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**The Study of Fertility and Fertility
Change in Tropical Africa**

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The Study of Fertility and Fertility Change in Tropical Africa

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The Study of Fertility and Fertility Change in Tropical Africa

Summary

The theme of this paper is that we still have a great deal to learn about fertility and fertility trends in tropical Africa. The most fundamental barrier at present is not so much inadequate data as imperfect field research. Even this is caused less by the absence of adequate techniques than by the failure to make the fullest effort in applying the techniques that are available. Africa is not as unknowable as some of our present demographic data would imply.

The large-scale data collection efforts are not going to give us reasonably certain fertility levels or detected fertility trends in the near future. In the meantime, a good deal can be learnt about the forces that sustain high fertility, and factors that may lead to changing fertility, from smaller studies that ask intelligent questions and devise research of sufficient calibre to secure some of the answers.

Data collection and analysis

Twenty years ago we knew almost nothing about fertility levels and trends in tropical Africa and were aware of our ignorance. Today we know a great deal more, but may be more deluded about how much we do know.

The dilemma has arisen because the paucity of data provoked into existence demographic surveys which are quite unique both in number and for the range of contiguous countries with comparable methodology and comparative analytical reports.¹ The majority of these tropical African fertility surveys have been those carried out in francophone countries by or with the cooperation of INSEE beginning with the Guinea Survey of 1954-5, although censuses have increasingly investigated fertility either for the whole population or subsamples in anglophone Africa as well. The data from these surveys and censuses were the material for which both the Brass and stable population methods of fertility analysis were first evolved.² The tropical African population data have so richly fathered invention partly because of their very pooriness and partly because of the early stage of demographic transition in the region:

¹ So great has been the impact of French government organizations on sample surveying in Africa that one-fifth of all the fertility and related surveys listed by the International Statistical Programs Center of the U.S. Bureau of Statistics as having been done in the developing world since 1960 have belonged to this group. This was the main reason that 42 per cent of all the surveys had been done in Africa (William G. Duncan, "The Nature and Content of Fertility Surveys Conducted Throughout the World Since 1960", *Occasional Papers*, No. 1, World Fertility Survey, London, October 1973). The comparative reports for the ex-French countries of tropical Africa have appeared under the INSEE series title, *Afrique Noire, Madagascar, Comores: Démographie-Comparée*. In anglophone tropical Africa the collection of some fertility data began with the 1948 Censuses in the Gold Coast and East Africa but data of the type available for francophone countries did not begin to be gathered until the 1957 Tanganyikan Census.

² The former are associated with William Brass and the latter in the form in which they are usually applied in Africa with Ansley J. Coale and Paul Demeny. Both are described and applied in William Brass *et. al.* *The Demography of Tropical Africa*, Princeton University Press, Princeton, 1968, and (Ansley J. Coale and Paul Demeny) United Nations, *Manual IV: Methods of Estimating Basic Demographic Measures from Incomplete Data*, New York, 1967.

vital registration schemes are non-existent, the survey and census data are so incomplete that vital rates worked out directly from the data are of little value, and fertility is probably close to being constant.

Without question these new methods handle bad data surprisingly well. The Brass estimates of fertility based on a comparison of the short and long term retrospective fertility of individual women draws on an essentially different body of material from the age structures of the whole community analysed by the Coale-Demeny approach, and yet, when they can be compared, correlation coefficients of around 0.9 have been shown.³

There remain problems especially if the focus of interest is fertility change. These problems can be grouped under four headings: methodological, impact on data collection, the detection of fertility change and the prediction of fertility change.

At some stage in each method it is assumed that mortality patterns are broadly similar to those found at similar levels of life expectation in other parts of the world. This has been contested,⁴ and it is possible that tropical African rates of natural increase are based on higher birth rates and higher infant and child mortality rates than we now believe, and hence that there is a potential for higher rates of natural increase than we have assumed. Both approaches are inevitably affected by bad data, especially by age data which suffers from more than predictable systematic errors in the direction of age misstatement. This means, for instance, that stable population analysis can hardly ever find a close match between an enumerated and a model population but must match various segments of the population (usually cumulated proportions to certain ages) which yield different fertility estimates from the same age structure. The Brass method includes various assumptions, few of which have ever been tested in the field (designing adequate tests is difficult)⁵: relatively high accuracy of parity data for young women, a similar error in gauging the short retrospective period for "current" fertility by younger and older women, and a non-erratic relation between parity and current fertility figures. As applied in Africa, both approaches usually employ Brass estimates of child mortality,⁶ and these depend for their accuracy on empirical relationships observed outside Africa and hardly tested or yet testable in tropical Africa.

The arrival of these sophisticated methods, which look their best when great amounts of computer print-out yield findings for a whole series of regional populations, have had two quite distinct effects on data collection. The first, which is all to the good, has been an increasing pressure to add well designed questions, for collecting the information the methods require, to censuses or related surveys and to other demographic enquiries. This is at present much in evidence in the "1970" census round (although plans for surveys including the necessary fertility questions are at present much less advanced than plans for the basic censuses) and in plans for the World Fertility Survey. What is less often observed is that this has frequently not been accompanied by a similar advance in collecting or trying to collect accurate data. My impression from talking to a wide range of statisticians and demographers across tropical Africa is that many are no longer afraid that inadequate data will yield only

³ Hilary Page, "Fertility Levels: Patterns and Trends", in John C. Caldwell *et al.*, *Population Growth and Socio-economic Change in West Africa*, Columbia University Press, New York, 1974.

⁴ Pierre Cantrelle, "Mortality levels, Patterns and Trends", in *Population Growth and Socio-economic Change . . .*, *op. cit.*

⁵ An attempt is being made to test some of the assumptions in a project at the University of Nigeria, Nsukka.

⁶ Brass *et al.*, *op. cit.*, pp. 104-120.

meaningless results; indeed many apparently think that extra time, effort and money may only marginally improve data and that the new methods (especially the Brass fertility one) will probably yield just as good estimates from the poorer material as they will from the somewhat better data. However, it can now be shown that better data can be collected by printed questionnaires translated before the enquiry into local languages (and not on the spot by interviewers) and designed so that the questions follow the ways of thinking, and indeed the ways of etiquette, of the people of the locality. More can be done on age sets and other methods of optimizing determination of age. More can be done in interviewer training, especially to break the dangerous association in the minds of interviewers between parity and age estimates. The last decade has seen some experimentation but largescale data collection has hardly improved since the 1960 census round.

The new methodology cannot cope very well with measuring the kind of limited fertility change which is likely to mark the early stages of sustained fertility change. Indeed, the methods are all based on the assumption of the *stability* of the population, and, although adjustments can be made to allow for fertility change which has been proved in some other way, they are much more likely to "correct" limited fertility change out of existence than to detect it. More serious is the variation between estimates for the same population at the same time which is often greater than the variation that a real fertility decline would be likely to achieve in ten or even twenty years. For instance, the difference between stable and Brass birth rate estimates for Ghana derived from the 1960 census data is 7 points⁷ or 13 per cent of the higher figure, the kind of absolute difference achieved during fertility decline by France between 1850 and 1900 or England and Wales between 1871 and 1901. In fact the bad data are unlikely to allow any fertility decline to be definitely established until well after it has begun.

The methods, apart from showing that fertility change has begun, were not designed to predict the onset of fertility decline and do not employ data that can easily be used for this purpose (although the Brass methods can be employed, but usually are not because of the small cell size that would follow from the number of respondents, to demonstrate the fertility of various subpopulations and hence to establish what may be portentous fertility differentials). Nor do they in any way explain the fertility levels or differentials which already exist.

Yet there is both an intellectual and an administrative demand for further information which might help to explain the very high levels of fertility in much of tropical Africa (and the relatively low level in certain areas⁸) and which might be used for predictive purposes. The INSEE programme had limited objectives of this kind from the mid-1950s but surveys which also investigated practices which might restrict fertility date only from 1963 since which close to 50 surveys of this type have been undertaken in tropical Africa.⁹

The surveys, most of which could be described as KAP surveys, have also had problems and are probably of more uneven quality than the more strictly demographic surveys. Some samples were deficient; in the earlier period samples were usually not national in scope; there was much better coverage of urban than rural population; and the coverage of anglophone

⁷ Page, *op. cit.*

⁸ A programme for work on these areas was formulated between the United Nations Economic Commission for Africa and the University of Ibadan Medical School in November, 1973.

⁹ For a list of 37 in West and Middle Africa, which have been used for a comparative analysis, see John C. Caldwell, "Fertility Control" in *Population Growth and Socio-economic Change . . .*, *op. cit.* In addition others have been held in East Africa (partially listed in John C. Caldwell, "The Control of Family Size in Tropical Africa", *Demography*, V, 2, 1968, pp. 598-619) and more recently in Nigeria.

countries remains much more complete than that of francophone ones (a position that may change with the appearance of a number of new universities during the 1970s in francophone countries previously lacking them). To some extent offsetting these drawbacks was the fact that questionnaires were more likely to be in the local language and the interviews tended to be done in depth often by a relatively small number of interviewers who had acquired a good deal of interest in the work. More serious problems centred on interviewing techniques on matters that were sensitive or difficult to conceptualize. The former is a problem which is shared with more strictly demographic surveys and censuses; recent work (described below) in Ibadan suggests strongly that so-called "memory lapse" resulting in the omission of dead (and, to a lesser extent, live) children arises to a very considerable extent from interviewer gaucheness in dealing with such matters. More attention to the way births and deaths are handled in everyday local conversation in the vernacular can reduce these omissions, as can longer and more discursive interviews. But KAP surveys have to go further into areas where survey organizers as well as inadequately trained interviewers are often unsure of themselves. Certainly KAP interviewers in tropical Africa have not all obtained as much information about the use of *withdrawal* as they could have done, and sometimes they have not been told clearly just how they are to identify the method and discuss it. Often organizers have not been clear in their own minds about what they classed as an antinatal measure and what they did not – what periods of abstinence or lactation, how infrequent sexual relations had to be, the position of the new mother who goes back to her ancestral family for a period, post-natal taboos apparently followed solely as a kind of hygienic law, and especially the use of charms or herbs prescribed by native doctors which often rouses scepticism in foreigners. Yet, as with the measurement of fertility, some different surveys of the same population were reassuring. The best examined case was that of Lagos, which was independently examined in 1969 by researchers from the Lagos University Medical School and the University of Ife Demographic Training and Research Unit. At first sight the levels of family planning practised appeared to be irreconcilable, but further examination showed a close correlation between the proportions of the population using each method of modern contraception (even the level of use of withdrawal). The difference lay, and still lies, in disagreement between the directors of the two surveys as to the extent to which the observation of the post-natal taboo and of lengthy lactation, for reasons stated to be unconnected with any desire not to conceive, again can be regarded as family planning.¹⁰

A summary of the present position

Since the Princeton University Office of Population Research group began employing Brass and stable population methods to analyse tropical African data, the patterns of fertility have appeared in four different publications.¹¹ There has been no radical change, but additions and

¹⁰ The debate appears in two chapters of *Population Growth and Socio-Economic Change . . .*, *op. cit.*: Robert W. Morgan (including a Report by P. O. Ohadike), "Nigeria: Fertility Levels and Fertility Change", and John C. Caldwell, "Fertility Control".

¹¹ (1) Ansley J. Coale and Frank Lorimer, "Summary of Estimates of Fertility and Mortality", in William Brass *et. al.*, *The Demography of Tropical Africa*, Princeton University Press, Princeton, 1968, pp. 151–167; (2) A. J. Coale, "Estimates of Fertility and Mortality", in John C. Caldwell and Chukuka Okonjo (eds), *The Population of Tropical Africa*, Longmans, London, 1968, pp. 179–186; (3) H. J. Page and A. J. Coale, "Fertility and Child Mortality South of the Sahara", in S. H. Ominde and C. N. Ejiogu, *Population Growth and Economic Development in Africa*, Heinemann, London, 1972; (4) Page, "Fertility Levels: Patterns and Trends", *op. cit.*

amendments have been made and accordingly the most recent version will be used here. For most of West and East Africa the total fertility ratio (TFR), which must approximate completed family size in this region, is over six and for very substantial populations over seven. Fertility is not quite as high on the East African coast or in southern Angola, nevertheless, the TFR remains above five everywhere in these areas. The anomalous region is one bounded to the south by the Zaire-Kasai watershed, to the west by the Atlantic Coast and the Cameroon Highlands, to the east by the East African highlands, while to the north it goes for some distance into Sudan and reaches beyond the area of surveys towards the Sahara. For the most part it is an area of relatively low population density. Even here there is an island of higher fertility embracing part of southern Chad and western Central African Republic while, if we drop the dividing line between higher and lower fertility to a TFR of $5\frac{1}{2}$, the low fertility area is divided into two by a ridge embracing all Congo (Brazzaville), western Central African Republic and all surveyed Chad. It is this tendency for demographic measures to change at national boundaries that provides one of the worries about the state of our demographic knowledge. Nevertheless, the low fertility area is real: it is reflected where there have been two counts in low population growth rates; the societies of the region are acutely aware of infecundity and subfecundity as being common problems; and some of the governments tend to be unusually pronatalist (although this may stem from the findings of demographers).

By 1970 the KAP data appeared to be sufficient to provide a similar synthesis, which unfortunately had to be confined to West and Middle Africa, but which probably largely applies to East Africa as well. This summary is drawn from that study.¹² The most reassuring aspect of the KAP survey data is the consistent way many measures change with change in certain other socio-economic variables.

The most significant finding is that tropical African fertility does not soar far above stated desired family size as if it were evidencing the problems of societies with little access to contraceptives. On the contrary, the region, in contrast apparently with the rest of the world (and confirming our confidence in the surveys), evidences very high "ideal family size".¹³ In both urban and rural populations ideal or desired family size usually exceeds six and in the latter seven is more often exceeded than not (these are figures for women – among males who favour polygyny male ideal fertility is much higher). For populations as a whole there is little evidence that present high levels of fertility are felt to be undesirably high.

However, some minorities are beginning to evince a desire for fewer children, and they are precisely those who tend to provide leadership in matters of political or social change: the most urbanized, the most educated and those employed in the modern sector of the economy (obviously not distinctly different groups). Few tropical Africans want smaller families than four children, but the proportion of parents wishing to stop at four is now probably over a quarter in many of the larger cities and usually exceeds a third among the elites found there. In Ghana and Nigeria, where we have comparative figures on ideal family size over time, there is clear evidence that the proportions desiring smaller families are increasing – perhaps in Ghana doubling (from a very low base) every five years.

¹² Caldwell, "Fertility Control", *op. cit.*

¹³ Tropical African data from Caldwell, "The Control of Family Size in Tropical Africa", *op. cit.* and "Fertility Control", *op. cit.* and international data from Bernard Berelson, "KAP Studies on Fertility", in Bernard Berelson *et. al.* (eds), *Family Planning and Population Programs: A Review of World Developments*, University of Chicago Press, Chicago, 1966, p. 658.

Nevertheless, the social scientist seeking evidence of likely behavioural change, must remain concerned about using attitudinal data for two reasons. The first is that, in the area of fertility control, measured attitudes often are at considerable variance with measured behaviour, either because people are so inconsistent that many behave quite differently from the way that they feel they should or would like to behave or more probably because our measurements of attitudes are deficient. Perhaps we do not get the concepts clearly across to all respondents or perhaps the concepts are not *real* in their society – one does not have attitudes on such matters or the control of such matters is not up to the individual – or perhaps our own prejudices are apparent and the respondents try to please us. The second is that, even if behavioural change follows attitudinal change there is likely to be a time-lag, and for fertility transition we have little satisfactory work on the magnitude of the lag. As discussed later, we also have far too little on the rationality of these attitudes to family size.

Hence fertility research in Africa is clearly shifting towards a greater concentration on measuring behaviour. In order to detect early movement towards fertility decline this means not only attempting to measure fertility itself but also antinatal practice and the link between antinatal practice and reduced fertility. The measurement of the impact antinatal practice makes on fertility is vital; without it much KAP measurement appears to be pointless. Yet little has been done in tropical Africa. Admittedly, this may be partly because of the assumption that reduced fertility will inevitably follow increased antinatal practice and that contraception will be used more efficiently with the passing of time and the spreading of a small family system thus rendering futile any attempt at establishing a fixed relationship between the two. The only published material for tropical Africa examines females in Accra, Ghana, in 1965–6, with results that were not entirely clear but which did seem to show that, up to 35 years of age, women who used any antinatal method were likely to be less fertile than those who had not tried to restrict fertility, while those who had ever used modern contraception had lower fertility still. The fertility of the latter group, as measured by parity, was one-third below that of those who had never used any practice.¹⁴ The fact that a clear pattern failed to emerge for older women may have been a product of the relatively small number of them who had employed anti-natal practices and the likelihood that a substantial number of them were woman of high parity before beginning such practice who were desperately trying to prevent any further births.

In contrast, there is now a good deal of material on contraceptive practice but nothing satisfactory on abortion. The evidence¹⁵ seems to indicate that the use of traditional and non-appliance contraceptive methods may increase to some extent, especially in the towns and perhaps as a transitional stage before experimentation with modern contraceptives, but that the future path of fertility will be determined very largely by the rate of spread of the use of modern contraceptives, particularly orals, IUDs and condoms. For predictive purposes a satisfactory approach would appear to be that of concentrating on usage levels and changes over time of modern contraceptives and on the evidence provided by differentials now existing in their use.

¹⁴ Data from D. Ian Pool, "Ghana: Fertility Notes" (mimeographed, May, 1970) reproduced in Caldwell, "Fertility Control", *op. cit.* The reduction in parity was recorded for the 25–34 years of age group. The picture presumably would have been clearer had this group been subdivided into the conventional five-year age groups, which have been possible with a total of 595 respondents of whom 54 had employed an antinatal practice and 24 had used modern contraception.

¹⁵ The discussion below is drawn from data summarized in Caldwell, "Fertility Control", *op. cit.*

Around 1970, at least in West and Middle Africa, knowledge, past use and current use of modern contraceptives was higher in anglophone than francophone countries, higher in coastal than inland areas, and higher in urban than rural areas. Thus, in the rural areas of francophone, savannah Africa, one woman in two hundred of reproductive age in conjugal unions had any real knowledge of modern contraceptives and use was infinitesimal, while in the coastal cities of anglophone Africa (Lagos, Ibadan, Accra-Tema, Kumasi, Takoradi-Sekondi, Freetown, etc.) probably 60 per cent of women had sufficient knowledge to allow them to obtain and use contraceptives if they wished to, 10 per cent had done so and 5 per cent were currently using contraceptives (compared for instance with past and current use rates in Australia of 87 and 66 per cent respectively¹⁶). This meant that in all of West and Middle Africa, a region with around 140 million inhabitants of whom perhaps 25 million are women of reproductive age currently in conjugal unions, 271 thousand or 1.1 per cent had ever used modern contraceptives and 125 thousand or 0.5 per cent were currently doing so. Obviously the impact on fertility was hardly likely to be measurable. But this was not equally true for smaller subdivisions of the population. Of those currently using modern contraception, half were to be found among the 1¼ million women in the major urban areas in the coastal areas of anglophone countries, a situation which alone may well have reduced the birth rates there by one or two points (disregarding the impact of abortion and traditional contraceptive practices also tending to reduce fertility or of other factors possibly tending to raise it).

In projecting contraceptive practice the most useful information is obviously that on recent increases in practice. Here, our knowledge is limited to Lagos between 1964 and 1970, where measurements exist at three different dates. A plausible interpretation of these findings is that the use of modern contraceptives is doubling about every five years so that the proportion of married women currently using them within marriage would reach one-quarter by the late 1980s and one-half by the early 1990s (unless major governmental intervention were to take place in the provision of contraceptives and the encouragement of their use, as seems very likely). This interpretation is supported by the retrospective data from a north-south cross-sectional survey of Nigeria carried out in 1969.¹⁷ Data over much the same period for the use of any form of family planning exist for both Accra and Lagos and generally support the picture drawn above. We also have information on the knowledge of modern contraceptive methods at three different dates for Lagos and of family planning methods in general at two different dates for Lagos, Accra and rural Ghana, as well as the retrospective study in Nigeria. All demonstrate that there has been a knowledge explosion of a very different order to that exhibited by contraceptive use, so that, if sustained at the same rate, nearly all women in Lagos and Accra will know something of family planning well before the end of the 1970s and in rural Ghana by about 1980. Since then the latter has probably been ensured by the establishment of a national family planning programme in Ghana. That the knowledge of the

¹⁶ J. C. Caldwell and H. Ware, "The Evolution of Family Planning in Australia", *Population Studies*, XXVII, 1, March 1973, pp. 13-14. Australian data is employed because at the time of writing there was no current recent data for the United States. Past use in the United States for women under 45 was 84 per cent in 1970 (C. F. Westoff, "The Modernization of U.S. Contraceptive Practice", *Family Planning Perspectives*, IV, 3, July 1972, pp. 9-12), while the Australian figure for the same group in 1971 was 89 per cent. Other evidence shows even greater similarities in levels of contraceptive practice.

¹⁷ John C. Caldwell and Adenola A. Igun, "The Spread of Antinatal Knowledge and Practice in Nigeria", *Population Studies*, XXIV, 1, March, 1970, p. 31.

existence of modern contraceptives and their purpose in spreading rapidly in cities of this kind and will soon become universal knowledge has been confirmed by survey work during 1973 in Ibadan in southern Nigeria.¹⁸

Most differentials in knowledge and use of contraceptives are not only clear and indeed marked but are also confirmed by nearly every survey. Knowledge and use and other KAP indicators decline from anglophone to francophone countries, and from metropolitan to other large urban, and progressively to small urban and to village populations. A similar decline occurs from the coast to the interior of the continent, as is the case with all other measures of social change, for in recent generations most externally derived cultural change has been imported via the ports and has flowed inland from there to more traditional populations. Decline in knowledge and even more steeply in use follows other socio-economic gradients: from elite to traditional, rich to poor, most educated to least educated, and non-manual to urban manual to farming occupations. The educational differentials are steep, especially the contrast between those with less than secondary education and those with further education: probably a majority of women with tertiary education have used modern contraception or by now are currently using it. Religion is of smaller significance than many have supposed, and the differentials that do exist tend to become smaller when standardization is carried out for education and other socio-economic and residential factors. The sex differential is significant and will probably prove as historically significant as it appears to have been in Europe: men have had more experience with contraception because of extra-marital relationships especially with prostitutes and this experience may well be imported increasingly into their marriages.

A look at the future

The existing data do allow speculative projections of contraceptive use from 1970 until 1980. The current use of modern contraceptives in West and Middle Africa is projected to rise from 0.5 per cent of all potentially reproductive wives in 1970 to 1.6 per cent if there is no further governmental intervention in the field, to 3.1 per cent if government family planning programmes continue to spread with some acceleration over the experience of the past decade (the most plausible assumption at this time), and to 4.7 per cent if crash programmes or programmes of the Korean intensity become common. Probably more than anywhere in the world, future family planning trends are likely to be determined in Africa by the extent of government intervention – as indeed are health and mortality trends. The medium projection implies a multiplication of the proportion of women using contraception of six times and of the number of women doing so of eight times, the discrepancy being accounted for by population growth. Thus a million marriages are likely to be using contraceptives by that date. Three-quarters of the anticipated rise in the number of contraceptors is explained by the assumed rise in the proportion of women using contraception in either urban or rural areas, one-sixth by the increase of the number of women of this age because of population growth and one-sixteenth by the change in urban-rural population balance. Disregarding population

¹⁸ Part of the Changing African Family Project described below.

growth (but not the change in urban-rural balance) and hence examining only the rise in the proportion of contraceptors, less than one-tenth of the increase can be explained by intensified urbanization. A somewhat larger proportion can be attributed to socio-economic change – rising educational levels, changed occupational structure and so on. But the majority of the rise, as in the late 1960s, is explicable only by increasing rates of contraceptive use among people of unchanged residential, educational and occupational situation. We are examining the spread of new ideas and their application, perhaps catalysed by rising levels of urbanization and education but by no means restricted to being proportional to them.

Such projections are shaky enough; employing them to project fertility change is much more so. However, the effort is instructive. If we adopt a set of plausible assumptions about parallel changes in the incidence of abortion and traditional contraception, we might anticipate relative falls during the decade in fertility (i.e. relative to what it would otherwise have been resulting from health, nuptial and other pressures determining fertility trends) of 1.8, 3.6 or 5.4 per cent respectively for the three projections or about 1, 2 or 3 birth rate points resulting in a reduction of about 180, 360 or 540 thousand births per annum. These falls would not be spectacular but they would be the necessary precursors of greater changes to follow. Nevertheless, they would mean very substantial changes precisely in those areas where governmental effort and expenditure would be likely to be greatest. Thus birth rates in francophone coastal cities could, with government family planning programmes, fall below 40 by the end of the decade and in anglophone coastal cities below 30, although the latter probably depends very much on the volume of rural-urban migration and on the fertility patterns adopted by these migrants.

What is clear is that existing data collection systems are unlikely to be good enough by the end of the decade, or to have, for comparison, good enough figures from the early 1970s, to detect, or at least prove beyond doubt, national changes in fertility (with the possible exceptions of Ghana and Kenya). But they may well be able to show fertility declines in some urban populations, and hence there is a need in demographic data collecting systems for sufficiently large samples in centres like Lagos, Accra, Nairobi, Dakar and Kinshasa.

Urgent research needs

Tropical Africa is a region where planners need research findings for guidance. It is also intellectually of great interest as representing demographically the last major pre-transitional or early transitional populations in that there are still areas where mortality decline has hardly begun and in that fertility ideals as well as practice are still very high except amongst very small subpopulations.

In the fertility area there appears to the writer to be nine areas of importance which should be given priority.

(1) We need better fertility measures – ones good enough to detect fertility change over the coming decade. This is largely a problem of better data collection systems and a far greater emphasis on perfecting communication between interviewers and respondents. It seems

unlikely that vital registration schemes, except perhaps on the basis of experimental areas, can be set up and made good enough in that period. The best possibilities, in order, for the region (although the order will vary for individual countries) seem to be the carrying out of the 1970 and 1980 census rounds in tropical Africa (and comparison with the limited amount of fertility data from the 1960 round and with that from other surveys), the World Fertility Survey rounds in the 1970s and 1980s, and PGEs or parallel data collection systems. At the time of writing it appears as if fertility questions suited to Brass analysis will be included in some countries in the censuses on either a 100 per cent basis or in a subsample and in other cases in a post-enumeration survey usually linked in some way to the census. As yet the separate survey programme has been planned insufficiently to be sure of its value, but this may merely mean that the African Census Programme is concentrating its full attention on each step in chronological order.

(2) It is important that we should have KAP-type information for francophone Africa at least equal in quality to that which we now possess for anglophone Africa. Information on ex-French Africa is at present limited to surveys in a small number of locations in a few countries: Senegal in 1967-8, from which work no report has yet appeared; Upper Volta in 1969 and Niger in 1970; a specialized study in Chad in 1969-70; studies, with an orientation towards the examination of traditional African cultural patterns with regard to reproductive behaviour, in Togo in 1969 and Ivory Coast in 1970, from which a report has yet appeared only on the former. In addition a more complete KAP survey has been undertaken and published for Rwanda. However, there is no equivalent of the national surveys, or the series of surveys which give some picture of most sections of society, that have yielded a reasonably comprehensive picture of Ghana, Kenya, Nigeria, Sierra Leone and, in the near future, Tanzania. The solution would seem to be a marked effort to ensure that the World Fertility Survey is carried out in a range of francophone African countries (significantly the African programme may begin in Zaire), and for funding bodies in the field to encourage smaller experimental surveys based in some cases on the new universities which are appearing in almost every country (see below for this aspect of the Changing African Family Project).

(3) More emphasis (in analysis as well as data collection) must be given to establishing links between anti-natal practice and fertility levels. A methodology will have to be developed which overcomes the problem that women of above average fertility are most likely to turn to contraception and/or abortion. In Africa measures will also be needed of infecundity or subfecundity and whether sexual activity (either in or outside marriage) is taking place (and possibly also an estimate of sexual frequency). It seems doubtful that the World Fertility Survey will retain all these questions in most countries, but a decision might be made in some countries that such an analysis is important and deserves the full range of necessary questions plus the effort in the field to see that they are asked properly.

(4) A major study is needed in at least one country of change over time in KAP indicators and parallel change in a range of other socio-economic indices. An effort would have to be made to show associations at individual, community, provincial and national levels. Much of

the work would consist in collecting existing material and perhaps arranging the further processing of data already in existence. Nigeria or Ghana would seem to be the country best suited for this – NISER would be an excellent base with its interest in both social and economic studies and its relationship to both universities and government.

(5) We need to know a great deal more about how fertility decline or contraceptive or abortion practice starts. This is possible in Africa because fertility transition is at such an early stage, but it is also necessary for exactly the same reason. In anglophone West Africa, conventional KAP surveys, after a decade of use, probably have no further major revelations for us; we know the approximate levels of KAP indicators and their associations with certain socio-economic characteristics. We are in the same danger as are so many social scientists of being so dazzled by the powerful tool statisticians have forged for us in the measurement of the significance of the association between two factors that we often overlook the importance of distributions. For instance, it is true that contraception is more likely to be practised by an urban elite than the bulk of the urban population and by urban population as a whole than rural population – yet the fact remains that some of the urban poor and some of the rural population do practice it (and, because of their larger total number, the absolute numbers of contraceptors may be greater among them than among their counterparts). We know surprisingly little in Africa about innovation in the area of fertility control, yet such knowledge is urgently needed because the evidence is that antinatal practice is spreading faster than socio-economic change can explain (but not faster than informational change can explain) and its spread is presumably hastened by the coming into existence of new groups or individuals practising fertility control. We need to know why some women begin such practice while other residents of the same area with a similar educational and socio-economic status do not do so. We need to know the extent to which they serve as foci of change. New questions and new methods of approach are needed. Almost certainly, many studies will have to draw more upon the techniques of social anthropology than has been usual in demographic work. These considerations were the genesis of the Changing African Family project (see below).

(6) The fundamental question in population change in tropical Africa is probably that of the economics of family size at the level of the individual farming family. It may well be that national economies may benefit by a reduction in the average size of the nation's families, and it may also be that per capita incomes will rise faster among peasants if in their totality their fertility is reduced. But this is not incompatible with a situation where the individual farmer who restricts his family size presents his family with per capita incomes which are no larger (and perhaps smaller) than those of the neighbouring large families. Furthermore, the farmer himself may be economically worse off still in terms of the amount of work he has to do personally to reach a certain income level, in the division of labour possible within the family, in the total capacity of the family for handling large tasks without outside assistance, and in guarantees of support in sickness and old age. He may also forego various social advantages in that a reduced number of adult children will in due course almost inevitably diminish his influence in the community. These are the considerations, rather than ignorance

or blind adherence to tradition, that will make him impervious to family planning information or an unlikely target for clinic contraceptive supplies. But the calculus may change; it already has in most urban areas but it may do so in rural areas too as the cost of children rises with the arrival of schools and the growth of non-agricultural economic opportunities. But it is essential that we devise research to measure at the level of the village family the real costs of children and their capacity for both production and consumption. Three attempts to encourage this within the continent have failed¹⁹, but perhaps a cooperative effort is needed between researchers within and outside the continent to set up a sufficiently sophisticated research project. Two efforts of an associated kind have been attempted: a limited attempt to measure the value of children in Ghana in 1963 and a much more ambitious project in Nigeria during 1973²⁰. The latter is yielding a range of valuable evidence on the matter but this information only highlights the need for detailed econometric-type measurements within the villages.

(7) For some areas where antinatal practice has begun to spread, we need measures over time of the nature of that spread: the incidence and changing incidence of practice, changes in type of practice, the spread to various sectors of society with the analysis tied closely to a chronological framework, and so on. This type of work is of most value amongst populations where the level of antinatal practice is already fairly high and hence the locale for such studies is restricted to a few cities. Such research, employing retrospective information, formed one segment of the 1973 Changing African Family project in Nigeria with the study being confined to the city of Ibadan (see below).

(8) A major problem of much of the KAP work, regarded as social scientific measurement, is that it does not yield a measure of completed behaviour. This is obviously true in the attempt to relate attitudinal material to behavioural patterns, but it is also true in relating antinatal practice to fertility and in relating either current antinatal practice or fertility to completed fertility. The only real measure of successful population control is reduced fertility until the end of the female reproductive span. Any earlier measures can only predict this – and perhaps wrongly, for reduced fertility early in the span may only be an indication of a changing age structure of reproduction. Apart from census or demographic survey information on older women (and these data have been limited and their analysis has been very scanty indeed), we have had no information on those women or families who had intentionally and successfully limited family size – who they were, why they did it and what they did. We need to study women who began limiting their family size a generation or more ago. This was the case among only a very small fraction of the population even in tropical Africa's most advanced towns, and hence the study is as yet possible in only a few locations and even there with huge samples. Such a study was made in Ibadan in 1973 as part of the Changing African Family project (and, as it is the first of its kind, preliminary results are reported in some detail in the

¹⁹ The writer described such research and attempted to make it one of the conditions for participation at the population conferences in Ibadan in 1966 and Nairobi in 1969 (but failed as can be seen by the chapters in *The Population of Tropical Africa*, *op. cit.* and *Population Growth and Economic Development in Africa*) and in a major study of West and Middle Africa (still little success, as can be seen in *Population Growth and Socio-economic Change in West Africa*, *op. cit.*)

²⁰ The 1963 study was reported in John C. Caldwell, "Fertility Attitudes in Three Economically Contrasting Rural Regions of Ghana", *Economic Development and Cultural Change*, XV, 1967, pp. 217–238. The 1973 study was part of the Population Council's Value of Children programme, and, although, in terms of funding and original concept, not a part of the Changing African Family project, it is treated under that heading below because of the interrelation of the field survey programmes.

last part of this paper). Perhaps one or two more studies of this kind are needed in other parts of tropical Africa. Certainly they do present us with an opportunity to measure the first faint movements leading to fertility decline, an opportunity already lost for ever in many societies and perhaps soon in all societies.

(9) The financing of children's education and the inter-relations between economic pressures arising from this and attempts to limit family size may be the most important factor in tropical Africa tending to reduce fertility. The matter is complex. Parents often finance education separately and not jointly; siblings may be a more important source of finance in many countries; other relatives may provide money (and accommodation if they live nearer schools) and the amount given may depend on the demonstrated inability of the parents to meet the cost (a demonstration which may depend for its success on the number of other children they have); the real break-through in educational financing may come when one of the children secures a job with a high income resulting largely from his extended education. The full ramifications and their implications for fertility and its control have never been explored for any African society. The sooner one or more competent studies are done the closer will we be to understand the mechanics of fertility change in the region.

The Changing African Family Project

In order to provide that information in the above list which seems unlikely to be provided by any other planned research efforts, the Changing African Family project²¹ was organized (i.e. originally the work listed under headings (5) and (7) with the later addition of (8) and the use of the Ibadan survey team to undertake the value of children work related to (6)). The emphasis is on "change in fertility control" or on "changes which may precede changes in fertility control". The emphasis has also been on new types of investigation and innovations in methodology suited to African conditions. It is hoped that work will be done in research centres with little previous interest in population phenomena (as well as in centres which have evinced such interest) and so provide in the future a better geographical coverage of population research in the continent. The distinctive feature of the research is that it was planned as a collaborative effort by researchers in 20-25 countries.

The fundamental concern in the project is that the information collected should describe the real situation in Africa – that the data should be the best that can be collected, that outside concepts should not be artificially injected into African responses because the research instruments have been too little changed, and that the technical apparatus of the research

²¹ The project is funded by the Population Council which also provides technical advice. It is based upon the Sociology Department of the University of Ibadan, Nigeria. However, the research projects are separate endeavours by teams throughout Africa, with the central organization providing some ideas and exchanging experiences, as well as guaranteeing technical standards and ultimately carrying out comparative analyses. Broader projects have been undertaken in Nigeria so that the experience can be shared with the other programmes. A paper, "The Changing African Family: A description of a project and an invitation to participate", can be obtained from the Sociology Department of the University of Ibadan, the Demography Department of the Australian National University or the Demographic Division of the Population Council. The Directors of the project are Professors Francis Olu. Okediji and John C. Caldwell; the Field Director is Dr Helen Ware; other members of the central staff directing the field work in Nigeria were Pat Caldwell, Bisi Adetona, Susan Soyinka (in charge of editing), Kayode Soyinka (advising on Yoruba culture and language and directing interviewing teams), Mr Israel Orubuloye, Mrs C. Akande and Miss O. Adeyeye; others on the Steering Committee of the project have been Professor Joseph Scott, Dr S. O. Imoagane and Dr O. Arowolo; the Population Council Adviser is Dr Tom Burch.

should be of the highest standard that can be achieved. To assist in the movement towards these ends, a major segment of the research was carried out in Nigeria in 1973 both to build up experience while proving what could be done and to secure much needed information. The central effort of the Nigerian programme was to ensure that interviews were meaningful to the respondents and that a dialogue was maintained between interviewer and interviewee that was understood within the latter's culture and was as similar as possible to a discussion which she or he could have had with the neighbours. Thus, in terms of most survey experience in Africa and elsewhere, an unusually high proportion of both the effort and the cost was expended before the main interviewing began. Because all work was to be confined during the 1973 programme to Western and Lagos States, originally almost entirely Yoruba areas and still very largely Yoruba in population, the decision was made early to restrict work to this culture and to see just how far one could go in improving data collection within one culture group. After that decision, recruitment began of an almost wholly Yoruba staff. An early research group (who later tended to become supervisors and office staff) was built up of persons interested in studying their own society and in conducting experiments with the culture. The group met almost every night to report discussions that they had participated in during the day in randomly selected households; they reported on aspects of the family, fertility and fertility control as they had found it and on the vocabulary used in this area. Trial questions and finally questionnaires were built up, the latter being at first in English. This was important because it allowed those research directors who spoke only English to debate with the Yorubas what they meant by the questions and what they were trying to find out. Again and again, questions were modified in English because it could be shown that Yoruba self-knowledge or the continuing field testing proved that the thrust of a question was not entirely appropriate or could be misleading. Then, as is the usual case when survey questionnaires are translated into African languages at all (probably the large majority are printed only in English or French with interviewers translating during the interview either on the spot or from notes written down during training sessions), an expert translator and university authority on the language – his own language – was asked to translate the first questionnaire after being carefully briefed in the objectives and desired approach of the project. The Yoruba questionnaire was then reproduced and a trial survey of considerable size undertaken and analysed. The analysis showed the same rise in non-response and in answers revealing less than a full understanding of the question with lesser education or literacy or residence in poorer areas that we have come to expect and tolerate in African surveys (but not in surveys in economically developed countries where we seem to assume that the phenomenon is absent or of little note because of higher all-round educational levels). Our original team was joined by a group of actors, who had specialized in getting the meaning of Yoruba drama over to mass audiences, and who now took the questionnaire to the field with interest and enthusiasm. The verdict (and one sustained by an independent language content analysis) was that the prime cause of our difficulties was that the translation was a good, literary one employing a large vocabulary and complex grammar of the type originally invent-

ed for the rendering into Yoruba of the King James' Bible. The last point is important. We were using an instrument designed to expand Yoruba so as to convey concepts originally brought to Nigeria in the English language; in contrast, we now wanted a Yoruba discussion of fertility, fertility control and related matters in the language and thought forms of the common people with the nearest English translation that we could obtain. (It should be noted that I have not referred to the original or indigenous language, as Yoruba itself has changed over the last hundred years – a family of languages has moved towards a single form, Oyo Yoruba, as a result of the Bible being translated into this dialect. In fringe areas, and especially amongst some of the older people, the full transition has not taken place – this was watched for carefully during interviewing and appropriate interviewers were used where necessary.) We slowly rebuilt the questionnaires continually reducing the size of the vocabulary to a point where we had to stop because the gains from any extra comprehension by illiterates would be outweighed by a loss of sophisticated nuances which could obtain extra information from the more educated. The vocabulary, especially in areas of sex, reproduction and antinatal practice, progressively dropped imported terms as it was shown that various Yoruba phrases did convey exact meaning to the widest number of people. (The problem that prevented the immediate discovery of this and led to continuing debate about the use of some phrases was that certain terms were regarded as indelicate or even bordering on the obscene – but occasionally, with apologies, they were used if the meaning thereby penetrated to a larger proportion of the population and was as clear as or clearer than alternative forms.) Special precautions, not always employed in African surveys, were taken to ensure that oral contraceptives were distinguished from other substances taken orally and said to have contraceptive or abortifacient properties. More importantly, the order, form and vocabulary of questions were reformulated to agree with Yoruba idiom and thought ways. The Yoruba people, in common with most other West African ethnic groups, express many of their thoughts as proverbs or as sentences which refer to proverbs or contain all or part of a recognized proverbial saying. Many of the questions ultimately used were accordingly of this type. Elsewhere in the questionnaire the degree of acceptance or rejection of proverbs was tested (usually without adopting the common survey technique of reversing some, because it was discovered that the negative of a Yoruba proverb, or indeed often an English proverb, is very far from being its complement – and, in any case, there is no real problem of influencing the respondents towards the acceptance of the conventional position, because the measure most commonly needed is the degree of movement from a traditional position coupled with an awareness of the movement). Again and again the real problem was rendering into English the exact meaning of the Yoruba, although this was probably a lesser problem than translation in the opposite direction because of the relatively large vocabulary of English and the fact that the English rendition would mostly be used by social scientists either saturated with the experience of the survey itself or others at a further remove who could be provided with additional notes on meaning. In any case, they were not the respondents. Wherever possible, both questionnaire and the interviewing procedure adapted themselves to Yoruba etiquette

and avoided situations traditionally regarded as gauche. For instance, the Yoruba regard a direct statement of one's present age as inviting ill-luck, but are much more willing to state two ages which will combine to give age at interview (and are in most cases ready accomplices when side-stepping ill-fortune in this way, cheerfully appreciating that the two answers will be added and often seeming impatient for the addition to be written down by the interviewer). In this work, we appeared to get the best age data by adding responses on age at first marriage and period since first marriage (but an alternative and useful check for respondents with surviving children is age at the birth of oldest surviving child and date of that birth – it is not as bad to ask about someone else's age but there is nevertheless some difficulty and hence, if one pair of questions only is asked, the focus should be on the first marriage). If respondents include some who have never married, alternative questions involving first education, first employment, first residence in the city or the present dwelling, age at the birth of the youngest sibling or at some notable community event must be employed. In the Nigerian surveys we also found it useful to ask for present age directly, late in the interview where it could do no harm, as a check on the addition in the case of some respondents and an indicator that further discussion on the matter would be warranted – even where the respondent gave an obviously wrong answer we found this illuminating. Similarly, the now conventional questions on number of living children at home and number elsewhere coupled with a third question on the number dead must be asked in this form in Yorubaland because of the ill-luck and danger to the children of stating the total number of births straight out. Nevertheless, we subsequently also asked for the total number, partly as a check which could lead to further enquiries as to the discrepancy between the two figures, and partly because, in a transitional society where many beliefs are being discarded, some respondents might provide the most accurate answer to a question in this form. Approaches of this kind demonstrated that quantitative data can be obtained of far greater accuracy than that which has been collected in the censuses – indeed of considerably greater accuracy than the writer had previously concluded was possible in the culture because of ignorance of age and cultural disregard for it.²²

All interviewers were capable of interviewing in both Yoruba and English and each questionnaire was printed in both languages. We had been led to believe that the more educated would almost invariably choose English because they could express more difficult concepts in that language. Unless the question arose, our interviewers did not explicitly offer this choice, as sheer snobbishness might easily lead the better educated to opt for the English-language questionnaire. Instead, the interviewers greeted them in Yoruba and asked about Yorubas in the household and then spoke to identified eligible respondents in alternating Yoruba and English until the conversation settled down into the language in which the respondent most obviously felt at ease. If, during the interview, greater clarity appeared to be attainable by repeating a question in the language not being employed in the interview, this was done; if, at this stage, it was determined that an error had been made and that better responses would be obtained in the other language, then a switchover was made. Even amongst the educated middle class, a surprising amount of the interviewing was carried out in Yoruba or both

²² J. C. Caldwell and A. A. Igun, "An Experiment with Census-type Age Enumeration in Nigeria" *Population Studies*, XXV, 2, July 1971, pp. 287-302.

Yoruba and English, and interviewers had few doubts that for persons interviewed in this way we were getting closer to the truth on these personal, intimate and emotive matters. In various other ways, we tightened up the survey procedures and attempted to mount an enquiry into Yoruba culture with Yoruba advice on cultural matters being the chief determinant at all times.

A critical decision was that on open-ended questions. Surveys of this size are much more easily and cheaply handled if nearly all questions are precoded. Furthermore, it can be argued that office coding of open-ended questions is the same process as the interviewer undertakes in the field without the empathy with the respondent that exists during the interview, and that the list of codes developed in the office should be no better than adequate pilot surveys can provide for the final version of the questionnaire. We were determined at the outset to improve on the writer's previous research by making this advance to entirely precoded questionnaires. But all the preliminary testing produced evidence against it. The area covered by the Value of Children survey was so great, and included peoples with such different cultural reactions, that the pilot surveys would have had to have been very large with an adequate sample covering the whole region. Interviewers, allowed to probe into difficult matters, frequently cannot resist the temptation to drop hints from the listed precodes; worse still, if respondents are initially tested on such matters as the advantages and disadvantages of large families by being asked to agree or disagree with a series of propositions (by no means a rare approach in the study of the value of children) they can be led by suggestion almost out of their culture. Finally, a written record of just what they said can clearly reveal nuances as well as allowing at a date later than the original analysis a rethinking of the whole matter and even recoding. We used open-ended questions at all difficult points: the first thoughts on family limitation and the first antinatal practice, the pressures towards large and smaller families, the views of relatives, personal evidence on fecundity and so on. The most rewarding use of the open-ended question came near the end of the Achieved Small Family Survey, where we put to the respondent the whole problem we had been trying to explore and explained the awkwardness of formal questions; then we asked them to solve our difficulties and to put us right by summarizing their own situation on their own words and by explaining why and where the interview had missed the point (this was a development of a technique used in a survey of Melbourne, where the respondents were finally asked to evaluate how closely the interview had got to the truth). Open-ended coding in the office was entrusted only to persons with extensive interviewing experience in the survey. Even so, only a select few were found to be good enough (this could be taken as casting doubt on the value of all precoded surveys, but it must be remembered that these were the most difficult questions). This approach also allowed continuous debate, frequently resulting in extensive recoding, in an effort to obtain meaningful and quantifiable categories of responses forming in reality a continuous spectrum. These questions certainly secured the best and most comprehensive replies in the surveys, partly because the fact that we obviously regarded them as special meant they were discussed at length. The long answers (partly achieved by the large blank spaces we had left

on the questionnaires) proved invaluable in guiding our thinking about the whole survey programme.

Perhaps the other most important stress was on the sampling. Informal discussions with field staff and ex-field staff across Africa have convinced me that much survey sampling is so weak – often much weaker than is implied in published reports – that the findings must be questioned. Some of the defects are in the design, which, for instance, may lead to interviews only in the houses facing the street in a city like Ibadan where such households are not like those found deep inside the vast blocks of the older part of the city. More of the trouble lies in substitution, which, even where it has been intended, is often unsupervised and does not follow the formulae laid down. The result is usually an upward bias in educational and other socio-economic measures of the respondents and concomitant reduction in infant mortality and exaggeration of antinatal practice. The following experience is drawn from one of our surveys and concerns sampling Ibadan city, which had populations, according to various reports, of between 600,000 and 1,600,000. Needless to say no data were good enough for stratifying for sampling purposes and houses were not laid out in patterns or even separated in such a way as to make division into blocks easy. But there were two advantages: an excellent series of very large-scale maps (1:1,250) had been compiled from aerial surveys seven years earlier, and the city was so compact that sampling did not have to take transport time or logistic costs into account. In each of the 250 maps covering the city, one-twenty-fourth of the total area was randomly selected (a single block in homogeneous outer areas and four quarter blocks, one in each quarter of the map, in the thickly settled inner areas where population density often changes very suddenly). The map was used only to define the area on the ground; thereafter, new maps were compiled marking in every dwelling and other existing features and ignoring destroyed dwellings which had existed at the time of preparation of the published maps. However, experience showed quite dramatically how inadequate plans for such measures are without exhausting and continual supervision at the highest level – an important point because it is the lowland tropical African climate which makes so many survey directors skimp personal field supervision (reinforced by a social climate which gives great prestige to academics or higher public servants and hardly expects to find them sweating in the field)²³. Yet, unless survey directors take personal responsibility for checking out every sample area, major distortions can occur in the survey.²⁴ The problem is that maps have not been readily available in Africa and children have little opportunity for practising large-scale map reading and identifying buildings or other features around their homes and learning to use map scales. Certainly, in spite of intensive training and field practice many of our interviewers or supervisors failed to identify completely their sample areas: the wrong areas were chosen, more frequently boundaries were confused (far more often resulting in larger areas than smaller ones), and there was a reluctance to keep rigidly to the instruction to exclude dwellings which had more than half their floor areas outside the sample area. It should be emphasised that Ibadan is one of the world's least planned cities and delineating sample areas means deducing borders from the position of drains, public taps and latrines and even

²³ Too much should not be made of this point in contrast to the situation in temperate and economically developed lands, where, in point of fact, researchers are often ignorant of shoddy field supervision because the work is delegated to firms who have no real commitment to the ends of the surveys.

²⁴ One can employ supervisors with cartographic, surveying or geographical training but there remains a need to test and guarantee their skills.

sightings on more distant objects, any of which may have changed since the maps were drawn.²⁵ The two major reasons for insisting so strictly on exact sample areas are the fear of differential enumeration of the population (sample areas are usually easily defined in higher socio-economic areas) and the need to secure total figures for the population and its characteristics by blowing up the sample. The latter point is vital because it provides the only real chance of validating the sample against data collected by other systems. A major weakness in African surveys is that testing of this kind is rarely reported and presumably rarely undertaken (comparable African data do tend to be scarce and inadequate but are more common than surveys reports would suggest). In the survey being reported here, the respondents were to be Yoruba females, 15–59, but nevertheless a full census was taken in all sample areas (resulting in an estimated total population for Ibadan of approximately 900 thousand, a figure which can be compared with that of the 1973 census – taken some months after our field work was completed – when this is released).

If better (or, even, analysable) tropical African fertility and fertility control data is to be collected, then all other aspects of surveying will also have to be tightened.

Training is vitally important but is often wasted by doing it largely as a block before any interviewing, except perhaps a few staged mock interviews, has been done. We have increasingly found that this does not work and leads to large numbers of inadequate interviews in the main survey. The future interviewing team (subject to a weeding out of inferior interviewers at every stage) should take shape during the preliminary testing and translation of the questionnaire and should be used for the pilot survey in such a way that surveying and training sessions alternate. It is doubtful whether this reduces the quality of the pilot, while it demonstrably very much raises the quality of training partly by bringing the field experience into the training school and allowing problems to be thrashed out there. The weakest area in training is often that connected with sexual matters or with antinatal practice differing from that found in the West. The survey directors must decide whether a decision to interview only women currently living with men is not in fact ensuring that an important antinatal practice (separation with no intention of breaking up the marriage) is not being ruled out; they must define when post-natal abstinence becomes contraception and how long lactation must continue before it should be classified as having a potentially contraceptive effect; they must make up their minds and clearly set out whether post-natal abstinence or lactation is held to be an antinatal practice if practised (or said to be practised) for reasons unrelated to the avoidance of conception; they must ensure that abstinence or infrequent sexual intercourse at certain periods in conjugal life is detected by adequate questions on sexual frequency; and they must distinguish whether they are measuring antinatal practice within a specific union in contrast to all the activities of a respondent (in West Africa men frequently have more sex outside their marriages precisely so that they can practise abstinence with antinatal intent within it). Training also suffers, often to an astonishing extent, from the embarrassment of those doing the training with two quite distinct results: the trainees are left ignorant on certain points, and they tend to transmit the embarrassment to their interviews with a consequent reduction

²⁵ This does not mean that the Ibadan difficulties were unique in Africa. They are probably the rule. The writer found it impossible to follow the instructions in another area of Nigeria which were meant to be understood by interviewers and their supervisors when determining the boundaries of 1963 census enumeration areas.

of the value of the data collected. These failings are particularly marked when dealing with withdrawal, abstinence and, to some degree, rhythm, folk methods and abortion. This leads to an undercount of antinatal practice (the seriousness of which is shown by the fact that the contraceptive methods listed above have been listed as the chief methods by which their fertility has been restricted by five-sixths of the Ibadan women who have achieved small families by contraceptive means) and to a differential in the completeness of reporting between these methods and the pill (which is easy to discuss, although the very ease may hide a confusion with abortifacient and other non-hormonal pills retailed in tropical Africa). If the surveys are to be worth conducting, these matters have to be discussed again and again with interviewers, almost to the point of lewdness, and it is useless to adopt the commonly employed stratagem of assuming that local supervisors will get the point across in their own language or that a lecture or two by a doctor or nurse will suffice. Embarrassment can be even greater when discussing the reasons for an undercount of infant mortality and corrective measures to be adopted, for a major reason for infant mortality is believed to be witchcraft by near relatives (frequently co-wives) who are often within earshot of the interview. Many European researchers find it very difficult to discuss witchcraft with African interviewers. Much turns on the quality of the supervision, especially the supervision of supervisors. In the Nigerian surveys we ran checks of every sample area to make sure that all possible residential structures had been listed and visited, and of all these residences to ensure that every eligible respondent had been approached, (and that ineligible persons had not been included) and of a large proportion of the respondents to establish both that they had been fully interviewed and that the interviews were of adequate calibre. The high proportion of reinterviews (which met very little resistance) was used also as a device whereby the very best interviewers could be employed checking – and adding to – the data on child mortality, contraceptive method and fertility (listed in order of difficulty in getting full data and hence of the extra effort employed). The basic point is that supervision has to be continuous: supervisors have to be checked and retrained continuously; there should be no strict differentiation between supervisors and other interviewers and supervisors should know they can return to normal interviewing while good interviewers can replace them at any time; supervisors should be rotated so that the establishment of friendship between them and certain interviewers does not lead to a relaxation of standards; supervisors should be the kind of persons who go out of their way to be helpful but who can also detect and report all faults (this means an endless search for persons of atypical personality in societies where cooperation is stressed more than hierarchical responsibilities); and finally dishonest or inadequate staff must be dismissed (the earlier the better). When staff are dismissed, or even when their efficiency is only suspect, all their interviews should be repeated – in far too many African surveys interviewers are dismissed because of inadequacy, but the interviews completed to that time (the evidence of inadequacy) are left in the file of completed questionnaires apparently in the expectation that their inadequacies will be diluted by the other schedules. Refusal to be interviewed at first approach was relatively rare (especially as we always had secured

the cooperation of the traditional authorities urging or even instructing cooperation). To cope with that which did occur, we used a range of persons to make subsequent contact. They included people of very different personalities and different positions in Yoruba society. Ultimately refusal to be interviewed was reduced to zero.

The distinction often made between field and office editing (and coding of open-ended questions) is a false one – both are interrelated and the first leads into the second. The editing team must also be plastic and it must be possible to drop editors (and re-edit all the schedules they have done) and rotate in supervisors or interviewers who show an aptitude for the task. A point that cannot be overstressed is that all editors must have substantial interviewing experience in the survey they are editing; otherwise, they will follow the text-books or training and become martinets introducing a false logic in data which is often inconsistent although accurately reported and where the inconsistencies are themselves often clues to the truth. All questionnaires must be completely and independently edited in the office at least twice; there is no other way of really supervising editors or of bringing basic problems in the data or the survey operation to light. The most important point about editing is that it must be fully integrated with the field work – indeed an aspect of it. Editing must start with the first trial interviews and inevitably plays an important role in the success of the pilot survey. It is the editing which guides the whole survey and editors must subsequently discuss all edited schedules with the interviewers concerned and with their supervisors. From these discussions, as well as direct reports from the field, the changes in instructions, perhaps the addition of codes and so on flow. These daily discussions between individuals yield much of the material for the steering committee meetings; in the Nigerian surveys meetings of this type, with no real limit on the number of interviewers who could attend, were found to be indispensable being held frequently, in the early stages of the survey taking place in one form or another every evening or night. The editorial discussions must flow back into the next day's field work or the whole tenor of the work is wrong. The editing (but not all the editorial supervision) in the Nigerian surveys was done by Yorubas partly because of their knowledge of the language but also because of their sensitivity to the culture, because much of the discussion, which always fundamentally centred on whether we were getting at the truth, was on aspects of the society (it was usually helpful for non-Yorubas to enter the discussion subsequently before any significant decisions were made).

Finally, it might be noted that behavioural differentials between respondents often arise not from different penchants for doing things but from different environmental circumstances. The use of family planning or medical care depends very largely on what is available. Hence, the Nigerian surveys found it essential for subsequent meaningful analysis to collect community information on a comparable basis and community schedules were designed for this.²⁶

The Nigerian Surveys

The three Nigerian surveys of the Changing African Family project were designed so that each tackled an essentially new area in tropical African research while at the same time

²⁶ The most comprehensive community schedule was used in the Value of Children enquiry and is available from the Changing African Family project. However, this has been largely incorporated in the model Community Level Questionnaire in Ronald Freedman, "Community Level Data in Fertility Surveys", World Fertility Survey. Occasional Paper No. 7 (May 1974), and for general reference this document is more useful.

attempted to find solutions for certain methodological problems.

The Beginning of Family Limitation survey had two interrelated central themes. One was the attempt to obtain age and marriage duration cohort data on changes in antinatal practice over time and the other was an investigation into the circumstances which led individuals (or couples) into beginning such practice.

The survey was of a one- in twenty -four sample of Yoruba females, 15–59, living in Ibadan in May-June, 1973. A short schedule was employed for the 6,624 females in the sample, while in addition a supplementary longer questionnaire was used for the 1,041 respondents (15.7 per cent) who had employed (or whose partner had employed with her) any antinatal practice other than abstinence (the first part of the questionnaire investigated abstinence and living apart at some length). Information on the method was commonly used, together with the extent of use and explanations for non-use or partial use, were collected from 1930 with single-year periods being employed from 1960 onward.²⁷ Similar information was obtained for stages in the life cycle defined as the intervening periods between such events as marriages and births of specified order. A battery of questions investigated both first antinatal practice and the beginning of consistent antinatal practice searching for the source of ideas, the reasons for use, the exact circumstances which led to such innovational behaviour, the difficulties in changing from traditional behaviour patterns, tension or agreement on the matter in the conjugal union, reactions and pressures emanating from relatives and friends, difficulties in continuing practice and the influence of the practice on the behaviour of others.

The survey was worth doing only if reasonably good retrospective quantitative data could be obtained. Hence the emphasis on working within a single culture and understanding that culture to the point where its own customs, forms of politeness and quantitative concepts could assist in obtaining the best data and the best dating of events. Preliminary analysis suggests that there is sufficient internal consistency in the quantitative data to make it usable and that the cohort analysis is justified.

The survey should provide the first detailed information for tropical Africa on the mechanisms of the spread of contraception and abortion over the last few years (and more generally over the last few decades). It should also allow us to do what we will now never be able to do for the European fertility transition, namely to ask a representative cross-section of the first women to innovate by attempting fertility control how they came to make this break with tradition and what stresses it entailed. The evidence so far indicates that the innovators are not as aware as one might anticipate of their role; peer groups change at much the same time and arguments are based on economic rationality employing views currently being bandied about with hardly any awareness that such views conflict with the conventional wisdom of a decade earlier or of contemporaneous groups in other socio-economic strata of Ibadan or elsewhere in the country. The time traveller to late eighteenth century France might be severe-

²⁷ The approach adopted was an attempt to collect data for an African city of the type (but modified to meet African conditions) collected in Melbourne, Australia, in 1971 and reported in Caldwell and Ware, *op. cit.* Published work on retrospective enquiries on antinatal practice in Britain include E. Lewis-Faning, *Report on an Enquiry into Family Limitation and its Influence on Human Fertility during the last Fifty Years*, Papers of the Royal Commission on Population, vol. 1, London, 1949; papers by Griselda Rowntree and Rachel M. Pierce in *Population Studies* in July and November, 1961; papers by C. M. Langford in *Family Planning* and *Family Planning Trends* in 1969 and by D. V. Glass in the May, 1970, Supplement of *Population Studies*

ly disappointed in that his conversations might turn up neither awareness of family change nor explanations of it.

The Value of Children survey was concerned with evaluating the economic and social value of children in the Yoruba family with an attempt to measure the extent to which extra children are found to be assets and liabilities. The focus of interest was on the forces that sustain high fertility and on any weakening that has occurred as a result of socio-economic change.

The sample was one of 3,000 Yorubas (1,500 females and 1,500 males) in 50 survey centres of Nigeria's Western and Lagos States (Yorubaland) stratified by province and size of residential centre within them so that the number of interviews was proportional to the population in each size class of centres within each province as claimed by the 1963 census. The primary stage of sampling was at the level of residential centre, the secondary stage of blocks within them, the tertiary stage of dwelling units and the quaternary stage of that of individual Yorubas over 17 years of age within the households. Every stage involved listing, although at the primary stage this had already been done for us in the published census volumes. In an attempt to provide a new dimension for the studies of the value of children, ideal family size and the inducement to have the marginal child, which appear to the writer to have reached a stage of limiting returns because of a lack of comparison with other things of value and competing claims on expenditure, an attempt was made to investigate the whole theme more widely. Respondents were questioned about their hopes and the things that luck could bring, about aims and plans in life, about expenditure patterns and the way they would spend windfall gains, about expenditure choices where children were taken to be one of the alternatives, and about their concepts of success in life and the strength of their desires to achieve it for themselves and their children. Six other matters were investigated in detail: the structure of the family and the extent to which it has departed from the traditional form, the strength of the desire for a marginal child with special emphasis on the wish for the next baby in the immediate future, the position of the childless, the dependence on children of women who had lost their husbands, the economic investment in children and the economic and social returns with an attempt to move towards the compilation of a balance sheet, and the social supports for high fertility.

Apart from the complexity of the sampling, the major methodological innovation was the use of large numbers of Yoruba proverbs and sayings and more recent conventional formulations of the conventional wisdom to probe the extent of agreement with these attitudes and the extent of conflict (and of awareness of the conflict) with them. Fifty-five sayings were commented upon and in addition a number of choices were offered between various different descriptions of situations. The approach appeared to be one of great value. Its use should be seen in the existing context of unease amongst African researchers that survey methodology is being imported into their societies from very different societies and is securing inadequate results because acculturation to the new conditions has been insufficient to allow successful transplanting. Every African on the research committee showed more excitement about the

use of these sayings than any other aspect of the research and claimed that this was an instrument that really would penetrate the society. The other approach of interest was an expansion of tables previously used in a more elementary form²⁸ in an effort to make up a balance sheet of the immediate investments on and returns from children, following a set of questions (not employed in the previous attempt) framed not to lead the respondent but to get him or her thinking in explicit terms of such matters (especially quantification and relative costs) to a greater extent than the society usually demands. The assumption here is that such matters lead to general worries – often not articulated in any form – and to responses which are rarely fully analysed but which can nevertheless take the form of significant behavioural reactions now or in the future.

By the time field work and preliminary analysis had been completed, we were convinced of the importance of such investigations being taken much further in two areas. Intensive studies are needed, especially in rural communities, of labour and other inputs by children and of consumption and other expenditure on them (as well as anticipated delayed returns from them judged by the returns now coming in from the previous generation of children). Especially on the input side, imputed values will frequently be necessary (with some investigation of how the researcher's imputed values agree with those assumed by the villagers). The researchers will have to acquire a great deal of knowledge of the economics of village life. The other aspect is that of educational costs and return. Every tropical African survey shows that, where fertility has been restricted or where there is a desire for a family of more limited size, the motivation is mainly economic and that the most important aspect of this is the fear that savings spent over the training of a large number of children will be less effective or satisfactory than expenditure on a smaller number. The matter is complicated by pressures for expenditure on the education of children beyond the nuclear family and the possibility of obtaining help from outside that family. An important aspect is that, although an educated person feels a responsibility for returning some of his extra earnings to the relatives who invested in his education, a stronger pressure is usually felt to help pay for the education of his (usually) younger siblings. Closely related to this is the concept held widely in tropical Africa of exploiting the education of one child (whose success may have been partly attributed to outstanding brightness or lucky circumstances) and thus starting a chain reaction of extended education and higher potential earning power among the siblings and thus raising the income level and social status of the whole family. This system may be the single most important factor in influencing the extent and timing of fertility transition in tropical Africa, and its economic and other aspects need thorough investigation.

The Achieved Small Family survey has certain unique aspects which warrant a report on some of the preliminary findings as well as its approach, and so will be dealt with in a separate section.

The Achieved Small Family

Much of the debate in governmental and family planning circles in Africa on the likely impact (and wisest timing) of fertility decline centres on the problems and gains which would

²⁸ J. C. Caldwell, "Fertility Attitudes in Three Economically Contrasting Regions of Ghana", *Economic Development and Cultural Change* XV, 6, January 1967, pp. 217–238.

be experienced by that new creation, the African small family. Yet such families already exist in Africa and have never been studied. Similarly much work has gone into examining those younger women who may achieve small families by using antinatal practices and into their characteristics and motivations, without any examination of those who did succeed in controlling family size. It seems a denial of the bases of the behavioural sciences.

There are of course reasons. The first is that achieved small families, demonstrably so because the mothers are beyond reproductive age, are few and hence costly to find and study. The second is that their behaviour pattern may represent that of a bygone age and have little relevance for the present or the future. Such an assumption is a dangerous one: it means that full fertility behaviour can never be studied; it may misinterpret the rate of social change; it leaves us studying a heterogeneous group of family planners, many of whom (and by no means a representative cross-section or a subgroup of predictable characteristics) will never succeed in restricting themselves to a small family.

The problem of finding the needles in the haystack was solved in several ways. The survey was undertaken in Ibadan, a city which has experienced substantial social change during the last few decades, and in which (in common with most other large centres in anglophone Africa) there has been some access to contraceptives for a long time. A small family was defined as that constituted by fewer than six live births to the mother. Such a categorization might seem astonishing in other parts of the world, but restriction to a maximum of five live births is certainly justified in a city where the only major study of fertility estimated a total fertility ratio of almost 10 for its Province²⁹ (probably much too high but nevertheless completed fertility of 0-5 tends to be exceptional, except among infecund or subfecund women who have never felt the need for antinatal practices). This definition does not prevent separate analyses of the data for really small families with perhaps maximum sizes of two or three live births, but for detailed cross-tabulation a larger survey than the one described here would be needed as over half our respondents were of parity 5 and only 9 per cent of parities 0-2 and 22 per cent of parities 0-3. The universe was a maximized survey of all women over 40 years of age, but again the purist can be satisfied by the fact that separate analyses will be done for women over 50 (half of all respondents) and for the lower parities. Finally (and this was what we really wanted – not just a decision to enlarge the number of respondents), antinatal practice was defined as any action *intentionally* taken to avoid conception: contraception, abortion, abstinence and living apart to make abstinence more likely.

The aim then was to secure a large number of Yoruba women over 40 years of age who had restricted themselves intentionally to less than six live births by some antinatal practice. Trials, indirect evidence from the *Beginning of Family Limitation* project and projections of the age structure of Ibadan from the 1963 census showed that our aim of interviewing close to 500 respondents would mean selecting every eligible respondent in the city. The new methodology employed in this survey was the screening methods whereby almost a million people were examined for eligibility without ruinously expensive costs in both effort and money. The whole problem was one of arranging the desired characteristics of respondents

²⁹ The Princeton study .TFR is given as 9.7 in Page, *op. cit.* The earliest analysis was published as Etienne van de Walle, "Fertility in Nigeria", *Population Studies*, XIX, 1 July 1965. The estimate must have been raised by age misstatement (perhaps together with some faking of the returns) at the 1952-3 census.

in such an order as to maximise the rate of screening and rejection (the actual steps and their efficiency will be reported elsewhere); even so, the survey was unusual in that its major cost was the selection of respondents, rather than interviewing and data processing.

The total number of eligible women located was 438; if the study had been confined to women over 50 with fewer than four live births it would have been 48 (and 500 women could not have been yielded by a survey of the populations of all major cities of anglophone tropical Africa) while a further restriction to women who had depended mostly on chemical, hormonal or appliance contraceptives or abortion would have reduced the number of respondents further still – to six! In order to examine the whole decision-making process that led to the restriction of family size, it was decided that husbands too should be interviewed. But such interviews were pointless unless the husbands were the women's first (otherwise they may well not have been involved in much of the critical decision-making) and unless the marriage was monogamous (polygamously married men are interested in the total number of their children and may give up sexual relations with one wife with no thought of restricting their overall fertility by this act). The number of interviews with husbands was 71 or just under one interview for every six women, a measure of the incidence of unstable and polygynous marriage in Nigeria as well as of widowhood (arising from high mortality levels and large age gaps between wives and much older husbands); it is also possible that the separation rate is higher amongst women who insist on restricting family size, especially those who do so by abstinence from sexual relations.

It is, nevertheless, very instructive to examine those who have restricted family size and the methods used to do it.

The eligible Yoruba females in Ibadan represented 0.7 per cent (about one in 140) of their age group. There was some differential by age: 0.8 per cent of women 40–49 and 0.6 per cent of those over 50. Thus, there is evidence of a trend towards greater restriction of fertility, but one so gentle as to suggest that there have long (perhaps always) been some women in the society who had reasons for wanting no more children and the resolution and the awareness of a means to carry their decisions out.

The methods used to achieve the small family (set out in Table 1) are of very great interest and will certainly be disconcerting to some of the organizers of both family planning programmes and fertility and KAP surveys and to some of the planners estimating likely fertility trends during the period of modernization.

The real import of the table is that those small families that do exist in Ibadan have been mainly achieved by massive abstinence. The writer is by no means convinced that this is a rapidly passing phenomenon. Just as Christianity in the West did not strive for early marriage and thus allowed delayed marriage to be used as a method for limiting family size, lengthy post-natal taboos on sexual relations have given abstinence (on the part of women) such an accepted place in tropical African culture that its use for fertility control has overshadowed possible alternative antinatal practices. Thus withdrawal is widely known but little practised, although the use of rhythm has been on the increase. It might be argued that all we have

Table 1
*Main Antinatal Method Employed to Achieve Small Family
 and Methods Ever Employed, Female Respondents*

Method	Main Method		Method ever employed	
	No.	%	No.	%
Abstinence more than two years after a birth or post-natal abstinence exceeding 2 years	190	43	360	82
Post-natal abstinence for periods of less than 2 years	136	31	378	86
Rhythm (periodic abstinence based on any theory about the fertile period)	29	7	60	14
Oral contraceptive	15	3	32	7
Condoms	14	3	22	5
Withdrawal	10	2	22	5
IUD	10	2	20	5
Diaphragm	9	2	15	3
Charms	8	2	15	3
Abortion	5	1	11	3
Medicines (from herbalists or mixed by respondent)	4	1	5	1
Sterilization ^a	3	1	6	1
Jellies, creams	2	(0.5) ^b	4	1
Injection (may include non-hormonal injections by herbalists)	2	(0.5) ^b	3	1
Foam	1	(0.2) ^b	5	1
Douching	0	(0.0) ^b	4	1
	438	100		

Note:

^a Female only; no respondents had had partners who had been vasectomised

^b Percentages under 0.5 shown to the first decimal place

detected here is the traditional post-natal taboo, customarily exceeding two years in duration according to some anthropologists, together with a rationalising of the reason for its use by a small number of women. However, the more research one does in West Africa the less likely does this appear to be probable. All Yorubas know that traditional society expected some period of abstinence after a birth – not (at least according to people now alive) for religious reasons but to ensure that pregnancies were not so close together that they did not endanger

the health of the babies and for motives that lie somewhere between rules of hygiene and rules of decency. No-one agrees on the period (and this is not new, for respondents' grandmothers seem to have told them widely differing periods) and it is, to my mind, certain that no woman during the last half century would have abstained for two or more years unless there was a more powerful reason than the strict prolonged observation of the taboo: fear of a premature pregnancy or dislike of sex or sexual desertion by her husband. However, the existence of the taboo and the strong West African bonds of the family of descent, which tend to overshadow those of the conjugal family, means that women often return to their parents or live alone (with their children) while the husbands find other women (or they do this while living in the same house). But the women do this in full recognition that they need a "rest" (from child-birth) or that the births must be more widely spaced³⁰ – all West African societies have expressions condemning people who have babies too close together and most have terms for *Kwashiokor* meaning the disease of babies who are born too close to each other. Perhaps the most significant aspect of the central role of abstinence is the possibility that modernization will in some areas lead to a rise in fertility because closer conjugal ties or greater physical or emotional distance from families of descent make abstinence more difficult to practise; it is this rather than a great courage to break the taboo in the more enlightened atmosphere of the towns that may raise urban fertility.³¹

Why did these pioneers of the small (or smaller) family do what they did? Few believe they are rationalizing subfecundity: almost four-fifths believed they had remained fully fertile until the end of the normal reproductive span and fewer than one-twelfth thought that they would have had difficulty in conceiving at any time; only half those who were not certain that they had remained fully fertile would have desired any more children (and then not more than five – or they would have been defined out of the survey). Nor do many attribute the decision to accidents in their own lives; in an open-ended question where they were encouraged to explain everything, only eleven per cent referred to straying husbands, difficulties in polygynous marriages or with relatives, loss of interest in sex or remarriage or health problems which prohibited having more children; in another question less than five per cent described the bearing of children as particularly painful and few of these claimed that the restriction to limit births was related to this. Their reasons for favouring the small family were exactly the same as is picked up widely in the developing (and developed) world: almost two-thirds of those giving specific reasons cited economic problems in rearing the family (others quoted other economic matters, such as support in old age) while another sixth argued that a family of this size would be better reared. The pattern was a more specifically tropical African one when the economic answers were subdivided: almost three-fifths referred specifically to the costs of educating the children. Thus, even though the proportion of parents who have deliberately had small families is still small, their reasons for doing so are precisely those one might anticipate generating a more widespread movement towards fertility control. The most important reason for isolating this group is to discover whether they have regretted the action and suffered many disabilities, for the reasons given for sustaining high fertility

³⁰ Tom Kumekpor, "Togo", in *Population Growth and Socio-economic Change . . .*, *op. cit.*

³¹ For a discussion of these issues see "Fertility Trends in Africa", *International Population Conference, London, 1969 Proceedings*, Liège, 1971, pp. 737–843, and especially the Moderator's paper: A. Romaniuk, "Fertility Trends in Africa", pp. 739–750.

(and by community leaders for not too conspicuously supporting family planning movements) is that family limitation would ultimately be regretted. The survey questions were exhaustive in this area (and only a few of the major responses will be quoted here), but the picture (as summarized in Table 2) is clear.

The picture in Table 2 is surprisingly coherent. Nearly all the women are satisfied with their family size; significantly fewer husbands are reported as feeling this way, but many of the dissatisfied have separated from their wives (often on this issue). Perhaps, however, a better measure of complete satisfaction with the decision to have a small family is the 72 per cent who would definitely have fewer than six children if they were to have their lives all over again.

But women (and their husbands) necessarily had to be strong-minded in this period if they were to persist with a planned small family. The major factor in making such decisions possible is a strong feeling in Yoruba society, in spite of being extroverted and strongly interested in family matters, that major personal decisions are one's own business. Middle class Yorubas with small families have told me that they would be astonished if any of their relatives made a comment on this or brought it up for discussion. This is borne out by the fact that over one-third of the respondents reported not knowing their mothers' feelings about their small

Table 2

*A Retrospective View of the Achieved Small Family Percentage
Distribution of Responses of 438 Female Respondents*

(a) Views of achieved family size

Relationship	Good	Bad		Don't know, no response, or not known (inc. dead)
		Should be smaller	Should be larger	
Self	92	1	7	0
Husband	87	1	11	1
Mother	33	0	31	36
Mother-in-law	26	1	31	42
Father	32	1	22	45
Father-in-law	27	1	25	47
Grandparents	20	1	14	65
Siblings	44	1	19	36
Uncles and aunts	39	1	19	41
Children	44	0	10	46

(b) Disadvantages and advantages of the achieved family size

	Yes	No	Don't know No response, Does not apply
Small family felt to be a disadvantage now	7	89	4
Small family expected to be a disadvantage in the future	6	84	10
Experienced disadvantages from the small family in the past	15	81	4
Enjoy children more than respondent believes is the case in most [larger] families	89	5	6
Would enjoy own family more if larger	8	86	6
Misses not having larger family	4	92	4
Disadvantaged because of less assistance with work	7	91	2
Small family costs less to support than large one	69	20	11
Now lack attention when sick because of small family	11	82	7
Would feel safer in times of sickness with larger family	27	59	14
Expect to lack care in old age that larger family would give	9	82	9
Would feel safer in old age with larger family	26	56	18
Have been able to give children more schooling than would have been possible with large family	59	36	5
Less prestige [mostly among friends and relatives] because of small family	9	91	0
Would have been more important in community [especially community organizations] with larger family	21	65	14
Would have been more important in community and regret this loss of importance	3	69	28
Would regard it as a tragedy if succeeded by no children	93	4	3
Would regard it as a tragedy if succeeded only by sons	38	58	4
People say they could not have any more children	14	79	7
People say they could not have any more children and this is distressing	3	90	7
Feels need to explain why family is small	34	66	0
Feels need to explain why family is small and resents this	12	88	0
Would have 0-5 live births if started all over again	72	13	15

family while the proportion reached nearly one-half for fathers. Very significantly in terms of the necessary emotional support for such behaviour, approval has been strongest from sisters and brothers and from the children of the small families themselves. The latter is much stronger than it might appear at first from the table. Most of the children whose views are not known

are still not grown up enough for the society to believe that they should have a view on such matters; of those whose attitudes are known over four-fifths approve of the small family. This might well be a mechanism for the spread of the small family (mitigated by the fact that small families yield proportionately fewer parents of the next generation).

There seems to be clear evidence that most of the respondents did not run into the kind of difficulties widely predicted for the small family. Admittedly many of these predictions centre on the still high levels of child mortality, while our respondents, at least at the time of the survey, lived in a city with much better health facilities than are found in most rural areas.³² Nevertheless, the possible deaths of children in a small family remains the central worry. Only four per cent, even of this atypical self-selected group, would definitely not regard a lack of descendants (or, for that matter, surviving children in their old age) as a tragedy. The relatively large number reporting that the small family had proved to be a disadvantage in the past were explained more by the fact that some of the children had died than any other single cause – and less than one-third of the dead children were consciously replaced by births beyond those already planned (although others may have occurred early in family formation and no distinction may have been made between replacement and further births). Thus, although only one-tenth of respondents lack care now or expect to do so in their old age, one-quarter would feel somewhat safer on these scores if their families were larger (some of the risks entailed by the small family are not entirely that most of the children will be dead but that some will break with family or prove to be poor income-earners).

But the other predicted problems prove to be largely illusory: the small family absorbs all the mother's affection; the problems of extra assistance are few (but this is the town, not the farming area);³³ any loss of prestige is not greatly resented; nor is discussion about why the family was small. Yorubas are often wryly cynical about expenditure or the ability to save; hence the relatively large proportion who felt that the small family brought extra emotional and financial outpourings and were not therefore cheaper. This is not, however, the explanation for those who thought that they could have educated a larger family to the same extent; some of their reasoning was that they would have exerted more pressure on other relatives for what were critically essential objectives; the other was that they might have started the chain reaction towards education among the children with one helping another.

Finally, to what extent are there socio-economic differentials in the creation of small families? Until the 1973 census findings are released, we have no accurate way of determining the extent to which those who achieved small families tended to be drawn from above average groups on the socio-economic scale (we could make estimates from existing data but it seems hardly worthwhile in view of the much better comparison possible from the census material). Certainly they do not come entirely from the elite or from monogamous, stable marriages: over half had no education at all; over half had been wives in polygynous marriages; over seven-tenths were or had been petty traders. Nevertheless, it is salutary to note that almost half had been married to men in urban non-manual occupations and one-quarter had themselves worked in this way.

³² F. O. Okediji, "Socio-economic Status and Attitudes to Public Health in the Western State: A Case Study of Ibadan", in *Population Growth and Socio-Economic Change* . . . , *op. cit.*

³³ The urban-rural contrasts are examined in the Value of Children project.

Amongst the women themselves, no association could be shown between the size of completed family and age, education or ward of residence in the city. There are appreciable numbers of illiterate women living in Ibadan's slums who have been determined to control their fertility. However, significant differentials can be shown in the methods used to control fertility. Table 3 is subdivided by age, partly so as to separate out the over 50 years group among whom reproduction has certainly ceased, but partly also to show change over time in antinatal practice. Education, which is widely taken in West Africa as a prime indicator of social change, is the socio-economic indicator employed.

Table 3
Main Antinatal Method Employed, by Age and Education
 (percentage distributions)

	Age:			50+		
	40-49			50+		
Schooling: (in years)	0	1-8	9+	0	1-8	9+
n	93	57	65	104	45	24
<i>Method</i>						
Abstinence only	66	60	29	87	80	50
Withdrawal ^a	5	4	0	3	4	0
Rhythm ^a	15	12	12	7	13	13
Condom	1	0	6	0	0	13
Diaphragm, jelly, foam	3	4	8	1	0	0
Oral contraceptive	3	4	31	0	0	13
IUD	1	7	6	2	0	8
Orals and IUD ^b	1	4	3	0	0	0
Abortion	4	7	5	1	2	4
Total all non-appliance contraceptive methods ^c	86	76	41	97	97	63
Total appliance methods	9	19	54	3	0	34
Total all methods ^d	99	102	100	101	99	101

Notes:

a Includes charms, herbs etc. used in conjunction with withdrawal and rhythm respectively.

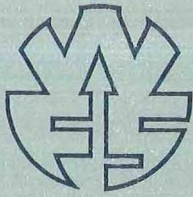
b Used successively

c Does not include abortion

d Totals not equalling 100 because of rounding

The table brings out clearly the fact that, although these women have all succeeded in limiting their families to approximately the same extent, the methods used to achieve this are very different. Among the uneducated, fertility has been controlled very largely by abstinence; but, more than half women under 50 with nine or more years of education have achieved the same end by using mechanical, chemical or hormonal contraceptives. It might be noted that this difference is not merely one of greater education; more education is also associated with higher incomes and the ability to buy these commodities, most of which are relatively expensive.

The table also shows dramatic evidence of change over time. This change is of two types. Firstly, the women over 50 had little chance of using orals or the IUD as their major antinatal method before the end of their reproductive period (the fact that these were the main methods for a considerable number of women, 40-49 years of age, shows how many women began contraception only at a very late stage in order not to regulate births but to stop them). The use of contraceptives increased very greatly over a period of about 15 years (the difference in the average age between the two groups of women). Furthermore, such innovation was at first practised mostly by women with 9+ years of education, but more recently (as shown by the women, 40-49) it has spread to all women with some education and even to some of the unschooled.



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