated thus 18 months ago. In view of Barker's paper, I asked him whether he would care to say which of the techniques was most successful in producing aversion, and which he found most unpleasant. His replies are probably the most interesting features of his case, and will be fully reported later. In short, however, he said that though the electric shocks were more unpleasant than the nausea (which we aim to ensure is minimal and short-lived), yet the apomorphine sessions were the effective ones.

MICHAEL J. RAYMOND.

Fairdene and Netherne Hospitals,
Coulsdon, Surrey.

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HISTORY OF CANADIAN PSYCHIATRY

DEAR SIR,

A lively interest has developed across Canada in the history of medicine in general and in the history of Canadian psychiatry in particular. The Canadian Psychiatric Association has established a Committee on the History of Psychiatry which is now attempting to prepare a comprehensive bibliography.

We would like to request the co-operation of your readers in helping us to obtain information about books, articles, including unpublished theses, etc., dealing with the history of psychiatry in Canada. Detailed information or vague references would be equally welcome. They could be sent to

Mr Cyril Greenland, Associate (Archivist),
Department of Psychiatry,
University of Toronto,
2 Surrey Place,
Toronto 5, Canada.

We should also be very pleased to receive superfluous or unwanted books of historical interest in this field, reports, pamphlets, letters or memorabilia which might throw light on the early days of psychiatry in Canada. In cases where documents of unusual interest cannot be released, we should, in any event, like to correspond about them with a view to arranging for reproduction.

J. D. GRIFFIN.

Chairman, Committee on History of Psychiatry,
Canadian Psychiatric Association.
52 St. Clair Avenue East,
Toronto 7, Ontario, Canada.

KORO—A CULTURE-BOUND DEPERSONALIZATION SYNDROME

DEAR SIR,

Following Dr. P. M. Yap's interesting paper (*Brit. J. Psychiat.*, January, 1965, pp. 43–50) it may be of value to describe a further case of Koro which came to notice in the British Military Hospital, Singapore, in 1964.

The patient was a married Singapore Chinese male aged 28 years who worked as a general labourer in a nearby Transport unit. His work record showed that he had lost a considerable period due to going sick with minor physical disabilities. He presented himself to the unit medical officer with a friend firmly grasping his penis, stating that the organ was visibly shrinking into his abdomen and that if it disappeared completely he would die. In appearance he was obese with loss of frontal hair (a photograph taken 1 year ago for his identity card was available for comparison). The generative organs showed no obvious disease, but were minimally hypoplastic. Physical examination showed no somatic illnesses, and in particular bilateral inguinal hernia and myxoedema