Matched controls in surveys of the use of general practice

ANOTHER USE FOR RESEARCH PRACTICE AGE-SEX REGISTERS

IAN RUSSELL, M.A., M.Sc., F.S.S.
Lecturer in Medical Statistics, Medical Care Research Unit,
University of Newcastle-upon-Tyne

SUMMARY. This paper describes how an age-sex register may be used as a sampling frame for the selection of matched controls in studies of the use of general practice. It discusses both the analysis of data collected in this way and the advantages and limitations of the method itself. The application of the method to a case-study suggests that patients who use hospital accident departments are also high users of general practice. Although care is needed in the interpretation of such results, it is concluded that matched controls can be used to ameliorate some of the problems of research in general practice.

Introduction

During the past few years there has been much reported use of the age-sex register as a management tool in general practice; for example, these registers have been used to schedule screening for paediatric abnormalities, cancer of the cervix, and the surveillance of such ' at risk ' groups as the chronic sick and the elderly (Acheson and Forbes, 1968; Pinsent, 1968; Hodes, 1969; Gruer, 1972; Boddy and Bryden, 1973; Dixon and Morris, 1974).

In the organisation of health care, Richardson and Dingwall-Fordyce (1968) have shown how age-sex registers can be used in planning the site of a health centre. Similarly in medical education, Irvine (1972 and 1974) has strongly advocated their use as a teaching aid in general practice.

In research, age-sex registers have come to be regarded as a *sine qua non* for the study of the use of general practice, whether carried out nationally (Logan and Cushion, 1958; Office of Population Censuses and Surveys, 1974), locally (Ashford and Pearson, 1970), or in a single practice. Problems which have been tackled in this way by individual practices include the demand for urgent treatment (Forbes *et al.*, 1967), the effect of an appointments system (Morrell and Kasap, 1972) and the effect of moving to new premises (Morrell and Nicholson, 1974).

On reviewing the Medical Research Council's report (1960) on age-sex registers (sub-titled *A basic requirement for epidemiological research in general practice*), it is interesting to observe that most of the uses reviewed above were then foreseen and encouraged. However, there is one function of the age-sex register envisaged in that report which a cursory review of the literature suggests has been under-used during the past 15 years; that is as a sampling frame for epidemiological research, in particular as the basis for the selection of controls matched for age and sex with a set of patients under investigation. For although Berkeley *et al.* (1972) described the creation of a general-practice register as a sampling frame for cytogenetic research in a population, only one paper has appeared which describes the use of matched controls in a study of

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the use of general practice—Whitfield (1972) employed it to test whether recently registered patients differ in their consultation rates from patients registered for between ten and 15 years.

Billewicz (1964), Pike and Morrow (1970), and Russell (1975) have all drawn attention to studies of matched controls or matched responses which have been analysed as if no matching had taken place, that is as if patients and controls came from independent samples. Now although Whitfield's analysis does take the matching into account, it is neither explicit nor efficient (in a sense to be defined). This paper therefore describes the method of matched controls as it applies to surveys of general practice and reports a case-study which illustrates the effective use of this technique.

Research methods

Rationale

It is well known that consultation rates in general practice vary considerably with age and sex (Ashford and Pearson, 1970; Morrell *et al.*, 1970; Office of Population Censuses and Surveys, 1974). Consequently, in any study which seeks to determine the effect of a third factor on patients' patterns of demand, there are advantages in matching the selected patients having the attribute under investigation (often referred to as *propositi*) with controls of the same age and sex, but without the attribute in question. However, the method of matched controls does have its disadvantages; since these will be discussed later, we assume here that the method has been adopted only after a careful consideration of the advantages and disadvantages.

Now it cannot be emphasised too strongly that, once this method has been selected, to divorce pairs of corresponding measurements on the propositus and his control, as if they were independent, is to deny the initial assumption that use is dependent on age and sex; and thus to accept any disadvantages the method may have without reaping all the benefits. In particular it is worth stressing that, whether these measurements are quantitative (e.g. number of consultations) or 'all-or-none' (e.g. whether the patient has been admitted to hospital), it is the *difference* between the measurement on the propositus and that on the control which is to be analysed, rather than the measurements themselves.

Selection of propositi and controls

If the number of patients in the group to be studied is 'large', the investigator will make best use of his time by drawing a random sample of propositi from the group. For example, it will often be appropriate to compare the numbers of consultations over a period of one year. In such a case, calculations based on the case-study described below suggest that a series of 100 pairs of patients and their controls will usually suffice to identify mean differences greater than one consultation per patient per year.

Consequently if the number of patients to be investigated is one thousand, a ten per cent sample would be appropriate. Such a sample could most simply be drawn by selecting the first propositus at random from the first ten patients on the list and taking every tenth patient thereafter.

When the age-sex register to be used as a sampling frame lists the patients of each sex in strictly ascending order of age, each propositus should be matched with the first patient who does not belong to the group under study. More commonly however the register will group together all patients of the same sex and year of birth in an alphabetical list; in such cases it is only necessary for the propositus to be matched with a patient in the same list rather than the one whose birthday is closest. Provided that patients having the attribute under investigation are excluded, any objective method of selecting controls will suffice.

By contrast, Whitfield (1972) selected his controls at random and then attempted " to match (them) as closely as possible " with propositi drawn from his list of recently registered patients. Not only does this approach produce several quite unnecessary mismatchings, but it also introduces an avoidable element of subjectivity into the selection of matched pairs.

Collection of data

Were it not for the studies known to me in which data have been collected from propositi in a very different way from that in which they were collected from controls, I would have thought it unnecessary to stress the need for identical methods of data collection. As Armitage (1971) puts it, in his commentary on Doll and Hill's (1950) classic study of the relationship between smoking and lung cancer using the method of matched controls:

> " This paper is an excellent illustration of the care which should be taken to avoid bias due to unsuspected differences between case and control groups or to different standards of data recording."

A light-hearted but nevertheless salutary account of the origin of some of these biases is given by Hart and Huskisson (1972).

In order to give effect both to the need for comparable methods of data collection and to the principle that pairs of corresponding observations should be regarded as inseparable, it is recommended that the information on an individual patient and on his control should be abstracted, usually from their medical records, by the same investigator on to the same questionnaire.

Statistical analysis

It has already been argued that the method of matched controls requires the analysis of differences within pairs. In the case of a clinical trial where the measurements are quantitative, this is usually achieved by the application of a one-sample t-test. For although this test theoretically assumes that the differences to be analysed are normally distributed, in practice it can cope with considerable departures from this basic assumption.

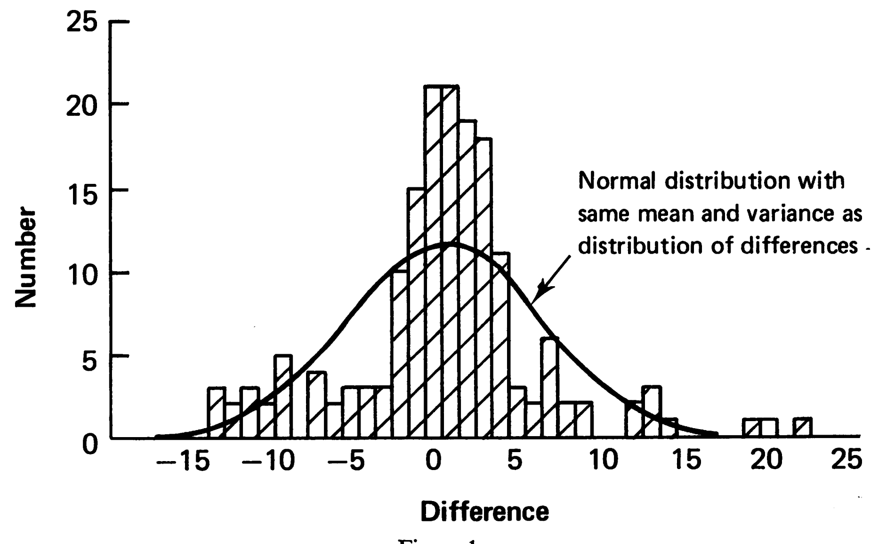

Figure 1

Difference between number of surgery consultations by a patient visiting an accident department and his matched control, in the year *preceding* the visit.

Now although the various authors who have investigated the mathematical distribution of consultations in general practice (Froggatt *et al.*, 1969; Spencer, 1971; Ashford, 1972; Ashford and Hunt, 1974) are not agreed on its precise form, they are all agreed that it is skewed distribution with a very long ' tail '; or, to put it another way, that a very small proportion of patients accounts for a considerable proportion of the workload.

Figure 1 gives an example of what the difference between two such distributions looks like, in this case the difference in the numbers of consultations between propositus and control. Although the skew is eliminated, one is left with a distribution which has, in comparison with a normal distribution of the same mean and variance, two long ' tails '.

At this point it is helpful to apply an essentially non-statistical concept of efficiency to the statistical problem of choosing between two tests of significance. Suppose two tests A and B are to be used to test a hypothesis which (unknown to the investigator) is false; then test A could be said to be more efficient than B if *on average*, for a sample of given size, it yields a more significant result, that is a ' better ' significance level; or alternatively, if, to achieve a given significance level, it requires *on average* a smaller sample.

Expressed in these terms, it would be very inefficient to apply the t-test to data on the use of general practice generated by the method of matched controls, as in figure 1. Instead, it is suggested that such data should be analysed by a non-parametric test, that is one which, unlike the t-test, makes no prior assumption about the distribution of the data. For although this is not the only alternative to the t-test, the more sophisticated alternatives require either statistical skill or the resources of a computer.

Since ' distribution-free ' tests, as they are often termed, are conceptually and operationally much simpler than the other choices and since the differences in efficiency are by no means overwhelming, it is recommended that matched control studies of the use of general practice should in general be analysed by non-parametric procedures. (Although these methods are to be preferred for the specific type of data under consideration in this paper, they should not be applied to other statistical problems without an assessment of their advantages and disadvantages, as discussed, for example, by Armitage (1971)).

A very readable and cheap account of non-parametric tests is given by Siegel (1956). Of the tests relevant to the method of matched controls, only two are appropriate for the sample sizes we are considering, Wilcoxon's test for quantitative pair differences and McNemar's test for ' all-or-none ' pair differences (often referred to as ' binary ' pair differences). However, the application and relative efficiency of these two tests are best described with reference to a specific example.

Case-study

Method

Walker (1973) surveyed flows of patients during a period of 12 months between a large inner-urban group practice in Newcastle-upon-Tyne and all the hospitals in the locality. As part of this study, the registers of patients of both the local accident and emergency departments were inspected for patients who had given the name of one of the eight principals as their general practitioner. Scutiny of the corresponding hospital records established, *inter alia*, that between them the practitioners had referred 295 (13 per cent) of the total of 2,287 new attendances at the accident departments of patients from the practice.

The sub-survey now to be described was set up to determine whether the patients responsible for the 1,992 ' casual ' new attendances exhibited a pattern of consultations in the practice similar to that of the remainder of the practice population of 17,507. A little over 12 months after the end of the main survey period, a ten per cent systematic random sample of 199 self-referred attenders at the accident department was drawn. Those whose medical records were no longer in the possession of the practice or who had joined the practice less than 12 months before the visit to the accident department were discarded from the survey. The remaining 169 propositi were each matched with the first patient of the same age and sex who had not attended either accident department during the survey period, whose medical record was still with the practice, and who had joined the practice more than 12 months before the propositus's attendance at the hospital. All 338 medical records were studied by a qualified medical practitioner who recorded the number of surgery consultations in the 12 months preceding the attendance at the accident department and in the 12 months following, with the obvious exception of any of the propositus's consultations relevant to the accident or emergency.

Results

Figure 1 shows pair differences in the number of surgery consultations in the year preceding the sampled patient's visit to an accident department. Now although the *observed* mean difference between propositus and control was as high as 0·75 consultations a year, the t-test suggested that there was a nine per cent probability of such a discrepancy occurring when there was no *true* difference between them; or to put it another way, that the result was not statistically significant.

On the other hand, the application of the appropriate nonparametric test, Wilcoxon's, yielded a corresponding probability (or ' significance level ' as it is often called) of 0·9 per cent. This result serves to illustrate, though not of course to prove, the superiority of Wilcoxon's test in this context, as argued above.

Table 1 sets out the corresponding data for the year following the propositus's visit to an accident department, in such a way as to demonstrate the calculation of Wilcoxon's and McNemar's statistics. For although McNemar's test was ostensibly designed for binary pair differences, it is clearly possible to apply it to quantitative differences merely by considering their sign rather than their magnitude.

For example, in the present case-study McNemar's test takes into account whether the propositus or his control consulted most, but gives no more weight to a difference of 21 than to a difference of one. Now although, as table 1 shows, this makes McNemar's calculation arithmetically much simpler than Wilcoxon's (which entails summing the ranks of all controls with more consultations than the corresponding propositus, according to the instructions at the foot of the table), McNemar's neglect of an important aspect of the data makes it a less efficient test than Wilcoxon's in the context of general practice (and most other) studies.

This is not to suggest that the investigator must always prefer the more efficient Wilcoxon's test to the quicker McNemar's. Indeed, the data of table 1—where all the tests we have considered produce a significant result, Wilcoxon's at the ½ per cent level, McNemar's at one per cent and the t-test at 3½ per cent—provide an example where, with hindsight, such a choice would have paid off. On average, however, McNemar's test will detect fewer true differences than Wilcoxon's.

Observant readers will have noticed that 23 of the 169 paired observations in table 1, those for which there is no difference between propositus and control, played no part in the calculation of Wilcoxon's or McNemar's statistics. This is essentially because such pairs give us no information on the overall difference between propositi and controls. The fact that 146 of the 169 pairs contributed to the final answer is worth stressing, if only because there are other less efficient ways of summarising our quantitative pair differences so as to apply McNemar's test for binary pair differences.

TABLE 1

DIFFERENCE IN NUMBER OF SURGERY CONSULTATIONS BETWEEN A PATIENT VISITING AN ACCIDENT DEPARTMENT AND HIS MATCHED CONTROL, IN THE YEAR FOLLOWING THE VISIT

Difference in number of consultations (1)	*More consultations*			*Total* (5)	*Cumulative Total* (6)	*Rank* (7)	*Sum of 'Negative' Ranks (Controls)* (8)
	Patient (2)	*Neither* (3)	*Control* (4)				
0	—	23	—	(23)	—	—	—
1	19	—	15	34	34	17·5	262·5
2	12	—	9	21	55	45	405
3	14	—	8	22	77	66·5	532
4	14	—	6	20	97	87·5	525
5	7	—	4	11	108	103	412
6	3	—	3	6	114	111·5	334·5
7	5	—	2	7	121	118	236
8	3	—	3	6	127	124·5	373·5
9	3	—	1	4	131	129·5	129·5
10	4	—	0	4	135	133·5	0
11	1	—	2	3	138	137	274
12	0	—	1	1	139	139	139
14	1	—	1	2	141	140·5	140·5
15	1	—	0	1	142	142	0
16	0	—	1	1	143	143	143
17	1	—	0	1	144	144	0
19	0	—	1	1	145	145	145
21	1	—	0	1	146	146	0
TOTAL	89	23	57	169	—	—	4051·5(S)

Calculation of Columns (7) *and* (8)
Column (7) = Column (6) $-\frac{1}{2}$ Column (5) $+\frac{1}{2}$
Column (8) = Column (4) $\times$ Column (7)

Tests of significance

N (Total *n*umber of non-zero differences) = 146
S (Smaller of the *s*ums of like-signed ranks) = 4051·5
n (Smaller of the *n*umbers of like-signed differences) = 57

(1) Wilcoxon's test for quantitative pair differences

$$z = \frac{N(N+1)/4 - S}{\sqrt{N(N+1)(2N+1)/24}}$$

$$= \frac{146 \times 147/4 - 4051 \cdot 5}{\sqrt{146 \times 147 \times 293/24}}$$

$$= 2 \cdot 57$$

Since N is greater than 25, we can look z up in tables of the normal distribution (*Documenta Geigy* 1970). We find that the probability of this value of z occurring by chance is slightly greater than 0·5 per cent.

(2) McNemar's test for binary pair differences

$$\chi^2 = (N-2n-1)^2/N$$

$$= (146 - 2 \times 57 - 1)^2/146$$

$$= 6 \cdot 58$$

Since N is greater than 10, we can look χ^2 up in tables of the chi-squared distribution (*Documenta Geigy*, 1970). We find that the probability of this value of χ^2 occurring by chance is slightly greater than one per cent.

For example, although Whitfield (1972) examined the actual numbers of consultations by both propositi and controls, the only significance test he reported was the result of applying McNemar's test, not to whether propositus or control had consulted the more, as suggested here, but to whether propositus differed from control in the fact of consulting *at all*. This made it necessary to discard from the analysis any pair neither of whom or both of whom had consulted and to concentrate only on pairs one of whom had consulted, the other not.

Consider the effect of applying such a technique to the data of table 1. In addition to the 13 pairs neither of whom had consulted and the ten pairs each of whom had consulted on precisely the same number of occasions, it would also neglect a further 80 pairs in which propositus and control differed in their (positive) number of consultations. Now although I have already condoned the use of a less efficient test which resulted in a considerable saving in computation, there is no justification in this case for applying McNemar's test to a mere 66 observations when it is no more trouble to apply it to 146.

Furthermore, since no age-sex register can ever be completely accurate (a point discussed below), the 66 cases to be analysed are more liable to error than the 80 cases discarded.

Discussion

Before discussing the method of matched controls in relation to the problems of research in general practice, it is prudent to examine the potential disadvantages in the method *per se*, as enunciated by Billewicz (1964):

(1) In the absence of a sampling frame containing information on all the matching variables, the collection of matched controls is more difficult and time-consuming than the collection of random controls.

(2) The method is often much less efficient (in the sense in which that term has been used in this paper) than the alternative of using a random control group.

(3) It cannot be assumed that the matched pairs are comparable in any respect other than those for which they have been specifically matched.

However, the application of the method to studies of the use of general practice, as described in this paper, converts the first of these potential disadvantages to positive effect and minimises the consequences of the second. For the fact that an easily accessible age-sex register is used as a sampling frame for controls is likely to make the method administratively more convenient than alternatives; and the nature of the rates of use to be measured is such that the loss, if any, of efficiency will be small.

It is essential for the general-practice researcher to heed Billewicz's call for caution when interpreting observed differences; for these could have been caused not by the factor ostensibly under investigation, but by a fourth variable which, unlike age and sex, had been ignored by the matching process.

In general-practice research, the most likely source of such a spurious effect is the inaccuracy of the age-sex register itself. We are not concerned so much with errors in patient's addresses, as reported by Forbes (1969) and Gilmore and Caird (1971), although such errors are important if the register is to be used as a basis for surveillance or screening.

More important is the proportion of patients whose names appear on the register, whose medical records are held by the practice, but whose true allegiance is to another practice or none. Although Rees (1969) and Clarke (1971) have discussed the extent to which the total number of patients registered with the National Health Service exceeds the total population, more relevant for our purpose are estimates of list inflation in individual or groups of practices.

For example, Hannay (1972) reports an inflation of at least 7·5 per cent in the number of registrations at a Glasgow health centre; however, he has conceded that a considerable proportion of these errors are attributable to discrepancies between his computerised age-sex registers and the executive council's own records, a problem discussed in detail by Farmer *et al.* (1974). Plans are afoot to computerise family practitioner committee records and thus eliminate these discrepancies by providing practice-registers direct from the same computer (Rivett, 1975). While this will serve to reduce inflation in age-sex registers, it is clear from the national studies mentioned above and from Munro and Ratoff (1973), who reported an inflation of ten per cent despite a disagreement of less than one per cent with the executive council, that some degree of inflation is inevitable.

Thus the method of matched controls, as implemented for example by Whitfield (1972), carries with it some risk of generating spurious results. For although each control has something like a ten per cent chance of being a ' dead record ' with a consultation rate of zero, the propositi will usually be so defined as to guarantee that their registration with the practice is genuine. For example, in our case-study, each of the propositi had acknowledged that he belonged to the practice in question when treated at the accident department.

Fortunately it is easy to put an upper limit on the effect of this bias by repeating the significance test after eliminating any pairs for whom either the propositus or the control did not consult during the entire period under review. Since a fair proportion of the patients genuinely registered with the practice will fail to consult within this period of time, this technique is likely to overestimate the effect of inflation; the true significance level will probably lie somewhere between the two calculated values.

When this check is applied to table 1 it is found that there are 19 controls and 11 propositi who failed to consult a general practitioner during the two years under review, i.e. 12 months on either side of the date of the propositus's accident. Since Wilcoxon's test applied to the remaining 116 pairs still produces a significance level of three per cent compared with the original ½ per cent, we can be reasonably confident that the difference in mean consultation rate cannot be attributed to inaccuracies in the register.

Thus we are able to convince ourselves that the implication of the case-study—that patients who use accident and emergency departments are also high users of general practice—was not the spurious effect of list inflation. However, it was less easy to be sure that it was not, in part at least, the product of the correlation between use and distance recorded by Morrell *et al.* (1970) and discussed by Glass (1974). For this reason the evidence of the case-study was regarded as a hypothesis to be tested in a more thorough epidemiological study, the results of which have been reported elsewhere (Russell and Holohan, 1974).

For all that, the method of matched controls, as described in this paper, is conceptually and computationally much easier than other methods. Consequently it allows the investigator to concentrate on the specific difficulties inherent in research in general practice rather than on higher mathematics. For Kalton (1968) has pointed out that, although general-practice records have unique advantages as a source of statistics on the use of the National Health Service, they have yet to realise their full potential because too little attention has been paid to the numerous methodological problems.

Now, despite the potential dangers discussed, the method can be used to ameliorate some of the problems raised by Kalton (1968). First it provides a means of carrying out retrospective studies, under the assumption that the medical records of propositi and controls will be equally ' sketchy '.

Secondly, it copes with the problem of a transient population, analysed in more depth by Lees and Cooper (1963), provided that propositi and controls are treated equivalently. Thirdly, it even goes some way towards meeting Kalton's criticism of single-practice studies, since propositus and control share the same (potentially unrepresentative) practitioner.

However, it must be conceded that the method is irrelevant to the major problem of method in general-practice, that of achieving appropriate representation in multi-practice studies (Kalton, 1972). For although the appropriate sampling methods have been outlined by Bevan and Draper (1965), the complementary snags of self-selection and non-response remain.

To summarise, the method of matched controls provides a quick and easy means of checking for associations in the use of general practice. Although care is needed in the interpretation of results, the method should suffice as an aid to practice management.

If, however, the investigator is concerned that the results should be both epidemiologically sound and replicable on a wider scale, the method should be used only to generate hypotheses to be subjected to subsequent rigorous testing.

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PRIORITIES FOR GENERAL PRACTICE

. . . " In summarising it may be interesting to define the priorities for general practice in Europe. I found it depressing that in all the countries we seem to be very reluctant to learn from each other. It seems that we insist nationally on experiencing the pangs and pains of birth in establishing general practice, without learning from the inevitable mistakes, nor the improved and less painful methods of delivery.

" A national professional organisation of General Practitioners for General Practice (is needed), with the specific aim of developing the academic content and functions of general practice, including research in and into general practice. This is essential to hurry the establishment of general practice as a respected and acceptable member of the medical family. This may be achieved all the more quickly if political—financial objectives for general practice are handled by different professional organisations. . . . All this will come to nought if at the same time we cannot persuade governments, politicians, parliaments, and the public that general practice itself is studying and analysing the delivery of care, particularly in cost effectiveness. No country can today watch the rapidly escalating costs of primary medical care, without political parties attempting some radical interference which may well be destructive to general practice as we see it.

" I believe here lies the programme of the International Society of General Practice, I fear it may well turn into a rescue operation for general practice."

REFERENCE

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