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Sampling events in space and time: a case study of burn injuries

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We describe the sampling plan used to estimate the number of burn injuries seen at emergency departments of hospitals in New England. We present the rationale for each of the options considered and the implications of each. The chosen plan included all 256 hospitals, but used a different systematic one-day-in-ten sample for each of ten subgroupings of the hospitals. The findings suggest that over 47 000 injuries or almost 400 per 100 000 population were seen at New England Emergency Departments in a 1-year period. This incidence is 60 per cent higher than pre-project estimates would have suggested. From the variability of the data in the sample, we calculate that our estimate is probably no more than 3 per cent under or over the 'correct' rate that would have been obtained by a full non-sampling approach, costing almost 10 times as much. Additional support for our strong belief in the accuracy of the sample-based estimate is provided by the fact that if we had used the same sampling approach to estimate the number of hospitalized *burn injuries, our estimate would have been in error by only 124 or 4 per cent from the total 3276 obtained by 100 per cent sampling of hospitalization records.*

Introduction

How many péople receive hospital treatment for burn injuries? Who are these victims and what is the nature and extent of their injuries? How much medical care do they consume? Where and how should facilities for the treatment of burn injuries be situated? The Bureau of Medical Services of the Health Services Administration of HEW was charged with assembling information of this kind for Congressional hearings on the oganization of burn care. Lack of sufficiently reliable data prompted a special prospective survey, which was carried out in the period May 1978 to July 1980 in six 'sites' in the USA (*Table I*).

Table I. Sites participating in survey

Site	*Population base*
1. New England States	12 million
2. Central NY State	3 million*
3. Virginia	5 million*
4. Alabama	3 million*
5. Texas	3 million*
6. Southern California	2 million*

*Approximate only, based on estimated catchment area.

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The main objective of the study was to gather comprehensive victim-specific data on the occurrence, distribution, severity and cost of the burn injuries presenting to the hospitals in these six sites during this period; the study was aimed at both those who were hospitalized and those who were simply treated and released (T&R) at emergency departments. A secondary objective was to demonstrate the feasibility of multisite participation in a comprehensive integrated data collection system as a means of assessing major health care problems of this kind.

For injuries requiring *hospitalization*, each site undertook to report every case; thus, apart from possible incomplete reporting, the number of hospitalized cases represents a 'census' for the study period. For T&R cases, five of the six sites undertook a census; the sixth site, representing the entire six-state New England region, adopted a sampling approach whereby only a fraction of the cases was ascertained and reported on. The reasons for this were both scientific and economic: it was anticipated that complete enumeration of T&R cases in New England might approach 30 000 annually; however the gain in information from a census of such cases, over what could be obtained from just a sample, would be marginal at best, while the additional cost would be considerable. Moreover, compared with hospitalized cases, the extent and detail of the information available in hospital records for T&R cases was much less. In addition, many of the T&R injuries involve only a single emergency department visit and vary less in nature or cost; from the viewpoint of demand for care, the single most important item of information regarding these injuries is *how many* there are.

In this paper we present the sampling plan used to survey T&R cases in the New England site, and show how the data collected from this sample can be used to infer quite accurately or estimate what the count of the total number of T&R cases would have been, had a complete enumeration been attempted. We also present what the results would have been if this sampling scheme had also been applied to hospitalized injuries. In this latter group of patients we actually carried out a census, and so we can indirectly check on the soundness of the sampling plan by examining how accurate the estimates derived from the sample would have been. Our objectives in presenting the sampling plan are threefold: (i) to emphasize, in general, the remarkable power and economy of sampling; (ii) to share with readers what we believe to be a somewhat unique approach to sampling when the units of interest are events scattered in place and time; (iii) to show that the possible errors in our sample-based estimates can be quantified. We also discuss briefly two other sampling schemes which were considered, but discarded in favour of this method. (The Ohio Trauma Study (Barancik et al., 1983), facing a similar task, used another sampling approach but did not assess the possible sampling variations.)

Methods

Sampling fraction

The approximate fraction of T&R cases to be sampled was chosen so as to be small enough to make the cost savings worthwhile yet large enough to estimate reliably both the number of cases and the relative proportions of each type (aetiology) of injury. Earlier data from a 3-year survey of burn injuries in the emergency rooms of seven Boston hospitals had suggested that electrical burns were the least frequent type, occurring with a relative frequency of 1.6 per cent. We were willing to take a 95 per cent guarantee that the relative error in our estimate of this frequency would not be more than 30 per cent in either direction, and so calculated n from the binomial formula

$$0.3 \times 0.016 = 1.96 \times \sqrt{0.016 \times 0.984/n} \qquad (1)$$

This suggested that a sample or denominator of approximately $n = 2625$ would be required in our study in order to estimate a relative frequency this small with the desired reliability. This size sample would allow the relative frequency of other more common types of T&R injuries to be measured with even greater precision, as one can see by substituting values larger than $P = 0.016$ into equation (1).

In order to translate the $n = 2625$ into a sampling fraction, we needed an estimate of the number of T&R burn injuries that would be uncovered by a year-long census. While readers will recognize the circularity in that this was the very data-item that the study was designed to produce, we had to 'start somewhere'. The best conventional wisdom was that this number was in the neighbourhood of 30 000, so that if we rounded the $n = 2625$ to a more conservative $n = 3000$, we needed a 1 in 10 sample of T&R cases*. We considered three approaches to sampling; we will outline two of them and describe the third in some detail.

Sampling approaches considered

The three possible compositions of the sample were:

1. *Every* T&R burn injury in each of a *sample* of the *communities* of New England.
2. *Every* T&R burn injury in each of a *sample* of the *hospitals* in New England.
3. A *sample* of the T&R *burn injuries* from each and *every hospital* in New England.

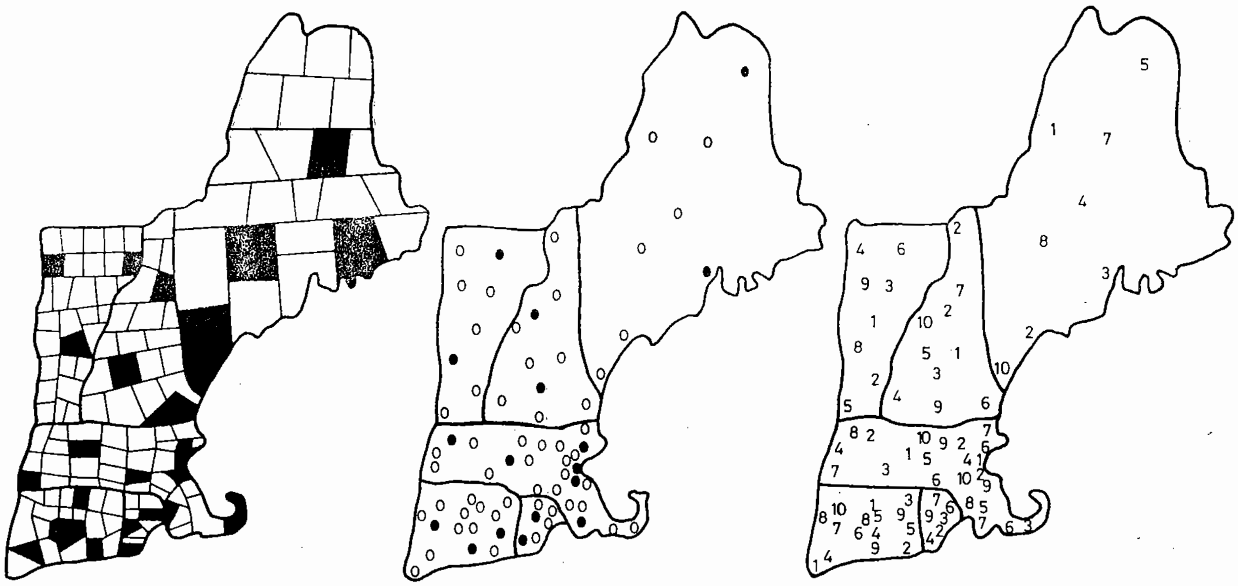
These are depicted schematically in *Figure 1a–c*. Approach 1 would have had the advantage that the New England region could be easily subdivided into communities; readily available demographic information would allow these to be grouped or stratified on the basis of age composition, size, population density, geographical features and other factors thought to influence burn incidence and aetiology. A sample of communities could then be drawn from each stratum, and the results, based on known denominators, could be easily extrapolated back to the entire New England region. The chief drawback of this approach was the difficulty in compiling the numerators, since victims who live in or are injured in a sample community might well receive treatment in a non-sample community. Thus considerable effort would be involved in finding such T&R cases; moreover, if we were to use residence or place of work, we would also have had to screen out residents of some non-sample communities from the T&R cases seen in hospitals in the sample communities. A second drawback was that large unexplainable variations in burn-injury rates between communities of even apparently similar composition (such as those found in the earlier mentioned survey of Quincy and Lynn) might cause the estimates derived from a sample of communities to be quite imprecise or 'far from the mark'.

Approach 2 would make the hospital the prime sampling unit. It would stratify the region's hospitals according to factors such as size and geography and enumerate all T&R burn injuries in a stratified sample of these hospitals regardless of the residence of the victim or the place where the injury occurred. Unfortunately, while this sampling would have provided good numerators, one could not use them to establish population-based incidence rates, especially for *subsets* of the population: the population served by each hospital could not be properly defined, and might well vary depending on the type of injury, where it occurred, the time of the week and the season of the year. Again, the estimate of the total T&R injuries for New England could vary considerably depending on which hospitals were chosen; there would be no guarantee that, even if they were chosen in a stratified manner, a set of hospitals which accounted for 10 per cent of the total hospital beds or 10 per cent of the total emergency department volume for the preceding year would see 10 per cent, or even close to it, of the total T&R burn injuries in New England.

Approach 3, the one finally adopted, combined the notion of sampling in space, embodied in approaches 1 and 2, with the notion of sampling in time. For each hospital, a systematic 1 in 10 sample of the project days was chosen, i.e. for some hospitals, those T&R burn injuries treated on days 1, 11, 21, ... of the project period were to be included in the sample, for others those occurring on days 2, 12, 22, ... etc., and for a 10th and final group of hospitals, those occurring on days 10, 20, 30, ... This allowed every hospital to be included in the sample and made the question of 'constituencies' a moot one. Furthermore, by concentrating only on those T&R burn injuries occurring on certain chosen sample days, it established well-defined time boundaries, so that data accessors concentrated on just those days, and neither they nor the study analysts had to worry about incomplete numerators or denominators. This systematic sampling of days had two additional advantages: (i) it produced a sample of days which was effectively stratified by season and by day of the week, something that an unrestricted random sample could not be relied on to do, and (ii) it spread the data-accession workload evenly over time and space.

The method of deciding which hospitals would have

*As will be presented later, the 'guesstimate' of 30 000 was in fact a gross underestimate, since the true number is somewhere nearer 48 000. While this underestimate highlights the danger of relying on guesstimation rather than estimation, it did not compromise the sampling plan in any way.



a b c

Figure 1. Three possible sampling strategies. **a**, A sample of communities: the same selected communities (solid areas) are studied for the entire project period. **b**, A sample of hospitals: the same hospitals (solid circles) are studied for the entire period. **c**, A sample of hospital-days; different hospitals are in sample on different days; 1 = days 1, 21, 31, ...; 2 = days 2, 12, 22, ..., etc.

Bed size / Project days	1,11,21,...	2,12,22,...	3,13,23,...	4,14,24,...	5,15,25,...	6,16,26,...	7,17,27,...	8,18,28,...	9,19,29,...	10,20,30,...
500+	CT	CT	CT	CT	ME	MA	MA	MA	MA	MA
	MA	RI	VT ↘							
300–499				CT	CT	CT	CT	CT	CT	CT
	CT	CT	CT	CT	CT	CT	ME	ME	MA	MA
	MA	MA	MA	MA	MA	MA	MA	MA	MA	MA
	MA	MA	MA	MA	MA	MA	MA	MA	MA	MA
	NH	RI	RI	RI	RI	VT ↘				
100–299							CT	CT	CT	CT
	CT	CT	CT	CT	CT	CT	CT	CT	ME	ME
	ME	ME	ME	ME	ME	ME	ME	ME	MA	MA
					⋮					
	MA	MA	MA	MA	MA	MA	MA	MA	MA	NH
	NH	NH	NH	NH	NH	NH	NH	NH	NH	NH
	RI	RI	RI	RI	RI	RI	VT	VT	VT	VT
	VT ↘									
under 100		CT	CT	CT	CT	CT	CT	ME	ME	ME
					⋮					
	ME	ME	ME	ME	ME	MA	MA	MA	MA	MA
	MA	MA	MA	MA	MA	MA	MA	MA	MA	MA
	NH	NH	NH	NH	NH	NH	NH	NH	NH	NH
	NH	NH	NH	NH	NH	RI	VT	VT	VT	VT
	VT	VT	VT	VT	VT	VT				

Figure 2. Allocation of sample-days to 256 hospitals, grouped according to bed size and state.
CT = Connecticut; MA = Massachusetts; ME = Maine; NH = New Hampshire; RI = Rhode Island; VT = Vermont

T&R injuries from project days 1, 11, 21, ... included in the sample, which ones would have days 2, 12, 22, ... and so on was as follows (see also *Figure 2*):

1. The 256 New England hospitals were sorted into four categories on the basis of bed size, to serve, to a first approximation, as a proxy for anticipated volume of T&R burn injuries. Each of the four separate subgroups was then further sorted by state (alphabetically, with Connecticut hospitals first, Vermont last); within each state, hospitals of the same size were arranged in essentially random order.
2. Beginning with the first state, the first hospital with over 500 beds in the state was to have days 1, 11, 22, ... in its sample, the second such hospital 'received' days 2, 12, 22, ... and so on until all of the largest hospitals in that state were exhausted. Beginning from where the last assignment ended, the largest hospitals from the second state were assigned to successive sets of days until all of these hospitals in this state were exhausted. This process was continued until all of the largest hospitals in all six states had been given a starting day.
3. Beginning again from the last assignment, the first hospital with 300–499 beds in the first state was assigned the next available starting day, the second such hospital the succeeding day and so on until, in a manner analogous to step 2, all hospitals in the 300–499-bed category in all six states had been assigned starting days. This process was then repeated for hospitals with 100–299 and under 100 respectively until all 256 hospitals had been assigned starting days.
4. The assignments were then reviewed and adjusted where necessary to ensure that within each size category, no two hospitals in the same geographical area were assigned to the same sampling days. By this method, a reasonable geographical spread was achieved, while the total number of hospitals of each size sampled each day was not allowed to vary by more than one. The assignments are summarized in *Table II*.
5. The project days 1, 2, 3, ... were translated into calendar dates with day 1 corresponding to 1 May 1978, day 2 to 2 May, and so on.

The main advantage of this sample of both time and place was the wide net used to produce the sample: all hospitals were included; the sample spanned the entire year and was well distributed geographically, making it a more remote

Table II. Number of hospitals of various sizes assigned to the different sets of sampling days

Hospital size (beds)	*Project days* 1,11,..	2,12,..	3,13,..	4,14,..	5,15,..	6,16,..	7,17,..	8,18,..	9,19,..	10,20,..	*Total*
500	1	2	2	1	1	1	1	1	1	1	12
300–499	4	4	4	5	5	5	5	4	4	4	44
100–299	11	11	11	11	11	11	10	10	10	10	106
<100	10	10	8	9	9	9	10	10	10	9	94
Total	26	27	25	26	26	26	26	25	25	24	256

Table III. Number of weekdays and weekend days in each set of 36 days for the project period 31 May 1978 to 25 May 1979*

Hospitals assigned to days	*No. of weekdays*					*No. of weekend days*				
	June–Aug.	*Sept.–Nov.*	*Dec.–Feb.*	*Mar.–May*	*Total*	*June–Aug.*	*Sept.–Nov.*	*Dec.–Feb.*	*Mar.–May*	*Total*
1,11,..	7	7	6	7	27	2	2	3	2	9
2,12,..	8	6	7	6	27	2	3	2	2	9
3,13,..	8	6	6	6	26	2	3	3	2	10
4,14,..	7	8	6	5	26	2	2	3	3	10
5,15,..	7	6	7	6	26	2	3	2	3	10
6,16,..	7	6	6	7	26	2	3	3	2	10
7,17,..	8	7	6	6	27	1	2	3	3	9
8,18,..	8	7	7	6	28	1	2	2	3	8
9,19,..	7	6	7	7	27	2	3	2	2	9
10,20,..	7	6	6	7	26	2	3	3	2	10
Total	74	65	64	63	266	18	26	26	24	94

*Day 1 = 31 May 1978; day 2 = 1 June 1978; ...; day 360 = 25 May 1979.

Table IV. Number of T&R burn injury cases reported by 10 different groups of hospitals for the 10 different sets of sample days

Hospitals assigned to days	*Project days* 1,11,..	2,12,..	3,13,..	4,14,..	5,15,..	6,16,..	7,17,..	8,18,..	9,19,..	10,20,..	*Total*
1,11,..	450										
2,12,..		490									
3,13,..			462								
4,14,..				461							
5,15,..					496						
6,16,..						467					
7,17,..							470				
8,18,..								432			
9,19,..									491		
10,20,..										488	
Total											4707

possibility that short-term or localized peaks in burn injuries (e.g. caused by unusually warm or cold weather, or by isolated mishaps involving a large group of victims) would unduly inflate the estimate of the annual figure. The fact that the sampling was every 10 days, rather than say every 7 or 8 or 20, was fortuitous as it meant that weekdays and weekends were properly represented for each hospital for each season (*Table III*). Other advantages included the ability to calculate bounds on the percentage by which the estimates derived from the sample were likely to over- or underestimate the 'true' census count, and to use exactly the same methods to estimate annual incidence rates for the various aetiological, geographical and demographic subgroups.

Results

T&R injuries 31 May 1978 to 25 May 1979

Although the sample survey of T&R burns covered the 16-month period 1 May 1978 to 31 August 1979, we present here the results for a first 'year' consisting of 36 cycles or 360 days from 31 May 1978 to 25 May 1979. This reduces the possibility of start-up errors for the first three cycles; also, since these results were compiled before all final edit checks had taken place, we excluded the final months of June to August 1979. In the $256 \times 36 = 9216$ 'hospital-days' which constituted the sample, some 4707 T&R cases were counted (*Table IV*). Since the sample of 9216 hospital-days represents only 10 per cent of the entire 92 160 hospital-days in the period, our *best estimate* of the total number of T&R burn injuries for the period is 4707×10 or 47 070. Considering New England's population of approximately 12 million, this represents an estimated 'incidence' of such cases of approximately 392 per 100 000 person-years in the New England region.

Since we do not really know what happened on the other 82 944 hospital-days, our estimate of 47 070 for the entire region for the entire period is just that – *an estimate* – and it would clearly have been different had the allocation of sampling days been different. The extent to which the different possible estimates would have over- or underestimated the correct total is usually described by the standard deviation of all such estimates and is called the standard error (s.e.) of the estimate. Clearly we would need to know the number of T&R cases for *every* single day for every hospital (again something the sample is designed to avoid) in order to calculate this quantity precisely. However, we can approximate the s.e. using the amount of variation seen just in the *sampled* days in each hospital. A number of assumptions, all of them likely to yield a conservative answer, were needed to perform this calculation which yielded an s.e. of 730 cases; thus it is quite likely that the estimated total of 47 070 is no more than 2 × 730 or 1460 cases above or below the true (but unknowable) total for the year. Expressed in percentage terms, this possible under- or overestimation or 'sampling error' is probably not more than 100 × (1460/47 070) or 3.1 per cent.

Similar computations were performed for the various aetiological subtypes, and the results are given in *Table V*. The uncertainty about these estimates is greater than it was for the total, because each subtype occurs less frequently. Although these data are not final, they suggest clear rankings of the types and causes of burn injuries presenting annually to New England emergency departments. Further epidemiological analysis is underway to pinpoint the subgroups of the population that contribute most to these statistics.

Table V. Observed sample quantities and projected totals for various aetiological subtypes

Aetiology	*Observed number*	*Estimated total*	*Standard error (%)*	*95% Confidence interval*
Chemical	752	7520	4.0	6930–8109
Electrical	76	760	10.9	598–922
Cold	29	290	18.8	183–397
Flame	403	4030	4.8	3651–4409
Hot surface	815	8150	3.5	7591–8709
Flash	186	1860	7.2	1597–2122
Scald/steam	178	1780	7.2	1529–2031
Scald/liquid	987	9870	3.0	9290–10 450
Scald/grease	397	3970	4.8	3596–4343
Radiation	553	5530	5.3	4955–6104
Other/ unknown	331	3310	5.3	2966–3654

How precisely would this sampling scheme have estimated the total number of hospitalized burn injuries?

For hospitalized cases, the survey included *every day* for every hospital. We can, however, determine what our *estimate* of the total number of such cases *would have been* if we had merely sampled, *using the same sample of hospital-days used in the survey of T&R cases*. The results are presented in *Table VI*, with the 10 different columns showing how many persons were admitted on the 36 project-days 1, 11, 21, . . ., how many in the 36 days 2, 12, 22, . . . etc. Had each hospital been assigned just one of these sets of days, only the counts shown in the diagonal of *Table VI* would have been observed, yielding a total sample of 340. Upon projecting this 10 per cent sample up to 100 per cent of the hospital-days, we would have estimated the overall total to be 10 × 340 = 3400. In fact one can see that the 'true' reported total was 3276 so that the relative overestimation was 124/3276 or 3.8 per cent, a remarkably good estimate based on only 10 per cent effort! In order to avoid any suggestion that this particular sample of hospital-days was just a 'lucky' one, we have computed what the possible over- or underestimates would have been if the sample had been different. To do this we randomly rearranged the order of the hospitals shown within each size-state group in *Figure 2*; we did this 500 times, each time producing different groups of hospitals (rows) for the 10 different sets of days (columns). The 500 different estimates derived from these are presented in histogram form in *Figure 3*, and show that no matter which random order the hospitals had been in before they were assigned to the different sampling days, it is very unlikely that the resulting estimates would have been more than 15 per cent in error; in fact in *90 per cent* of the 500 possible samples, the percentage error was less than 9 per cent. A further 500 simulations showed that if we had used a random ordering of the six states, rather than the obvious alphabetical one actually used, we would have achieved the same narrow spread of estimates. Moreover, had we built into these simulations the same kinds of adjustments which we actually made to keep the hospitals sampling on any one day geographically distant from one another, the possibility of a serious over- or underestimate would be even less. These results provide strong reassurance that the estimate of 47 070 for the much more frequent T&R cases (more than 14 times as frequent as admissions) is probably more accurate

Table VI. Number of hospitalized burn injury cases reported by 10 different groups of hospitals for each of 10 different sets of project days in the 360-day period 31 May 1978 to 25 May 1979*

Hospitals which sampled T&R injuries on days	*Project days**										
	1,11,..	*2,12,..*	*3,13,..*	*4,14,..*	*5,15,..*	*6,16,..*	*7,17,..*	*8,18,..*	*9,19,..*	*10,20,..*	*Total*
1,11,..	32	38	37	43	25	41	41	24	37	37	355
2,12,..	40	43	37	39	45	36	36	35	40	38	389
3,13,..	47	42	39	49	48	39	39	51	41	44	439
4,14,..	32	22	25	13	22	30	23	26	36	26	255
5,15,..	34	36	33	38	40	37	26	46	45	35	370
6,16,..	38	35	34	30	29	37	29	34	26	28	320
7,17,..	30	41	34	25	19	29	37	37	21	40	313
8,18,..	24	20	27	27	25	30	20	26	27	24	250
9,19,..	24	16	30	37	35	25	30	27	33	27	284
10,20,..	32	34	27	28	30	29	36	26	19	40	301
Total	333	327	323	329	318	333	317	332	325	339	3276
Diagonal total											340

*Day 1 = 31 May 1978; day 2 = 1 June 1978; ...; day 360 = 25 May 1979.

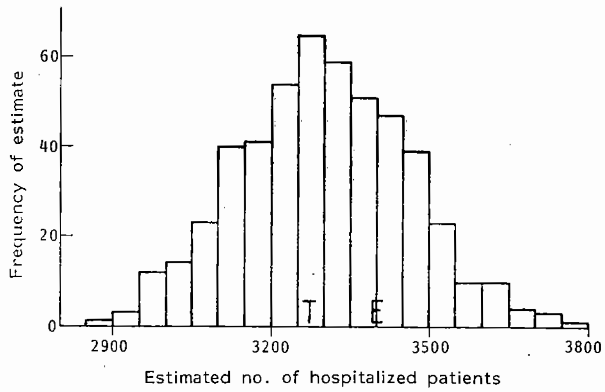


Figure 3. Distribution of 500 'estimates' of total admissions obtained by randomly rearranging the allocation of hospital days. T, 'true' census figure of 3276; E, estimate of 3400 produced by sampling plan used for T&R patients.

than our confidence interval of ±3.1 per cent would suggest.

Discussion

The planning of health services must be based on firm accurate data. However, 'guesstimates' of the demand for services are often far from the mark (for example we had anticipated only 30 000, or roughly 65 per cent of the 47 070 T&R injuries in New England) and one is often forced to carry out special-purpose surveys to obtain accurate information. We believe that the power and economy of a *sample* survey as a method of gathering such data are greatly underrated, and hope that this case history will help sampling become more acceptable and trusted. The reasons for the distrust of samples are many, the two main ones probably being: (i) the fear that the estimates produced will be seriously 'wrong', and (ii) the helplessness felt when one is provided with probability or confidence statements which are guaranteed only 'in the long run'; 95 per cent of *all* the 95 per cent confidence limits that a statistician gives to different clients can be expected to be correct, but the one-time client cannot really check the *one* he receives. However, both of these fears can be allayed, first by careful yet commonsense attention to the variables that affect the quantity being measured and second by taking a sufficiently large sample that even the worst possible 'error' is both tolerable and highly unlikely.

In contrast, the blind trust in a complete census is often misplaced, in that paradoxically there are often greater sources of (non-sampling) error in a census than in a sample: the much larger task means more people to train and supervise, more opportunity for inconsistencies, fatigue, carelessness and undercoverage and, unlike the sample, much less time to recheck each measurement. These sources of bias are often far more vicious than the unpredictable, but at least neutral, probability mechanism used to generate a sample. The recent law-suits brought by a number of US cities against the Census Bureau for allegedly undercounting their populations in the 1980 Census emphasize the fact that, if the Constitution permitted it, these populations could be far more accurately estimated from a sample of households. Similar underreporting, and more seriously differential underreporting by subtype of case, has been a serious limitation of some medical registries (Demlo et al., 1978; Goldberg et al., 1980) and data banks. (Incidentally, our 'census' was not perfect either: quality control checks by an outside agency during the demonstration project revealed that the census probably missed some 2–3 per cent of the 'true' total number of burn injuries.)

Obviously we do not advocate a sampling approach in every survey; there are many situations, such as in certain cancers, high risk infants, special cohort studies, etc., where the wider scope of a sample is less important or less practical than a concentrated approach. However, in assessing the demand for medical care, particularly in relation to events or presentations with both a geographical and a temporal component involving a large constituency, we believe that a sampling approach such as we have taken can be a very accurate and cost-effective method. In an era where the labour-intensive costs of obtaining good planning information are rapidly rising, we see the possible gains from a sample rather than a census as the difference between being approximately correct and precisely wrong.

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