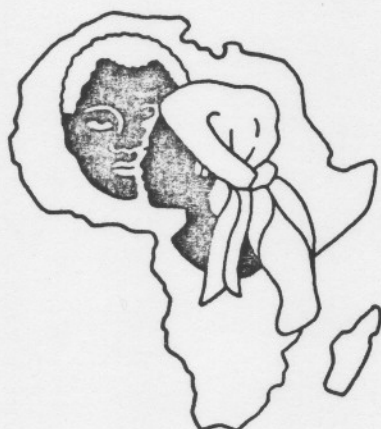




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The Study of Mortality in the African Context

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THE STUDY OF MORTALITY IN THE AFRICAN CONTEXT*

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Introduction

The demographic study of mortality in Subsaharan Africa is dominated by two paradoxes. The first has to do with the recognition awarded to the topic. The persistence of high mortality levels--higher probably than in any other large world region--makes it a potentially burning social issue. The people of the area are concerned about access to modern medicine and the eradication of diseases. If a field calls for the development of accurate statistics, this is it. We know little about mortality levels and their distribution over space; we know even less on trends, and virtually nothing about mortality differentials by social and economic circumstances. There are no major breakthroughs in morbidity and cause of death statistics. Africa is still far from the stage reached in Europe 150 years ago in the study of mortality. When William Farr organized the collection of vital statistics in England and Wales his concern and that of his contemporaries was with the fight against disease. Farr, a mere Compiler of Abstracts at the Registrar General's Office, was hailed as the foremost medical statistician of his time; it was said that after him "pestilence no longer walketh in the dark." The use of the data he helped to collect was decisive in the conquest of the major scourge of the time, cholera. He provided information on the location of the most unsanitary sections of the country and identified the most dangerous occupations. We doubt that the demographic statistics that are collected today in Africa are used very much in

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the same way, to identify areas of infection and classes of the population specially vulnerable to specific diseases. Despite the importance of these issues, and despite the universal desire to prolong life and to eliminate the human wastage of early death, little effort goes into the collection of demographic data on mortality. This becomes more apparent if we compare research on mortality to the much more active interest in fertility, although the latter topic is not widely recognized in the area itself as a burning issue. Our aim is certainly not to suggest that less research should be directed towards fertility and its determinants. Rather, we find it paradoxical that mortality research does not elicit more attention.

But even this view must be qualified, and this leads to the second paradox in the study of African mortality. Its main weakness is in the area of data collection and in the publication of collected data. In fact, the very absence of conventional data has presented a challenge to demographic analysts, and an enormous amount of ingenuity has gone into remedying the shortfalls of the data collectors. The developments have occurred far from the field, as a result of the work of a handful of scholars working in universities (mostly London and Princeton) on data of the most sketchy kind. The use of indirect methods of estimation of mortality is now dominant. But the development and improvement of data collection should normally lead to the elimination of these techniques, not to their triumph. Anyway, it is likely that we are fast approaching diminishing returns in this area, as the invention of yet another set of multipliers to analyze the number of children surviving or another set of model life tables are not likely to advance the field significantly, at least as long as data collection lags behind.

In the long run, a system of complete and accurate vital statistics will have to be developed in Africa, as elsewhere in the world. There can be no reasonable doubt that the routine registration of current deaths, close to the time when they occur, remains the only method to obtain reliable mortality statistics covering a whole country on a continuous basis. The goal is out of reach in most areas of the

region within the foreseeable future. This does not mean that the existing systems should be neglected. They require careful nurturing so that an administrative tradition will exist along with progressive acceptance by the population. Also, even partial registration of vital events can be helpful for civil and legal purposes, such as identification and establishment of certain rights. Finally, a tradition of birth registration, even when it produces only biased direct statistical by-products, will enormously improve the knowledge of ages, and will therefore facilitate the gathering of information on mortality by surveys. This is the experience in Latin America, where the methods that will be discussed in a later section of this paper, work rather better than in Africa where they have been pioneered.

Because the prospects of rapid improvement of vital registration are dim in Sub-Saharan Africa, the collection of mortality statistics will continue for sometime to be an uneasy compromise between the goals of continuity, accuracy and universality. The following data sources will continue to coexist.

a) Continuous coverage of the population in selected registration areas.

These areas are usually unrepresentative of the whole country, and the completeness of the collected data is often in doubt. The registration area may progressively be expanded to the entire country.

b) Inclusion of mortality questions in censuses to provide coverage of the whole population at widely spaced intervals. This solution stresses universality at the expense of continuity of coverage over time and of accuracy.

c) More or less complex and frequent surveys taken on a sample basis. Here the emphasis is on quality of the data and on representativeness at a point in time.

In the following pages, we shall concentrate on censuses and sample surveys. We shall first discuss some of the problems of data collection; second, we shall review some of the analytical techniques that can be applied to the collected data; and third, we shall comment on the need to collect and tabulate more detailed data,

both as a way to provide insights into the data biases, and as a means to get more knowledge of mortality levels and trends.

Data collection

The difficulties of collecting data on mortality in Tropical Africa are widely acknowledged; but their exact nature is still in doubt. Many of the problems are not restricted to Africa or to illiterate respondents. The question on deaths during the last year in the household had been tried and found wanting in the United States censuses of the late 19th century, and its repeated failures in Africa should not surprise us. Retrospective questions about the deceased, his or her personal characteristics including age and cause of death, do not seem to be giving very reliable results anywhere. Most countries are relying on vital registration to provide this type of information; but in its absence, ways have to be found to get as reliable information as possible through survey techniques.

The blame for the failure of mortality questions must be apportioned between the respondents, the interviewers, and the survey organizers and questionnaire designers. It is too easy to blame biased results on the illiterate population and on folk cultures that pay little attention to the variables that are of interest to demographers. Allusions to a reluctance to discuss the subject of death with strangers are too frequent to be a myth, but this fails to explain why successive surveys or censuses have sometimes fared very differently when they tried to elicit responses about recent deaths. One example of that was the probably excessive number of deaths turned up by the Tanzania Census of 1967 and their clear under-reporting in the 1973 demographic survey of the same country. Such differences must probably be attributed to subtle differences in the survey procedures and the phrasing of the questions, rather than to a sudden change in attitudes toward death during the intervening period. Survey design is important; and the training of interviewers and their supervision in the field is crucial.

It certainly should not be assumed, moreover, that the competence and professional integrity of interviewers is uniform in a large survey or a census. Everyone who has been involved in the field has either seen or heard about cases of entire villages where particular interviewers had found no deaths, leaving the relevant column entirely blank. The most plausible explanation is that these interviewers had misread the questionnaire.

This unevenness is precisely one of the reasons why censuses are not a good medium to collect data on mortality; the other reason, of course, is that such questions can be time consuming beyond the time that can reasonably be allotted in covering the whole population. It would seem however that the question on survival of the children is well established in African censuses, and that the question on survival of the parents (the so-called "orphanhood" question) is included with increasing frequency. Although this is information that one does not really need to collect from the entire population of the country, the questions seem easy to ask, and they provide the most useful and usable data that can be obtained from any type of inquiry. It is another paradox in the study of mortality, that more detailed questions give less reliable results; and that intensive surveys of various kinds obtain their best results with the survivorship and orphanhood questions as well.

More intensive local or sample surveys have mostly relied on two techniques, alone or in combination, to obtain data on current mortality: A question on deaths in the household during the year prior to the survey; or continuous registration of the deaths for a short period (such as one year) between two rounds of a survey. The matching of individuals at the two rounds allows another handle in the search for those who might have died. Although some of these special surveys have given interesting results, it is fair to say that the results of the questions of children and parents surviving still constitute the standard against which other approaches are measured.

According to Blacker, in Africa:

Retrospective questions on deaths in the last twelve months have rarely been successful: apart from massive errors of omission; questions seem to have been particularly susceptible to complete misunderstanding.¹

Why this is so remains puzzling. If the systematic underreporting of deaths during the year studied is the result of the people's reluctance to talk about their dead, then it is not obvious why they are willing to mention those children and parents who should have been specially dear to them when the question relates to the cumulative, life-time loss of them, but are unwilling to report them specifically when they died in the recent past. It is possible that the reluctance stems from the fact that the death was recent, and that the interviewers ask for a precise designation of who died, rather than for a more general statement of the overall number of dead children. Why then would the precise questions "Is your mother (or your father) still alive?" elicit reliable responses?

There are two additional reasons at least why asking about deaths during the last twelve months may have been the rather dismal failure that it has been everywhere. First, there is the uncertainty about the reference period. This issue is widely discussed when the parallel question on current fertility is treated, but it does not get much attention in the case of mortality. An effort has been made in many surveys to ask about the last death that occurred in the household and then to try to narrow down the date of the event. The results of this approach have not been systematically evaluated as far as we know.

A second cause of underreporting of deaths during the previous year, as assessed in a question to the head of household, is that it is possible, or even likely, that deaths occurred outside of currently constituted households. Certain deaths may result in the dissolution of the household--as when the head dies, his widow remarries, and the children are absorbed by other households; or when an old person, who lived alone, passed away. There are certainly many such instances, and no

respondent will be in a position to report them unless special care is taken to look for vital events that occurred in a geographical or administrative unit, on a de facto basis, for example by interviewing local notables or by consulting local records. It must be noted, however, that limiting the survey question to "deaths in this household" makes no reference to where this household was residing at the time of the death; and that in highly migratory populations there is no necessary equivalence between the population that is now residing in an area and is reporting on its losses during the year and the population that was subjected to the mortality risks of the area during that time. This point is particularly crucial in the dual record survey approach, when one is attempting to match deaths reported in a retrospective inquiry with those registered during the year. The question on "deaths in the household" could be (and is sometimes) rephrased as "deaths in this dwelling unit (compound or plot)," in the hope that present residents or their neighbours might know about events that occurred prior to the time when the present residents moved in. But once again, there exists little discussion of this problem, and how one copes with it, in the literature.

It would seem that the multi-round survey provides a solution to the problem of the potential loss of deaths in households that have disappeared. It becomes possible to match those present at the beginning and end of a period, and to allocate the non-matches to intervening deaths and departures. The galling issue of deaths among the in- and out-migrant households, however, remains. Moreover, a very substantial proportion of the deaths occurs among very young children, who were born between the two rounds of the survey, and had not been seen previously by the interviewer.

The design of surveys that will effectively resolve the problem of collecting current, reliable and useful data on mortality remains a task for the future. The track record in Sub-Saharan Africa, as we read it, has not been good this far. Of course we would like to be refuted on this point. The senior author of the present

paper wrote a polemic paper for the First African Regional Conference held in Accra in 1971.² He questioned the wisdom of large-scale national longitudinal surveys that had not been preceded by careful pilot surveys to test the methodology, assess the capabilities of the statistical organization of the country, and document the cost-effectiveness of this technique when compared with simpler, more frequent, one-round inquiries concentrating on Brass-type questions. Enough data and experience must have accumulated by now to judge whether this criticism of surveys that were then in process was justified or not.

In the article cited above, reference was made to the 1965-1966 Rural Demographic Sample Survey of Nigeria, a multi-round survey which missed many deaths, apparently of young children. A more recent example is the 1968-1969 National Demographic Sample Survey of Ghana reported by Gaisie.³ This survey consisted of two rounds of a panel study. The follow-up survey sought to elicit information, among other, on deaths during the period between the two rounds. In addition, births and deaths were recorded independently by local teachers specially trained as registrars. This careful design resulted in "general underreporting of deaths by both the household surveys and the registration system," and the author had to rely once more mostly on the Brass technique using the proportion of children surviving to estimate child mortality.

French-speaking African countries have heavily relied on the multi-round survey method. In a recent review of demographic research in Africa, Francis Gendreau has reaffirmed its value, and stated that the Algeria survey has clearly established that it improved data collection.⁴ But the method's major successes are still coming from outside of Subsaharan Africa, and have often been obtained in countries where a tradition of almost complete vital registration has already been established. There is certainly now enough experience gained to arrive at an objective evaluation of the method, both outside of Africa (where its potential has been demonstrated) and in Africa. It seems to have been established beyond doubt that small-scale

intensive surveys, carefully controlled and supervised, can obtain valid results.⁵ It is the extrapolation of quasi-anthropological techniques of data gathering to very large population samples that seems either too costly or administratively too unwieldy at this stage to provide a real alternative.

The small-scale surveys, carefully conducted, can be invaluable in providing information on particular aspects of mortality in Africa, and can contribute to improving the methodology of data collection. Even a small population can provide enough deaths of children to permit statistically significant statements on infant mortality; but it takes on the order of the observation of 100,000 person years at risk to provide a valid basis for a complete life table. The tyranny of large numbers is likely to limit the amount of accurate knowledge that will become available from small surveys.

These considerations explain why the next section of this paper will be mostly content to review analytical methods of indirect mortality estimation, as they are likely to remain the most useful in Subsaharan Africa for some time.

Methods of analysis

In this section we shall mostly discuss the estimation of infant mortality from data on the proportion of children who have died, tabulated by age of their mothers, and the estimation of the mortality of adults by the orphanhood method. These techniques are well documented in the literature and we shall describe them only very briefly here. We shall however, discuss their liabilities and strengths, particularly for the estimation of overall mortality levels. Before we do so, however, it is perhaps in order to comment briefly on a third indirect approach to the estimation of mortality, through the so-called intercensal survival method.

That method is potentially of great interest in Africa because it uses nothing more than two comparable age distributions, from censuses preferably five or ten years apart.⁶ It is based on the simple fact that, in the absence of migration,

cohorts enumerated in the second census must be smaller than in the first census, and that the difference must be attributed to attrition by death. But because of age misreporting and differential completeness of enumeration by age it is best to rely on entire ogives above a certain age. Thus, one asks in fact what level of the Model Life Tables would be required to project the population aged x and over at the time of the first census, to the population size aged $x + 10$ and over at time of the second census (if it is ten years later). This gives an estimate that excludes the mortality of children under 10 and, in general, of those aged less than $x + 10$ at the second census. x can take values between 0 and the middle of the age pyramid, and a final estimate is taken by some method of averaging, usually by selecting the median of the estimates based on successive values of x . The final estimate reflects the level of the mortality of children and an assumption about the pattern, which is assumed to conform to the model life table family used in the projection.⁷ As the taking of periodic censuses gains acceptance in Africa, the method should become more useful. But even though there are now several countries which took two censuses that would at least apparently satisfy the requirements, only in a few instances were the two enumerations sufficiently comparable in completeness and scope; "the comparisons have all too often been vitiated by varying degrees of coverage or by migration."⁸

One of the advantages of the method is that it gives an idea of the mortality of adults, and can be used together with the most reliable of all indirect methods, the technique invented by Brass to convert answers given by women on their children ever-born and surviving, into conventional life table functions for the early years of life. Brass has shown that the mean age of all children born to women in successive age groups can be computed if we know the underlying fertility schedule, and that the proportion of the children who died among those born to women in an age group, is roughly equivalent to a probability of dying between birth and that mean age. With appropriate adjustments for the shape of the fertility function,

Brass showed how to derive $q(2)$, $q(3)$ and $q(5)$ (or, in conventional life table notations, ${}_2q_0$, ${}_3q_0$ and ${}_5q_0$) from the proportions surviving among the children born to women in the age groups 20-24, 25-29 and 30-35, respectively. The use of other age groups (15-19 and above 35) to derive ${}_1q_0$ and ${}_{10}q_0$, ${}_{15}q_0$ etc., although possible in theory, should not be attempted in practice. Other versions of the method have been presented by Sullivan and Trussell.¹⁰ The three sets of estimates are very close and there is little basis for selection among them, except the relative simplicity of the respective procedures. On this count, the Sullivan version has possibly a slight edge.

These procedures raise, of course, a number of questions, both concerning the plausibility of the results and the adequacy of the models underlying the procedure. It is entirely plausible that the reporting by women in the census of the number of their children who died is biased downward. We shall reconsider evidence on this point in the third section of this paper.

Biases in the reporting of dead children may be specific by age. Moreover, the misreporting of mothers' ages may distort the results. In addition, the decline of mortality over time may have affected the relationship between the various ${}_xq_0$. Finally, the issue of the right pattern of mortality to be used in Africa remains unsolved. In fact, there is some evidence that mortality in later childhood is higher, compared to infant mortality, in Tropical Africa than in any one of the models. The preference for the North model is precisely related to the fact that that family of tables conforms the best to such a supposed pattern of mortality.¹¹ Uncertainty about the underlying pattern is compounded when one attempts to draw conclusions from such data about the overall level of mortality in a country. According to Blacker:

The dangers of using simple-parameter models to estimate adult mortality from infant and child mortality, or vice versa, have been receiving increasing recognition. There is incontrovertible evidence from a wide variety of developing countries that mortality patterns may deviate substantially from any of the existing single-

parameter models. The assumption of a fixed relationship implied by these models between infant and child mortality on the one hand, and adult mortality on the other may thus lead to a massive bias, not only in the mortality estimates themselves, but also in the variety of uses to which they will be put...¹²

This has led to a search for more appropriate models, and for indices of the mortality of adults, to be used in conjunction with infant mortality to access two-parameter models of mortality. By analogy with the derivation of child mortality estimators from the proportion of surviving children, analysts have turned to a question on the survival of parents to estimate the mortality of adults. The idea is simple enough: since the mother of an orphan was necessarily alive at the time of the birth of her child, her death must have occurred between the birth and the present, when her child is recorded as N years old. For a group of people aged N , and born when their mothers were on the average $\bar{M}$ years, this information permits us to compute ${}_Nq_{\bar{M}}$ for females. A similar reasoning leads to estimates of male adult mortality, using the responses about the survival of the respondents' fathers.

Means to implement this idea have been proposed by Brass and Hill, and Hill and Trussell.¹³ The first need was to introduce the orphanhood question in surveys and censuses; the results are now beginning to accumulate. They are reviewed in a recent article by Blacker.¹⁴ We shall not discuss the computation procedures but shall be content to discuss the possible limitations of the procedure and what we have learned from it.

Once again, the orphanhood method is a triumph of analytical ingenuity and we are not surprised to see William Brass' name associated with it. On the other hand, it is difficult to find criteria to evaluate its results, since we have no standard to measure it against (i.e., not accurate estimate of adult mortality in Africa). There are several a priori reasons to distrust the method. It weights mortality by the number of surviving offspring in the population; parents with seven children will be reported seven times, and the parents with no living offspring, zero times. As does the Brass method, this method also assumes no systematic link between child

and mother's mortality which would, if it existed, bias the mortality estimate downward. There is a possible confusion between foster parents and real parents. Finally, considerable mortality decline has occurred in most African countries during the life time of the respondents to a census or survey. Since the mortality of parents refers to a long period prior to the time of enumeration, the sequence of ${}_Nq_M$ should be distorted and should not normally reflect one single mortality level.

Brass' logit system of life tables provides a means to integrate child and adult mortality in the same two-parameter model. A slope parameter B determines the relationship between child mortality and any point indexing mortality at a later age. The sequence of ${}_Nq_M$ for successive ages N of the respondents, expressed in terms of B, allows to test the consistency of the estimates. And surprisingly, despite the theoretical objections listed above and despite the possible effect of age biases and the possible unsuitability of various fertility and mortality models used in the estimating procedures, estimates of adult mortality through the orphanhood method have been remarkably consistent. Blacker notes that:

It is quite possible for the mortality estimates to appear both plausible and consistent and yet at the same time to be seriously biased; the agreement with the alternative estimates (all of which were subject to large margins of error) may have been fortuitous, or all the estimates may have been biased in the same direction.¹⁵

Thus, the experience this far warrants the inclusion of the orphanhood question in surveys and even in censuses; it was included in the recent censuses of Botswana, Gambia, Sudan and Sierra Leone. This success has led demographers to experiment with other indirect approaches to adult mortality: from information on widowhood from first spouse, or even on sibling survival.¹⁶ These methods have not graduated yet from the experimental stage. The spouse-survival technique is at least theoretically more promising than the orphanhood technique: there is only one first spouse per person; male mortality can be estimated as reliably as female mortality; and the period over which adult respondents have been exposed to

mortality risks is smaller than in the case of parent survival. The question "Is your first husband/wife still alive?" appears easy to ask. There are of course possible biases involved in the method: for example, the mortality of widows may be higher than that of married persons.¹⁷

Thus, the trend seems towards the introduction of more and more indirect questions in the questionnaires; the displacement of the responsibility from the data collector to the data analyst; and a reliance on the "robustness" of techniques that average the biases and the effects of age. The resort to models, including model life tables, is as important as ever. The Coale and Demeny Regional Model Life Tables remain master of the field, as we are still unable to determine how African mortality patterns diverge from patterns identified in reliable data. Two-parameter systems have demonstrated their usefulness; the Brass logit system is well entrenched and Ledermann's tables are occasionally used.¹⁸

Disaggregating the data on children surviving

In this final section we are turning to a more detailed look at the question on children surviving to see if we can learn something more about the biases to which it is subjected, and more about the factors that influence mortality. Here we will go beyond the more usual practice of reporting and studying averaged summary indicators of mortality, to look at their distribution. Our intention is to indicate how crosstabulating parity and mortality by age can increase our understanding of the mortality mechanisms operating in the study population. Our investigation depends on data available in form that will permit decomposition, and in such magnitude that statistically useful number of cases by individual cell will remain after disaggregation. Here we will use a 10% sample of the 1974 Liberian Census which encompasses 44,653 women over age fourteen.¹⁹

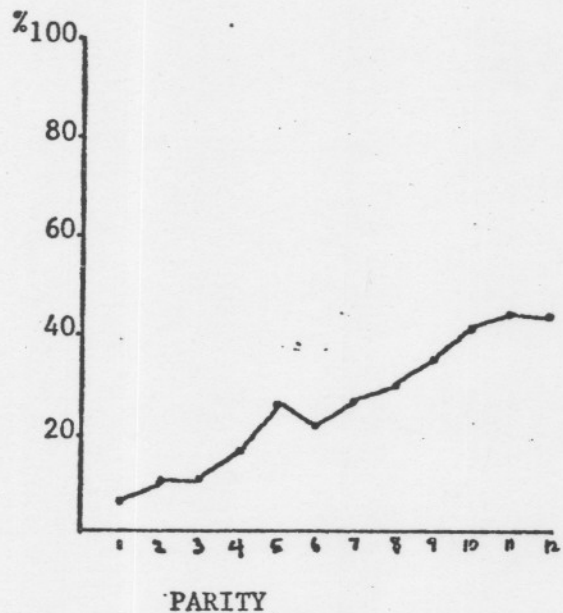
Brass shows that the mean age of all children born to women in successive age groups can be computed if we know the underlying fertility schedule; and, that the

proportion of the children who died among those ever-born to women in an age group is equivalent to a probability of dying between birth and that mean age. By translating the age of the mother into the age of the child, Brass expects a regular covariation between the child's ${}_nq_0$ and the mother's age.

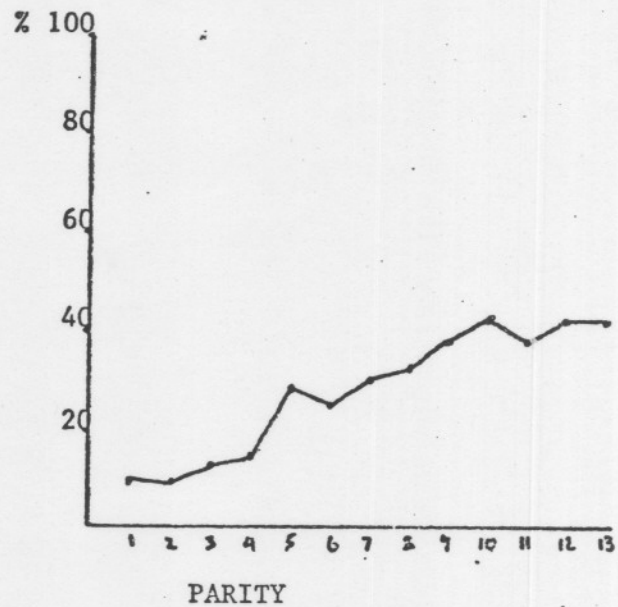
The q values obtained by applying the Brass technique to the Liberian data conform to level 12 of the North model life tables. In this section, we attempt to disaggregate the proportions dead children by age and parity of the mothers. Figure 1 and Appendix Table 1 show that at each age mortality is a linear function of the parity reached by the women. The story remains essentially the same for higher and lower ages. The results presented in Figure 2 and Appendix Table 1, however, are more surprising. In each parity group the proportion dead among children, which is equivalent to the child's ${}_aq_0$ value, should increase regularly with the age of the mother. Not only is this not observed by parity of mother but the " q " curves look relatively flat or even "U" shaped. They begin too low and do not increase sharply enough. As parity increases the curves' levels increase, a result that is expected from Figure 1. The assumption that the average age of children increases with the age of their mother, should be true even when parity is kept constant. One would expect high mortality of the children of young women at high parities, and therefore a "U" or even a "J" shape of the curve. On the whole, however, the disaggregated data indicate that the summary curve's level and shape may not reflect the Brass relationship between age of mother, age of child and the child's ${}_aq_0$; but rather it may reflect mostly the greater covariation between parity of mother and child mortality. In other terms, the reason the ${}_aq_0$ values vary as expected may not be that the children of older mothers are themselves older, but that older women are of higher average parity. If this is so it is conceivable that a set of multipliers could be developed to relate the proportion dead children by parity of mother to ${}_aq_0$ values. This would be a great benefit in that it would require only survey questions on the woman's number of live births and number of surviving

FIGURE 1: PROPORTION DEAD CHILDREN BY PARITY OF MOTHER WITH AGE CONTROLLED

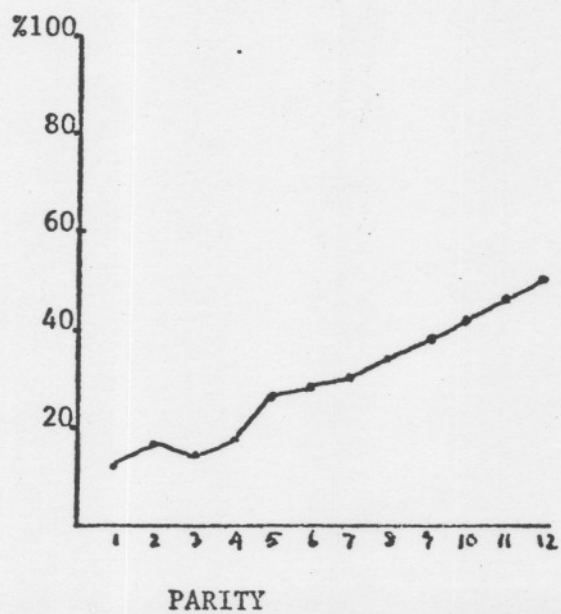
Age Group 35-39



Age Group 40-44



Age Group 45-49



Age Group 50-54

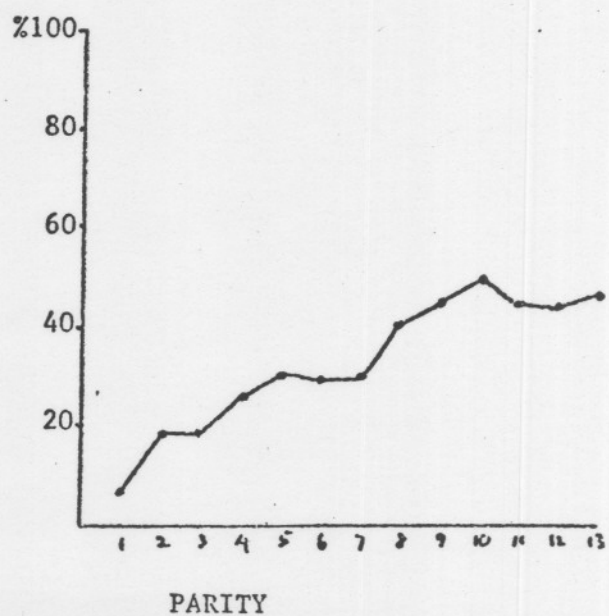
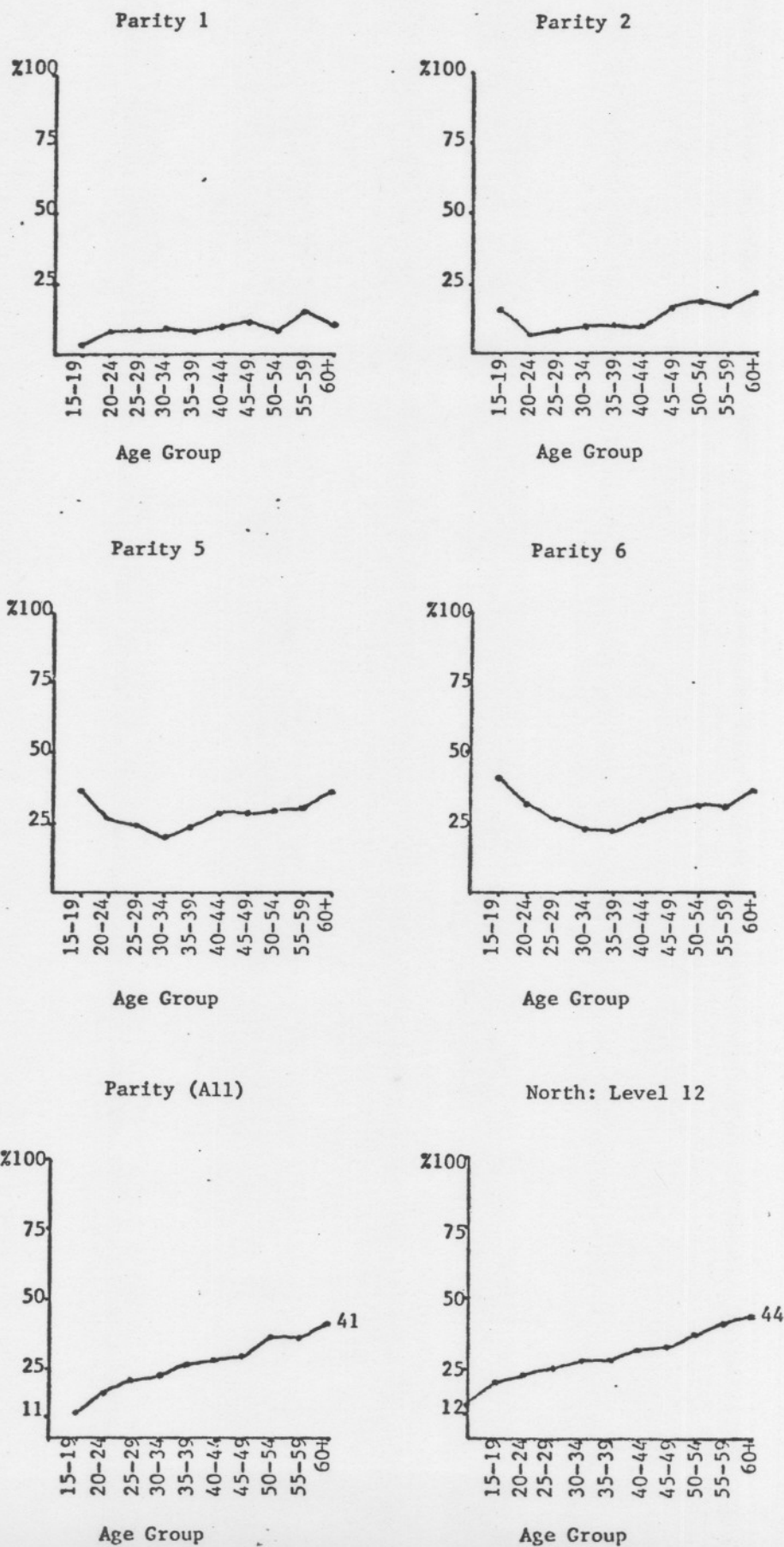


FIGURE 2: PROPORTION DEAD CHILDREN BY AGE OF MOTHER WITH PARITY CONTROLLED: AND, ${}_a q_0$ BY AGE OF CHILD FOR NORTH (12) LIFE TABLE



children and the biases and difficulties with age reporting might be avoided.

Before going too far afield with these speculations based on the data presented in Figure 1, it would be prudent to subject these data to careful analysis to confirm or deny their validity and to identify the mechanisms that might have produced them. We will give a few exploratory examples based on disaggregation of the data to suggest the approach that might prove useful.

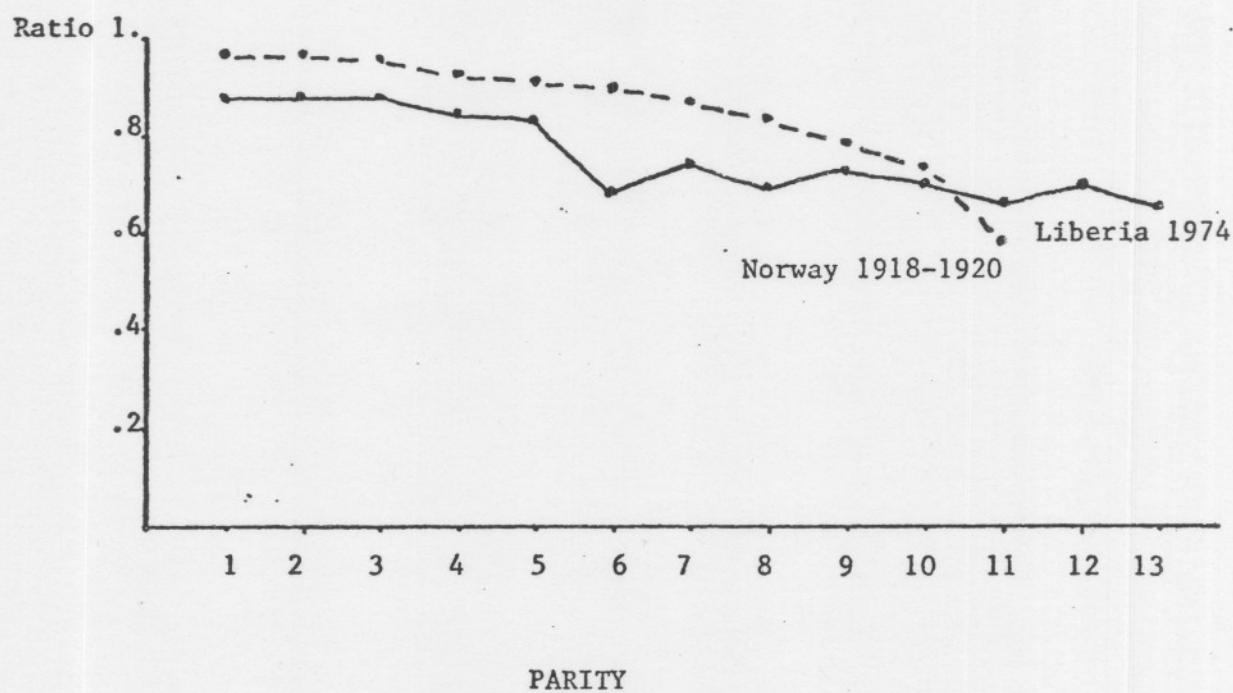
The three components of the Brass question: age, parity, and mortality, are each subject to independent and joint response bias. In any survey bias on age is ever present. The Liberian data show the expected great fluctuations and heaping about a declining age distribution and there is little point in dwelling much further on this familiar problem, except to say that age misreporting affects our curves. Our discussion will therefore concentrate on the fertility and mortality responses. We will consider bias in reporting live births only as it affects mortality rates and will not develop an extensive criticism of the fertility data themselves.

In Figure 3 and Appendix Table 1 we show parity progression ratios for Liberian women in the age groups 50-54 and 55-59. Parity progression ratios are the proportions of women with at least n children, who have at least $n + 1$. As a comparison, Figure 3 also plots the corresponding values in a 19th century natural fertility population with accurate data.²⁰ The Liberian series is consistent, despite a tendency to "unheap" on parity six (which appears at all ages); it is lower than the Norwegian series for low parities, but this is to be expected because Liberian women have not necessarily been continuously married and the Norwegian women have. More surprising is the continuation of the ratios at a high level at superior parities; this might reflect a tendency for women to overreport the number of children ever-born.

Figure 4 presents the percent of women having at least 1, 2 ... 12 children, by age. The evolution of the curves above age 40 may reflect a change of fertility

FIGURE 3: PARITY PROGRESSION RATIOS

LIBERIA: Age 50-54; NORWAY: Age 50



LIBERIA: Age 55-59; NORWAY Age 50

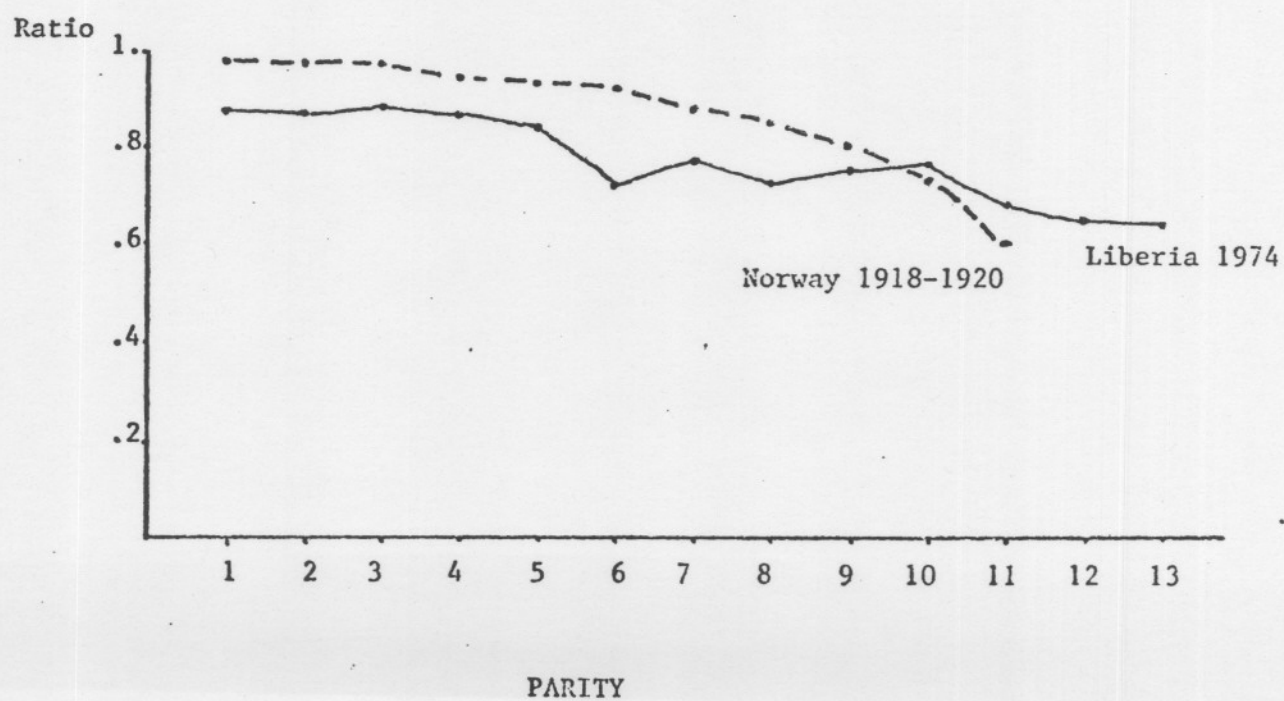
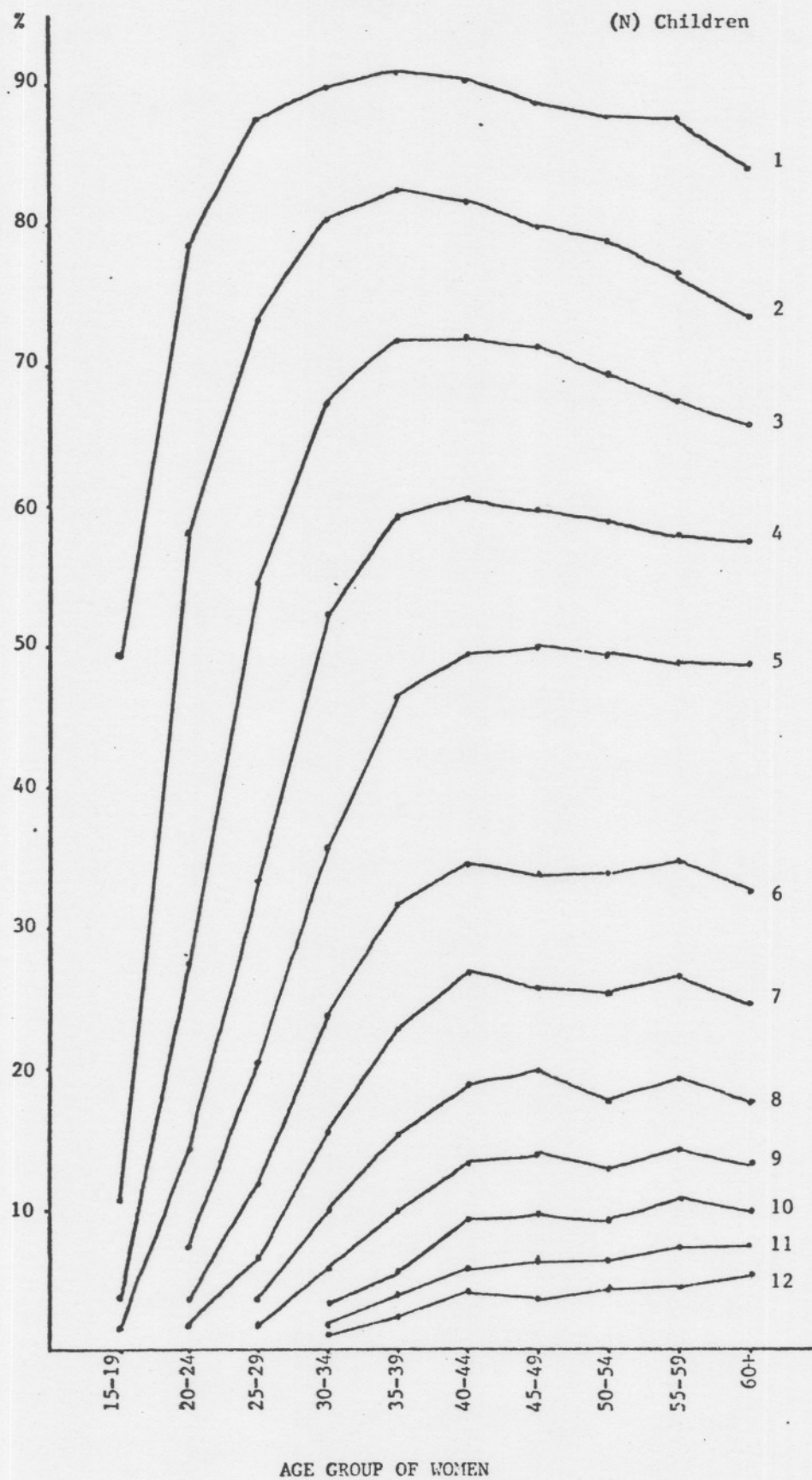


FIGURE 4: PROPORTION WOMEN WITH AT LEAST (N) CHILDREN BY AGE



over time; or again, it may be the result of a pattern of forgetting by older women. That some process of overstatement of low parities (particularly parity zero) is at work, is suggested by the shape of the curves representing women having at least 5, or more, children. A number of explanations for the pattern could be advanced, but none conclusively proved at this stage. In view of the low level of mortality revealed by the Brass question, it is possible that dead children have been omitted in answers to the question on children ever-born. If so, we should learn something from the detailed tabulation of the number of dead children for women of each parity by age. Such a tabulation is illustrated in Figure 5.

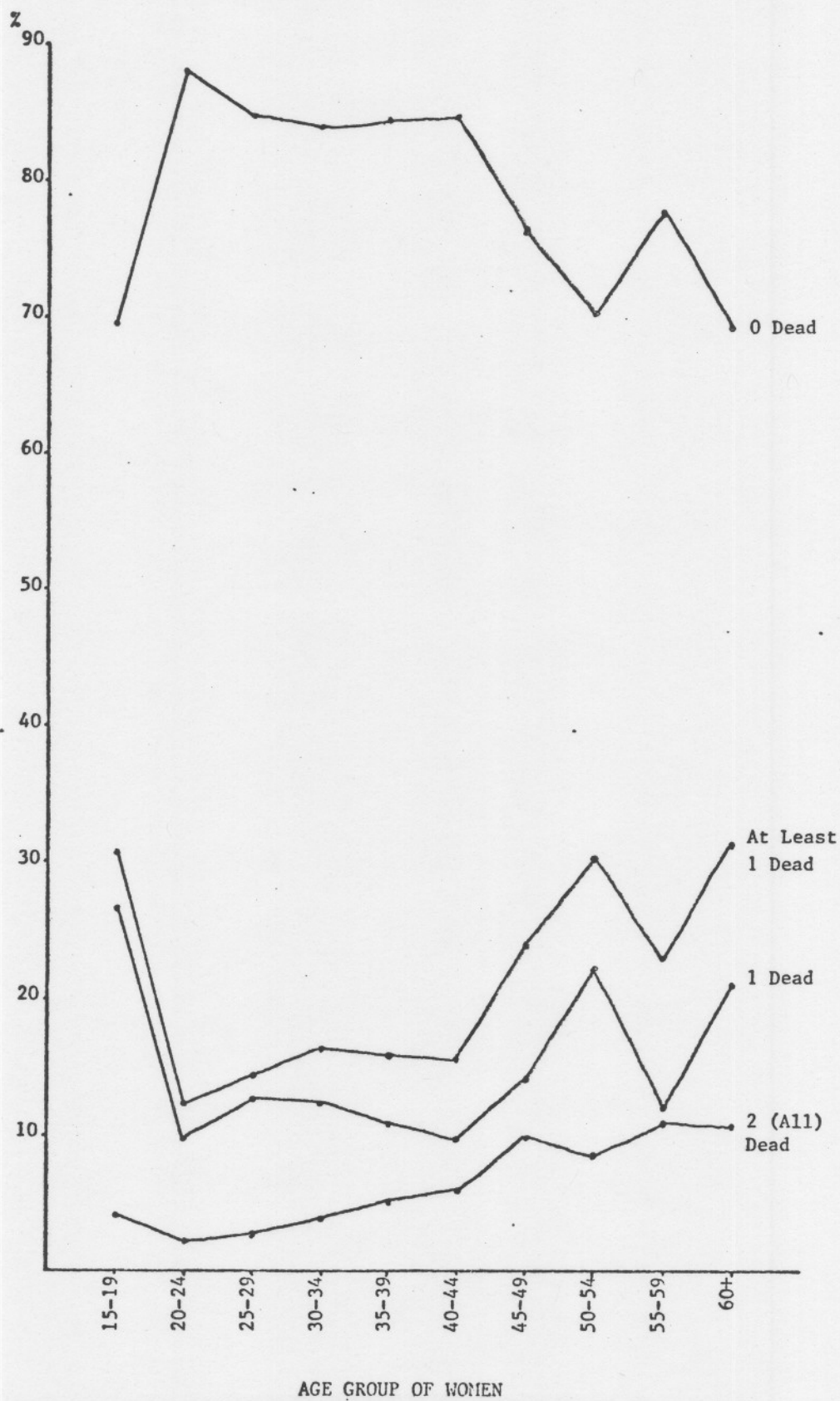
Conclusion

We have tried to indicate with the Liberian data how it may be possible to learn a great deal about the location and nature of bias by subjecting data to intense disaggregation and scrutiny. These data were generated with simple cross-tabulations of data that are already collected.

Detailed analysis is limited by the lack of knowledge of the real expected distributions. These data must be developed through the systematic assembly of the results of large-scale field surveys and censuses to provide sufficient cases for analysis. Statistical modelling of the relationships will also help and these too must be compared to the empirical results of large-scale surveys and censuses. In the future, if we are to fully understand the mechanisms underlying the shape and levels of mortality in this region we will need large amounts of discrete data.

This then is an appeal for the systematic tabulation of the data collected in large-scale surveys and censuses. To make sense, the disaggregation of parity and child survival data must rest on a large number of cases, and this can be obtained only from census data. On the other hand, census offices would be tied down if they had to provide all these minute tabulations. It is best to make raw data available to demographers. In most countries, a public use sample drawn from the

FIGURE 5: PERCENT WOMEN AT PARITY TWO BY AGE WITH (D) DEAD CHILDREN



census would provide a large enough number of cases for most purposes. In most countries of the world, such public use sample tapes are made available to researchers as a matter of course. We hope that the example of Liberia will be followed by other countries.

In presenting results from Liberia, we had to abstain from definitive conclusions. Further work is clearly in order. The data suggest a peculiar pattern of child mortality, more dependent on parity than on age. If these results are verified elsewhere, they would suggest that high parities are one of the main determinants of mortality in Tropical Africa--a finding with serious policy implications. If, however, as we fear, the data reflect reporting bias and omissions of dead children from the mothers' responses, then a detailed distribution of the parities and proportions dead children may provide an important insight into these biases and the way to correct them.

- ¹J.G.C. Blacker, "The Estimation of Adult Mortality in Africa from Data on Orphanhood," Population Studies, vol. 31, no. 1, March 1977, p. 107.
- ²Etienne van de Walle, "The Role of Multi-Round Surveys in the Strategy of Demographic Research," in P. Cantrelle, ed., Population in African Development, vol. 1, pp. 301-308.
- ³S.K. Gaisie, "Levels and Patterns of Infant and Child Mortality in Ghana," Demography, vol. 12, no. 1, February 1975, pp. 21-34.
- ⁴Francis Gendreau, "La démographie des pays d'Afrique--Revue et synthèse," Population, nos. 4-5, 1977, pp. 899-943.
- ⁵For example of such mortality surveys, see those reviewed in P. Cantrelle, "Is There a Standard Pattern of Tropical Mortality?" in P. Cantrelle, ed., op.cit., pp. 33-42; and Anne Retel-Laurentin and Daniel Benoit, "Infant Mortality and Birth Intervals," Population Studies, vol. 30, no. 2, July 1976, pp. 279-293.
- ⁶The method has been described in United Nations' Manual IV, Methods of Estimating Basic Demographic Measures from Incomplete Data, Population Studies No. 42, New York, 1967, pp. 7-12 and 57-61.
- ⁷Another variant which does not require model life tables as inputs is described in William Brass, Methods for Estimating Fertility and Mortality from Limited and Defective Data, an occasional publication of the International Program of Laboratories for Population Statistics, University of North Carolina, Chapel Hill, pp. 106-116.
- ⁸Blacker, op.cit., p. 107.
- ⁹The theoretical justification of the method is given in W. Brass et al., The Demography of Tropical Africa, pp. 104-112. A simple guide to computational procedures appears in U.N. Manual IV, op.cit., pp. 7-12 and 57-61.
- ¹⁰J. Sullivan, "Models for the Estimation of the Probability of Dying Between Birth and Exact Ages of Early Childhood," Population Studies, vol. 26, no. 1, March 1972, pp. 79-97; J. Trussell, "A Re-estimation of the Multiplying Factors for the Brass Technique for Determining Childhood Survivorship Rates," Population Studies, vol. 29, no. 1, March 1975, pp. 97-107.
- ¹¹Cantrelle, op.cit.
- ¹²Blacker, op.cit.
- ¹³William Brass and Kenneth H. Hill, "Estimating Adult Mortality from Orphanhood," International Population Conference, vol. iii, Liège, 1973, pp. 111-123; Kenneth H. Hill and James Trussell, "Further Developments in Indirect Mortality Estimation," Population Studies, vol. 31, no. 2, July 1977, pp. 313-333.
- ¹⁴Blacker, op.cit., p. 107.
- ¹⁵Idem, p. 126.
- ¹⁶Hill and Trussell, op.cit.

- ¹⁷ Kenneth Hill, "Estimating Adult Mortality Levels from Information on Widowhood," Population Studies, vol. 31, no. 1, March 1977, pp. 75-84.
- ¹⁸ H.J. Page and G. Wunsch, "Parental Survival Data: Some Results of the Application of Ledermann's Model Life Table," Population Studies, vol. 30, no. 1, March 1976, pp. 59-76.
- ¹⁹ We acknowledge the generous access given us by the Ministry of Planning and Economic Affairs of the Republic of Liberia to a 10% sample of the individual census returns. This sample is the basis for the original analysis of the Liberian data presented in this paper.
- ²⁰ The series is for Norwegian women aged 50 years, continuously married since age 20-21, as of 1888-90. See, Louis Henry, Population: Analysis and Models, Arnold, London, 1976, p. 89.