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MORTALITY MODELS AND THEIR USES IN DEMOGRAPHY

an address by

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The choice of a subject for my talk came about in this way. I was supervising a project by a student of mathematical statistics at University College who, at the same time, was sitting actuarial examinations. His topic was connected with life tables but on discussing it with him, I failed to follow what he was saying. Even when I understood the technical language, I was not at all sure what the objectives were. The lesson I learned was that the ways actuaries and demographers look at the same subjects may be quite different. Although I would not dare to comment on the value of the separate approaches to this particular audience I thought it might be useful to speak about how demographers, over the past few years, have been trying to use life tables, particularly in the form of mortality models.

Mortality models are a system of schedules which describe the variations in the force of mortality by sex and age in terms of a limited number of disposable constants (or degrees of freedom, or parameters). The resulting rigid framework of the possible structures of life tables can be applied to a number of purposes. The demographic uses of these kinds of system although wide are primarily of two broad classes. The first is estimation from limited information. For about 80% of the population of the world information about mortality is exceedingly scanty and those demographers who are mainly concerned with developing countries have to attempt to estimate life tables from little data. The function of the models is to substitute for what we do not know. The uses are, in fact, entirely comparable to those of statististical distributions, except for the important distinction that normally, in the latter case, the model makes up for the vagueness due to chance factors whereas, in dealing with developing countries, the problem is errors of bias and not of chance. Many of the applications are indirect. For example, the estimation of mortality from survivorship between successive censuses, from the proportions of children alive by age of mothers, or of fathers and mothers alive by age of children. The models play a major part in estimation problems of this type.[4]

The second kind of application is for the projection or forecasting of the mortality of the future. In a sense this can also be described as a limited data problem because in all countries the future is something about which we know little. Forecasting is a tenuous extrapolation from past experience and there is a restricted base on which it can be done even in populations with good data. The major examples of such projections are the United Nations population forecasts[14] which are all, in one way or another, based on " demographic " mortality models.

For both of these broad purposes, the model system must be a simple one, in the sense th it can be specified by a small number of parameters because in neither case is there much of a data base for estimation. Elaborate models requiring many constants to be determined are useless. This rules out the whole range of attempts to express mortality as a mathematical function of age from Gompertz onwards. These include the increasingly more complex functions of probabilities of dying by age that stem from Gompertz,[1] and the rather different methods of Karl Pearson[11] where the curve of deaths is described by a series of overlapping normal distributions, centred in childhood, middle-adulthood and old age respectively. All these relations require too many parameters to define them adequately for the purposes here.

About twenty years ago, demographers turned to what might be called " family relation " methods. These are essentially systems derived from the internal relations between the forces of mortality. That is, the general shape of mortality by age is taken as empirically determined and concentration is on the variations among populations and over time at given ages rather than the form of the variations with age itself. The pioneering system was the United Nations model tables, originating in ideas of Professor Valaoras who is, in fact, neither an actuary nor a demographer but a physician. The United Nations model life table system[12] is essentially an empirical construction from the available observations on mortality by age calculated by a procedure of statistical regression which is not of any special interest. In effect, they are simply averages of mortality patterns by age at different levels. This gives a one parameter set, a " grid " of life tables over different levels, which can be specified in a number of ways, but for each level of mortality there is only one unique pattern by age.

These model life tables, which were published in 1955, are still widely used but in time they were found to be too limited and they had technical defects as well. After that, around 1965, Coale and Demeny[6] produced another set of life tables, based on essentially similar ideas, but they divided the known life tables (those they were willing to accept as reasonably accurate) into four separate groups within which, according to their calculations, the age patterns were more homogeneous than overall. They then constructed four one-parameter sets of life tables. These Coale-Demeny Regional life tables, as they are called, are probably the most widely applied of the demographic model life table systems. However, they are also subject to many of the criticisms which have been directed at the United Nations tables, most forcibly that their range of variation is too restricted. In a broad way they describe the average patterns of mortality on which they were based but the data come almost entirely from relatively recent Western experience. Excluded are many environments, particularly of older times or in developing countries, for which it is highly doubtful if the patterns were similar.

The work of Ledermann[9] is of a different nature. He first undertook a theoretical investigation to discover how many parameters were needed (how many degrees of variation) to explain the relationships among known life tables adequately. He concluded (his methods are not beyond criticism, but by and large are on a sound basis) by forms of multivariate statistical analysis, that something like five degrees of freedom were required. In other words, five disposable constants could describe rather accurately the way mortality patterns varied in different populations.[13] He found it much more difficult to produce a workable system based on five parameters and the Ledermann model life tables[10] which were published in France are rather disappointing. Although the theoretical basis implied some five parameters, the calculated tables are not very much different from the existing sets. Most of them are in fact one-parameter although certain sets are designed for a double-entry system, that is, with two degrees of variation. These are not easy to use in practice.

Other very similar systems have been proposed but this brief account covers the empirically derived model life tables which have been widely used in demography. This talk, however, is mainly concerned with a slightly different kind of system in which an empirically expressed relationship between mortality and age is converted by an explicit mathematical function into other relations which can serve as a set of model life tables. The method can be presented in a number of ways. It is notable that this kind of system has recently also become of considerable interest to statisticians analysing survival curves. Suppose there is a transformation of scale which changes one particular pattern of life table into a simple function of age and all other life tables then follow the same function (but with different parameters). A simple mathematical relationship between these life table mortality schedules therefore exists on this transformed scale. The idea can be easily illustrated by the oldest system of this category, that is the one due to Kermack, McKendrick and McKinlay.[8] Some forty years ago, these authors and, independently, Derrick of the Government Actuary's Department, proposed the use of a relation which with a slight amendment can be expressed in the following form. Assume there is some transformation of the age scale x into y (and it does not matter what this transformation is) which turns l_y (the survivorship in the life table to the corresponding point y) into a function e^{-ay}. Of course there always is a transformation of x into y to give this relationship for one mortality schedule with any specified a but the assumption is that the same change of scale turns all other life tables into functions of this form but with different values of a. Then $\log l_x/\log l_x' =$ constant, where l_x and l_x' are for two life tables and the ratio of the forces of mortality at the same ages in the two mortality schedules is constant. Kermack, McKendrick and McKinlay derived the relation for age specific death rates rather than forces of mortality and applied it in their classic work on mortality projection by generations. Thus from the known mortality up to the current age for a given generation the ratios of forces of mortality to those of previous generations can be estimated and extrapolated to later ages. Future mortality is then derived from the ratios and the known specific death rates of earlier generations.

The idea was not followed up at that time and was criticized for the wrong reasons, partly because of confusion with another theory that will be discussed later, namely, that a generation carried its own mortality with it. The valid criticism was essentially an empirical one that, in fact, life tables did not behave as assumed, that the relationship between the forces of mortality over age was not constant but varied. The correct conclusion from this, that the wrong transformation function was used, did not seem to be drawn.

For a number of years I have been interested in the study of life table systems with more complicated conversion-type functions. If the one considered is called an exponential-type system, then primarily these are probit or logit systems. The two are closely similar to each other. The idea is most easily explained by reference to the diagrams opposite. A and B in Fig. 1 are curves of death or probabilities of dying at different ages in two life tables. Suppose there is a transformation of the ages to some scale y which turns the curves of deaths into symmetrical peaked distributions of the shapes illustrated in Fig. 2. On the probit system they are Normal or Gaussian curves. The logit system leads to a distribution which is more complicatedly expressed but which has much the same shape. Then on the transformed scale of y, the model relation gives all life tables as two-parameter curves specified by their central value and by the breadth of the distribution (the standard deviation in statistical terms). In other words, on the transformed scale the curves of death have exactly the same shape, but a varying location and a varying spread and

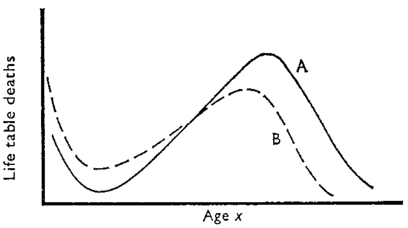

Fig. 1

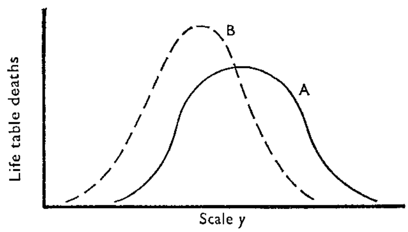

Fig. 2

therefore they are a two-parameter system. As in the exponential-type conversion function of Kermack, McKendrick and McKinlay, there is a simple expression for l_y on the logit system, $l_y = \dfrac{1}{1+Ce^{ky}}$ where C and k are the two disposable constants. The relation gives not just one model mortality system, but many because it is a transformation of life tables. Starting with any initial mortality schedule others can be generated from it by varying the two parameters. The simplest way of expressing this is in the form,

$$\text{logit}\,(1-l_x) = \alpha+\beta\,\text{logit}\,(1-l_x')$$

where the l_x and the l_x' are the proportions surviving to age x (and $(1-l_x)$ the proportion dying) in the generated and initial life table with α and β constants. The logit of P is $\frac{1}{2}\log_e P/(1-P)$ and is tabulated in the Fisher and Yates' statistical tables.[7]

The justification for the relation is, of course, entirely empirical, and a large number of comparisons of the system with observations have been made. Recently, Carrier and Hobcraft[5] have published a monograph in which life tables of the system are tabulated, with many derived measures and their use for various applications described. This is not the place for a long, detailed examination of the accuracy of the approach. What can be said is that it works remarkably well for broad descriptive purposes, in reproducing how mortality varies between populations and over time, although the refinements of local deviations at particular ages are not delineated.[3] In general the parameter α changes the level of mortality. If α is fixed, varying β essentially tilts the relationship between earlier mortality, particularly in childhood and death rates at older ages. Thus the two degrees of freedom mean that starting with any given pattern of mortality the level and child-adult relationship can be altered in any way. A remarkable feature of the verification of the system is the behaviour of the empirical β's relative to the mortality level. If l_x' is taken as an "average" life table then it is reproduced by $\alpha = 0$, $\beta = 1$. But the average β is found to be near one in observed life tables whatever the level of mortality. In studies of how mortality has changed in countries which have a long series of life tables, α moves steadily with falling death rates while β fluctuates about 1 but has a strong tendency to return to this central value. Thus the data from England, Sweden and some of the Scandinavian countries which have the longest experience show this surprising consistency in the behaviour of β over time. The basis of the model may, therefore, have deeper implications than are apparent from its largely empirical derivation. If there is a biological reality underlying the age scale transformation, the resulting distribution of lengths of life can, perhaps, be given a more fundamental interpretation in which α changes with improved control of disease but variability remains fixed. Some fluctuation in β would remain because of time lags in the spread of disease control to different groups in the population.

The value of the system can best be illustrated by an indication of the uses to which it has been put. Some years ago its application to generation projections of mortality was examined, that is to serve the same purpose as the one parameter relation of Kermack *et al.*[2] The introduction of the second parameter meets the main objections about the weakness of the assumed constancy with age in the ratios of the forces of mortality, although there are difficulties in fitting the logit model to the short sections of the life table for recent generations. Undoubtedly the logit system *describes* the changes better than the exponential but judgement on the value of the predictions raises other issues. The model is also convenient for the general formulation of the projection process because the calculations give, for life tables in different periods, a series of values of $\alpha(t)$ and $\beta(t)$. For projections of mortality into the future the problem reduces itself to extrapolating two time series. Whatever method is used, the approach is simpler than projecting separately for each age group, treating each specific death rate as a separate independent measurement, when they are clearly highly dependent. The system is useful in cutting down the measures that are worth projecting to the two main components of change, those of level and child-adult relation.

It is interesting to note that in a number of countries which have long series of life tables, e.g. Sweden, Norway and the United Kingdom, $\alpha(t)$ has been changing almost linearly with time.[3] In other words, it can be suggested that the scale of $\alpha(t)$ is a good one for gauging the pace of mortality change. The traditional measure, the expectation of life at birth, has not changed linearly over time. It has risen faster at low expectations and more slowly at higher. On the logit transformed age scale, improvements in death rates become less for a fixed shift in location as lower mortalities are reached in accordance with experience.

These are some of the direct applications of this system. Another field of use which is of particular relevance to demography concerns the relationship between mortality patterns and age distributions. The question is the extent to which changes in mortality affect age distributions as compared with changes in fertility. The intuitive view is that mortality must have a large influence since if death rates fall more people live on into old age but the theoretical answer is that this is offset by the increased births to the larger numbers in the reproductive period. The logit system is particularly convenient for looking at these problems because the calculation of age structures for different levels of fertility, and the parameters α and β can be performed so neatly on a computer. Hobcraft[5] has done many studies in this area. It turns out to be broadly true that changes in the level of mortality have a limited impact since the value of α hardly affects the age distribution at all but changes in β alter it substantially. If the level of mortality is measured by the expectation of life the conclusions are not quite so tidy, but the general result, that the proportions in the major age sections of life (childhood, reproductive period, middle and old ages) are determined by fertility and the *relation* of childhood to adult mortality rather than the level, remains true.

The model system has value because of its descriptive capacity but there are also signs that its relational structure has a broader kind of biological reality. If this is true we can speculate on what kind of light it throws on some of the classical problems of mortality. The definition here of a " classical problem " is one that people keep on asking about but where it is almost impossible to give a meaningful reply.

The first one is about the span of life. The question is " What is the upper limit of life? ", and, of course, we all know the answer. It is finite but also infinite in the sense that we cannot specify any fixed limit. If we did then next week somebody might find a man in the Armenian mountains or the valleys of Ecuador who is older. The logit model copes with this in an elegant way by the scale transformation. On the new scale the upper tail of the distribution of the length of life goes to infinity. The question is turned into a fairly straightforward one about the probability beyond any point in the upper tail. The particular form of transformation is such that increases in y at late ages lead to progressively smaller rises in x. Thus as the incidence of mortality falls the probability of lengths of life above a fixed age goes up but more and more slowly. The effect is illustrated by some calculations of the consequences of moving α to values which have not yet been reached. If it continues to change at the average rate of both England & Wales and Sweden over the past 100 years or so (this was nearly constant) we might anticipate that by the middle of the 21st century a life table for females, say, would have something like 90% surviving to age 75 compared with 63% now, and 62% to 85 compared with 27% now, but only 5% to 95 years compared with 2% plus now. So the general way in which the transformation operates has this effect of making the increase of the percentages surviving to ages, near the highest usually achieved, slow indeed but the increase at slightly lower ages substantial.

Let us now consider two questions embodying views which are virtually contradictory. One is the idea that a generation carries its own mortality with it. This is the basis of the Kermack, McKendrick and McKinlay forecasting method and they explicitly put it forward. Presumably the implication (it is not always easy to know what is meant by these very general statements) is that if, in childhood, a generation has a particularly favourable mortality experience then, simply because of that, without any relationship to subsequent conditions, it will tend to have a favourable mortality experience at older ages. The directly opposite idea, which one hears equally often, is that the saving of life at earlier ages in a generation means the accumulation of the less fit at later years and, therefore, leads to higher death rates than would have occurred otherwise.

In the relation logit $(1-l_x) = \alpha+\beta$ logit $(1-l'_x)$ let us assume that β is always close to 1, except for short term fluctuations and consider the difference on the logit scale between mortality in successive generations. If β were exactly equal to 1, the generations would give a set of parallel lines on the transformed scale obtained from any of the life tables. In concept it would be easy to try and answer the kind of question posed. The test would be that when there was a big change in mortality between two generations, on the hypothesis that a generation carries its own mortality with it, the lines would tend to be just as parallel as for a small change. This follows because if a generation does particularly well in childhood compared with its predecessors but still maintains the parallelism it means that on this scale, which has been found reasonable, it has the same advantage at each age. On the other hand, if the advantage did not persist, there would be a tendency for the lines to converge when it was big or diverge when it was small. If the convergence or divergence was so great that the variation in generation differences was bigger at higher than lower ages it could be taken as evidence in favour of the accumulation of the unfit through unbalanced saving of life. However, the simplifying assumption about β is not acceptable since it moves away from 1 over periods of time in populations but in a regular, consistent way. The curves on the logit scale do not leap up and down, but there is some tendency for them to have long-term cyclic trends. This complicates the analysis but does not alter the argument essentially. The criteria must be applied locally, relative to the size of β to determine whether for large generation differences the lines tend to stay as parallel as for small differences or whether there is convergence and divergence to a " random " or consistent extent.

Unfortunately, it is not easy to obtain firm answers from the analysis of the observations, partly because there are not generation mortality measures for enough countries over substantial periods of time and also, of course, because the tidy theoretical straight lines are confused by bumps, due to wars, epidemics and so on, which can be regarded, from this point of view as being haphazard or chance effects. I spent some time recently looking at such data as could be found which might throw some light on these questions. The Swedish measures (in fact, the Scandinavian data in general, but it comes out most clearly for Sweden) over an extensive period of time suggest that the hypothesis, " a generation carries its mortality with it ", is amply confirmed. When allowance is made for the long term trends in β the lines tend to remain parallel whatever the distance between them (and some of the generation differences are striking). From the England and Wales measures, the conclusion is not quite so clear cut but on the whole they tend to confirm the results from Sweden. At least the patterns are entirely opposed to the view that there is any accumulation of the unhealthy because of particularly favourable childhood experience. This opposite hypothesis seems to be ruled out. But the evidence from the U.S.A. is exceedingly curious. What one finds in the U.S.A. generation measures are characteristics that seem to contradict the ideas already considered, although there are other interpretations. For some reason, in the U.S.A. a number of generations which started off in childhood with mortality little better than the previous generation have shown large relative improvements at older ages. The trends almost suggest that a bad experience in childhood leads by itself to mortality advantages later although, of course, it is possible to argue in an inverse way that if a generation which had a good childhood experience had not accumulated the unhealthy, it might have done as well as the poor starters. The size of the effect does not make the argument very plausible, however. The particular conditions of the U.S.A., primarily the high immigration which has changed the composition of the generations with age, casts doubt on the appropriateness of the data for examining these particular hypotheses. Judgement must be suspended.

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SYNOPSIS

The search for functions which describe how mortality varies with age is an old one. Over the past two decades demographers have developed model life tables based on the structure of relations among mortalities in different populations rather than explicit expressions of age. The earlier work was analytic and empirical using techniques of statistical regression but more recently functional models have been constructed. These developments are described with particular concentration on the logit relation families of curves.

Some uses of the model life tables in demography are examined such as mortality projections, the dynamics of age structure and estimation from the limited data of developing countries. The implications of the logit system for certain of the " classic " speculations are considered—the span of life, whether a generation carries its own mortality with it and the accumulation of the unhealthy at older ages with saving of life at the younger.

DISCUSSION

Mr. R. P. Bews. I would just like to say how very much I have enjoyed Mr. Brass's talk tonight. It contained much which was of interest and I am sure that it will form a valuable addition to what are, I am afraid, of necessity my rather limited and infrequent encounters with the science of demography. The complex mathematics which are associated with the study of population mathematics have hitherto tended to hinder my digging into the subject deeply. In fact, I must confess that my knowledge of population mathematics is limited almost entirely to those chapters in P. R. Cox's classic work on Demography which deal with the subject and, because of this, it was with some trepidation that I approached this meeting tonight, especially when it is devoted to a paper which introduces such subjects as " model life tables based on the structure of relations among mortalities in different populations rather than explicit expressions of age ". This is a concept which, I must confess, is rather new to me and, as the President has suggested, the " logit relation families of curves " are also something of a novelty—to me, at least. It was reassuring to find that the latter were little more than our old friend the normal curve in disguise so that, although it would hardly be correct to call them close friends, at least one can regard them as nodding acquaintances.

I think it is a tribute to Mr. Brass's clarity of exposition when I say how little difficulty and how much pleasure I had in following his account tonight of how these model life tables may be used and the implications which they have for us. I would thank him for leading us painlessly through what I suspect to be some rather tangled mathematical thickets. Having said that, I must confess that there is one point where I have not understood Mr. Brass thoroughly. When he was speaking about the logit formula which incorporates the parameters α and β, he said, if I heard him rightly, that a variation in α did not cause any relative variation in the proportions surviving in these model life tables, whereas a variation in the β factor did: yet, when he was talking about the proportion surviving based on extrapolations from the England and Wales life tables, he said that the change in α provided very marked differences in these proportions. There seems to be a contradiction here and I would like to hear Mr. Brass's comments on this point. If I am right in this and he was talking about a variation in α in the England and Wales life tables, perhaps he could tell me what effect a change in the β function would have on them.

Prof. Brass. This question illustrates the dangers of talking to members of a profession which is not your own. the tendency to assume quite wrongly that they know about the problems which concern you deeply. In fact I had some doubts whether I should put this example in at all since fully to explain it I should have gone much further. What I was talking about is the effect of α and β, which are parameters of mortality models, not on the life table probabilities of surviving or on the stationary population, but on actual populations developing over time with a fixed fertility. Some twenty years ago, stemming directly from the work on model life tables, Bourgeois-Pichat, Coale and Stolnitz came up with what seemed at the time an almost earthshaking discovery, that the age distributions of populations had practically nothing to do with mortality but were mainly determined by fertility. In the crudest way, Bourgeois-Pichat's statement was that if the fertility of a population remained constant, falling mortality led, of course, to a more rapid growth rate but hardly affected the age distribution at all. Like all epoch-making discoveries (and I think it was that) it has been found to be wrong, but not in a simple way. Since it was announced there have been much more complex investigations by demographers of what really changes the proportions by age in a population. Fertility, undoubtedly, has an enormous effect but if we keep fertility constant but vary mortality, what alters the age distribution is not the level of mortality but the pattern and, particularly, the relation on an appropriate scale between childhood and adult death rates. The logit model system provides what seems to be about the right scale for measuring this. If a population is projected with fixed fertility and mortality falling on a logit scale with α varying but β constant, the age distribution hardly alters. On the other hand, changes in β, which measures the relation on the scale between adult and childhood mortality, have substantial effects on the age distribution. Such changes do take place in actual populations.

Mr. P. Giles. I wonder if Mr. Brass would expand one other little direction in his talk? He has mentioned Coale and Demeny's statistical approach to forecasting besides his own method of using logit curves. The problem of forecasting future rates of mortality brings to my mind the recent discussions on the comparative level of mortality between the sexes. In particular, female mortality has been steadily improving recently but male mortality has not improved to anything like the same extent and this contrast is exciting great discussion, I believe. Perhaps Mr. Brass would tell us what his views are on the contrast.

Prof. Brass. I am one of those people who believe that males and females are different. There are some model mortality systems that link male and female mortality precisely—that includes the Coale-Demeny and the United Nations. These systems are such that for any given level of male mortality a level and pattern of female mortality is specified. Of course, the relationship is not the same at high and low mortalities. But, as our information grows, particularly about developing countries, it seems less and less likely that this is a reasonable procedure. I prefer to try and treat the male level and pattern of mortality separately from the female level and pattern. But the question also raises slightly different issues. In terms of the logit system, what is happening in Europe and the Western world is that the β value for male mortality is changing in ways which are different from the female trend. While the β measure for females has been changing slowly, that for males has been moving from central values strongly in the direction which indicates that adult mortality is becoming high relative to childhood mortality. The question that arises is whether this is a temporary or a more or less permanent phenomenon. In other words, is there going to be, at some time, a reversal of this trend leading to a substantial improvement relatively in adult male mortality. The logit system (though I have not gone into the reasons deeply) implies that there is going to be this kind of reversal, if weight is given to the average consistency of β over time. I have no more ability than anyone else to foresee the future but I can say that such reversals have happened before. In particular in Scandinavia there was a period in the 19th Century when, over a considerable length of time, improvements in adult male mortality were slight but the reduction in early mortality substantial. Then this changed and the adult mortality improvement caught up. These changes are, of course, measured on the logit scale which implies a smaller percentage movement in death rates for adults than for children as a norm. Therefore, there is some justification, historically, for believing that the present slowness of improvement in male mortality may be a temporary phase and that it will be reversed in future. My prejudices lie that way. I think that if males start behaving as sensibly as females their mortality will decrease more rapidly.

Mr. P. R. Cox. I am delighted to be a visitor here tonight and if I ask a series of related questions I hope you will take it as a sign of gratitude and not of greed.

In the first instance, I should like to ask Mr. Brass whether it is not a fact that Derrick wrote his paper in 1925 whereas Kermack, McKendrick and McKinlay came somewhat later. I have usually given them a reference date of 1927 or 1928 but if there is an earlier publication I should like to know. This is only for the record because I do not think that the discovery of generation mortality relationships can be regarded as such a great scientific development that there must be argument as to who came first and I am quite prepared to accept that in all probability the "three K's" had never read the Institute of Actuaries paper of 1925.

But a more weighty point derives from this, which is my second question. I think that actuaries who discuss mortality, whether it is relation by age or in time, have always wanted to have a fairly simple "feet on the ground" explanation of why this is happening. They do not want just a formula: they want something which has a real meaning like Gompertz's and Makeham's laws. Now, I think the "three K's" on the whole were quite happy to have a formula. I think Mr. Brass is quite happy to have his logit formula, but Derrick had to explain what he was doing by saying "each generation carries its own mortality with it". Would Mr. Brass say that this is a correct thing for actuaries to be worrying about—the realistic interpretation of mortality theories—or does it not really matter? Can we just play with formulae? In this connection, does the logit system in fact have a down-to-earth explanation of this kind?

I think that where Derrick's generation theory has gone wrong is that when you are 80, it does not really matter quite a lot whether you were born in 1900 or 1880 or 1860, you are still a very old person. The generation of your birth is not going to have a great influence on your future longevity. The things you have lived through, the accidents which have happened, are going to be dominant. Is there any sort of tie-up between the two-parameter logit theory and the wearing-off of a generation's effects in old age?

Prof. Brass. Yes, surely. Mr. Cox has an advantage over most of you because he has heard me talking about some aspects of this topic before. He is quite right about Derrick and it is really because Kermack, McKendrick and McKinlay are such a nice set of names that I put them first. But there is some fairness to it. For a demographer or a statistician, Kermack, McKendrick and McKinlay's work is more impressive, because they examined the implications of the idea thoroughly, particularly in a later paper in the *Annals of Eugenics*. Should there be a simple explanation of what is happening? This is a point that always worries me a little. I think it often tends to mean a familiar explanation of what is happening and much of this is a question of knowledge of the language. The reason why some people, as for example the first speaker, find the normal distribution comforting is because they are familiar with it. A probit system based on the normal distribution has much the same features as the logit one and I will try to "explain" in these terms. There is some kind of natural scale of mortality governing the variations in risk by age because broad patterns are similar in all populations. It is obviously complicated, particularly in the early years of life, because it is connected with what happens to our mothers before birth, the trauma of birth, the impact of the environment, and at later ages the breakdown of body cells, and so on. Assume there is a scale without worrying about its nature and transform it in such a way as to obtain a normal distribution of the length of life. Then, what we are saying is simply that on the proper scale a normal distribution of the length of life is obtained for different generations and different life tables but the average impact is different. Thus the mean length of life changes with the conditions; the spread or variability may change also but in general only to a small extent and there is a tendency for it to return to an average value. This seems a different kind of explanation from the statement that a generation carries its mortality with it, because of the terms in which it is expressed. It is not really a different kind of explanation, because it also says that a generation mortality has a fixed form which can be determined from the death level and pattern in childhood. (The lower tail specifies the rest of the Normal distribution.) The relation is different from the Derrick and Kermack *et al.* one but it has to be since theirs does not fit the data adequately. An acceptance of the fact that a natural scale exists which leads to a simple relation among all life tables can be regarded as a basis for the statement, " This is a theory that a generation carries its mortality with it " just as well as a constant ratio of the forces of mortality. Whether it is a true statement is another matter. Is it a valuable statement? I do not think so. Whether you believe it is true or not matters little because, in practice, the question is whether by starting from some knowledge of a generation's early death rates the mortality at older ages can be predicted better. Whatever may be said about the theoretical concepts, in practice they do not help much for this purpose. The proportion of mortality variation that can be explained by this means is small if not negligible.

Prof. D. J. Finney. As one who has spent much time thinking about probit and logit transformations, I was immensely interested to hear Mr. Brass talking about these matters from an angle which is completely new to me. I thought that he, perhaps a little consciously, disguised or concealed one aspect; this is perhaps not particularly relevant to the questions of prediction, but I hope that before the evening is over he will satisfy my own curiosity. He spoke more than once about the transformation from the chronological age scale to the actual age scale on which we can assume a normal distribution or a logistic type distribution: I am very curious to know, first, whether this transformation is entirely empirical and second, just what it looks like. Is it in some sort of logarithmic form that can be functionally represented, or is it something exceedingly odd that just happens to work? Of course, to anyone who has worked in this field it is apparent that one could take any standard family of curves and find an age transformation that would give a fit. One might, for example, try one of the gamma family of distributions to see whether it gives anything different. One would expect that as extrapolation goes rather further into the future these would make some substantial difference to the inferences drawn, but probably the cautious demographer would not go so far.

A second point has occurred to me. Mr. Brass discussed what will happen to the distribution of the parameters in the future. Is this still something that just " happens ", or are we now reaching a stage of history at which we almost exercise conscious control? Over the last 30 or 40 years there has been an immense reduction in childhood mortality and in infant mortality largely because of the medical attention that has been directed at their causes: this attention has been to some extent emotional in origin, to some extent taking advantage of the circumstances that led to the virtual elimination of certain diseases, such as tuberculosis, becoming a practical possibility. If today a similar impetus were given to appropriate medical research, like benefits might be obtained in respect of mortality from cardiac disease, various forms of cancer and other ills of later life. Are we reaching the time at which we ageing men may get together and say, "We are now determined to reduce our own mortality", or are we going to be as restrained as we have been so far and let things take their course? Perhaps I exaggerate, but I shall welcome an opinion from Mr. Brass. Is this effect still too small in relation to total mortality to be of any importance in our discussion?

Prof. Brass. I have never been very interested in what the transformation of the age scale is and, to an extent, the whole nature of the process implies a situation where it is too complicated to be simply expressible. It is almost a contradiction in operational terms to ask what the transformation of the age scale is. If there were some reasonable function it would be possible to express it in terms of age, and this is precisely what it has proved impossible to do. As Professor Finney obviously realizes it is a very peculiar transformation either on the logit or the probit system, because the point of zero age goes to minus infinity and it is difficult, except in mathematical terms, to make this meaningful. In fact, in practical applications this is not important because, usually, the observed pattern of mortality at *very early* ages is not well-fitted by this scale. It can be made to agree if special devices are used to account for the deaths in the first week or so after birth which are more related to foetal mortality but this is seldom necessary. If there is anything more than a mathematical trick here, then the transformation would seem to have some kind of deeper biological meaning and I have tried on occasions to examine how it conforms with theories of ageing. I have experimented with mathematical models, combining ideas about the before-birth influences with the effect of environment and also with functions developed in the theory of ageing but nothing much has come out of it.

Whether this particular logit or probit system is the best one, is unknown. I have tried several others but none has worked as well. It was emphasised at the start that for the kind of demographic purposes envisaged (this is certainly not all life table purposes) simple functions with few parameters are required and these are conditions which severely restrict the possibilities.

To what extent can we control mortality and how does the model fit with this? Perhaps Professor Finney has more belief in the medical profession than I have. But that is not quite the right way to put it. In studying mortality, particularly in developing countries, for a number of years, I am more and more convinced that for a whole population the dominating influences are social and economic. The efforts of the medical profession are also dominated by the social and economic circumstances. There are plenty of examples, one of them being the nature of mortality in the United States, another that in all countries there are selected groups of people who have very different death rates from others. I suspect that what is needed to reduce adult mortality is that adults should look after themselves as well as they have cared for children. The children do not have the freedom and authority to make a mess of their own lives; the adults do. If this happened my guess is that patterns of mortality would return to one which on the transformed scale had much the same variation as in the past, because of bio-social features, although with a higher mean length of life.

Mr. A. D. Wilkie. May I ask Mr. Brass one question? The suggestion that a generation carries its own mortality with it seems to me contrary to the idea I had always previously thought was the case that changes in time were of considerably more importance than changes between one generation and another. For example, the sort of drop in mortality that seemed to occur in this country after 1942 with the introduction of penicillin affects all generations more or less at the same time and so a time improvement affects the generations at different ages. Or have I got things all wrong?

Prof. Brass. No. I think you are quite right. This is one of the reasons why these questions are so difficult to answer. To talk in terms of the logit system, although this is not the only way to do it, the effect of such changes in time periods are reflected largely in movements of β over time. If there is a sharp improvement of mortality it tends to have the general effect of distorting the β value for the particular group that is affected early in life but having little influence for a group that is affected late in life. It is because these time changes are so important that it is difficult to allow for them in examining whether, underlying, there are also generation effects. There are big differences, mostly unexplained, in mortality improvements for generations of children. In some periods, say over ten years, there are large decreases in childhood mortality and in others little. It is a real question to ask, if you have a substantial improvement, whether it is retained in the future. I do not see any evidence in the data that supports the hypothesis of the accumulation of ill health, a quite widely held idea in the medical profession that saving people from dying at early ages is just leading to more trouble for them at later years. However it is not nearly so certain whether a generation that starts off in childhood with a greatly improved mortality tends to regress towards the average betterment or continues to have an extra advantage. With more data, say about 100 years worth, it might become an answerable question.

Mr. P. R. Fisk. It seems appropriate to me to intervene at this stage because the last question seems to me to be answerable in one way. In one of your descriptions of your parameters α and β, you describe them in terms of one variable only that is, time; but of course there is no reason why you should not describe them in terms of as many variables as you care to have. It could be $\beta\ (x_1, x_2, x_3, \ldots . t)$ where x_1, x_2, x_3 and so forth can be other variables which may be of an economic, a social or a medical kind. Now, a model of this type, that is, a life table survivorship distribution with regression included in the parameters, is going to be the basis of the next meeting of the Royal Statistical Society in London on March 8th and I have no doubt that Mr. Brass will make a very valuable contribution to the discussion at that meeting.

Another thing that occurred to me, which is quite unrelated to the previous comment, is that you mentioned at the very earliest stage that there are problems in trying to fit life tables to the material obtained in under-developed countries. Now, I can well see that there are problems in collecting satisfactory data from these sources and I recall a paper published in *Biometrika* (1954) Vol. 14 by J. M. Hammersley and K. W. Morton which attempted to estimate a frequency distribution based on rather scanty data, using as a basis another empirically obtained distribution for which a great deal of information was available. The assumption underlying the procedure was that the two distributions belonged to the same family although the exact nature of the family was completely unknown, and that they differed only in their mean and variance. I wonder if this sort of approach, which is in a sense a regression type of approach, might not be useful when trying to make use of information from life tables for a country which has very reliable data in the estimation of the life table for an underdeveloped country with less reliable data. One would need to know that the distribution for each country belonged to the same family. To use this sort of approach may be rather unsatisfactory because I suspect that the problem in underdeveloped countries is that error in their demographic data is not caused by random variation, as required by the Hammersley and Morton approach, but by biases.

Prof. Brass. You are referring to the Royal Statistical Meeting on David Cox's paper, which I have read. The statisticians, in many ways, are working in a simpler universe than the demographer. That paper is primarily concerned with studies of survivorship over limited periods of time (often not human survivorship) of individuals treated in particular ways, for example with drugs or surgery. There is nothing like the complexity of the relations with scale which arise with mortality over a whole human lifetime. Therefore, the problems are allied but I do not see at the moment any practical method of using the techniques that have been proposed. The concepts might, however, provide illumination. One obvious approach is to take into account that populations are not homogeneous groups. It is possible to consider the mortality of these groups separately and ask questions about how the simple parameters might be broken down to describe the varieties of sub-populations. This is interesting but I have not felt that it was terribly useful for demographic applications.

I am not familiar with the method of fitting that you mentioned but I think it is close to a procedure which has been used in an empirical way with the logit model. Not only can the α and β parameters be varied but also the underlying initial life table pattern (or standard). An example of particular interest comes from work on mortality in Fiji. Direct information is poor but there are several pieces of data which can be put together—quite good inter-censal survivorship materials, rather inadequate vital registration and survey reports by mothers of child deaths. The aim was to fit some kind of logit model to these but there were indications that the mortality pattern was rather odd. It would not be well-fitted by the basic standards that have been widely used, for example a general average over all life tables or the Coale-Demeny Regional patterns. In searching to find a standard which might give a better fit I came across a Polish life table of the 1930's which had just the local features needed, particularly very low adolescent mortality compared with childhood death rates, and this worked very well. The α and β values found for Fiji were very far from those for Poland and, therefore, a completely different life table was derived but it had the internal characteristics required. It is possible, therefore, from a little knowledge of the mortality characteristics to pick a basic pattern and thus improve the graduating powers of the model. I wish Mr. Fisk would some day work out a theory of fitting where the errors are not necessarily random. I do my best to tell students what to do with non-chance errors but it essentially reduces to " use your judgement ". Demography is, possibly to a much greater extent than statistics, a profession where judgement is all important.

Prof. J. R. Gray. I am not an expert on demography and any comments I make can only be made as an actuary and statistician. I am reminded of the schoolboy's approach to the history examination: I do not know anything about the Wars of the Roses so I will talk about the Jacobite Rebellion. I hope, however, that my remarks will have some relevance.

Statisticians are basically concerned with average or expected values and the probability distribution of deviations from average.

The classical actuarial science of life contingencies is based on the assumption that expected values will be attained in practice and life tables are constructed and interpreted on this basis. Provided company resources are adequate to deal with runs of adverse fluctuations from these expected values, no danger will arise through ignoring deviations. However, the simplifying assumption that "all will balance out in the long run" can only be acceptable provided the "long run" gets a chance to operate. With large life assurance companies, fluctuations do not cause real problems and can probably be justifiably ignored. There is, however, no shortage of recent evidence to suggest that small companies with limited reserves, which might be tempted to offer cut-price premiums to encourage new business, would be well advised to consider the problem of deviations in greater detail and perhaps all actuaries could usefully do so.

Statisticians, through stochastic process theory, can provide some further insight into the behaviour of life tables. A pure death process model with a suitably defined force of decrement could give relevant information. Another possible model would regard the life table as a superimposition of branching processes with appropriately defined distributions of descendants. However, although such models provide useful information in assessing general probability patterns of deviations, there are clearly defects which prevent conclusions being acceptable as exact. The picture which emerges is that although the statisticians can throw some light on another aspect of the behaviour of life tables, they include so many simplifying assumptions in their models that a certain amount of objection could be taken to their conclusions.

In particular, these models ignore the heterogeneity of an actual population where a saving of life at younger ages (negative deviation from expected number of deaths) is likely to be accompanied by some corresponding adverse fluctuation at later ages caused by the longer survival of unhealthy lives. Such balancing is not provided by random models. Actuaries need more than the methods of mathematical statistics to reach a deeper understanding of the main tool of their profession—the life table.

After tonight's lecture, there can be no doubt that demographers have an equally important contribution to make towards reassessing the behaviour and interpretation of life tables. I was particularly interested to learn about the new demographical methods and models which can help to develop the actuary's insight and I am sure we should all try to use the ideas we have heard about tonight to think more deeply about concepts which we perhaps too readily take for granted.

Reference has been made to Swedish data in general. In September when I was in Stockholm I learned that recent Swedish data had unexpectedly exhibited a tendency towards deteriorating mortality. My informant commented that as far as he was concerned the way the world was going this was probably no bad thing! Another feature of my Scandinavian trip was that I met a Norwegian actuary who told me he knew a Scotsman called Brass who was both an accomplished demographer and excellent lecturer.

Gentlemen: I am sure tonight's speaker has undoubtedly justified this reputation and I would like to add my thanks for his stimulating address and the stimulating way he has answered questions.

The President. The way you have received Mr. Brass this evening and the number of questions that you have raised shows how much you have appreciated his lecture to us. The subject of mortality is one of the foundations of our professional activities and although in our everyday work we tend to couple this with the financial aspects as well, nevertheless, most of us are interested and often fascinated by the subject of mortality in its demographic context. It is always a great pleasure to listen to an expert and we have heard one this evening.

I would thank you, Sir, for coming to us this evening in the midst of these difficult days of power cuts and I would ask you to accord Mr. Brass a hearty vote of thanks.