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INVESTIGATING THE NATURE OF POPULATION CHANGE  
IN SOUTH INDIA  
Experimenting with a micro-approach

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This paper will neither focus on the problems encountered when attempting to examine the context of population change by employing large-scale surveys nor report on the findings of the 1979-83 project on the Origins of Population Change in South India. The former has been treated in two papers<sup>1</sup> and the latter in a series of reports.<sup>2</sup> The aim here will be to reflect in retrospect on what we tried to do and why we chose that course, and then to consider the degree of success attained.

In 1979 we knew that there was something worth investigating in South India. The National Sample Survey claimed that rural fertility in Karnataka was low, with a crude birth rate less than 30 per thousand, more than one-sixth below that claimed for rural India as a whole, and probably over one-quarter down on what it had been 20 years earlier.<sup>3</sup> We wished to ascertain whether this was true even in areas not affected by the Green Revolution, but we also wished to explore suggestions that there were socioeconomic and religious differentials in the acceptance of family planning. Accordingly, census and other records were employed to identify dry-farming areas, over one hundred kilometres from either Bangalore or Mysore, with a population containing a substantial number of both Muslims and Harijans. It seemed preferable that our main centre should contain a primary health unit. It had to be in one of the large areas covered by the Indian Population Project (a program of the World Bank, Government of India and State Government of Karnataka which had established the Population Centre in Bangalore) but we believed it should not have been subject to an intensive campaign by that Project. We randomly chose one centre.

The choice of research site, when only one is chosen, can hardly be said to be typical except in terms of the criteria employed. This is a continuing problem of anthropology and can be justified both on the grounds that it is better to know one place well than all places superficially and also that many interrelations found in one centre probably hold more generally even in very different localities. Ultimately, associated work was carried out in six very different localities and we increasingly had the opportunity to discern what was a purely local phenomenon and what was much more general.<sup>4</sup>

The choice of locality had both its strengths and its weaknesses. The local birth rate had come down to about 32 per thousand, a drop of around 13 points over 20 years. There was something to explain. The different situations of the Muslims and Harijans were important and we were able to investigate these communities to a satisfactory extent only because they were sufficiently strongly represented in the area. On the other hand, this decision meant that we were restricted to a multi-caste village with a population of around 2,500. We soon found that the surrounding smaller hamlets were very different in their social, economic and occupational structures: fewer than ten per cent of adult males work outside agriculture (compared with 50 per cent in the larger village), education is more recent and at lower levels, and most of the population is usually of a single caste.



Almost half the population of India live in hamlets of this kind. We soon added eight of them to our study totalling another 2,500 persons. This raises an important question of interpretation with regard to anthropological studies in India, and perhaps elsewhere. Most of the anthropological research seems to have been done in larger villages where there is a greater diversity to study and where it is often easier to find somewhere to stay. Nevertheless, clearly much is lost by ignoring the smaller, simpler centres.

Our first focus was on changing fertility, although we later found that this cannot be fully understood without a substantial knowledge of changes in family structure, marriage, morbidity and mortality. The single-theme survey or investigation has very serious limitations.

#### The method of investigation

There were important decisions about how to start such an investigation, who would do it, and how to do it.

We retained much of the demographer's armoury. We carried out initial mapping for census and sampling purposes. The maps became more important than we had anticipated because we subsequently employed them to show distributions of characteristics and behavioural phenomena that tended to suggest new ideas and to generate hypotheses for testing. We carried out a full initial census which identified all the population and permitted the employment of retrospective data to determine demographic trends. Because subsequent household research vastly increased our knowledge, the initial census data were to be considerably modified. This was a salutary lesson in terms of reliance on one-time census or survey findings. One of the greatest problems is found in securing adherence to definitions of who actually constitutes a household and normally resides in it; even some of the most sophisticated households, who seemed to have fully understood our definitions, were later found to have erred, usually by including members who usually live elsewhere. A few deliberately misreport because of apprehension about land reform and other fears.

We also kept records of vital events and annual censuses, but probably did not devote as great a proportion of our resources to this effort as would have been the case if our major focus had been there rather than on the social and economic context of demographic behaviour. Nevertheless, the continual checking and updating of household schedules on every visit made it difficult for vital events to pass unrecorded even if they had been missed by the vital registration surveys.

Our demographic background also played a part in the decisions taken about investigators. The three principal investigators could have carried out a typical anthropological investigation, but we decided that we wished to increase the scale of the work and usually collaborated with about eight to ten assistants although the number fell for some purposes. The original assistants came from a variety of backgrounds but were selected because of rural origins and training, usually a graduate degree, in the social sciences. Although they had been subject to prior training, most of the training was essentially apprenticeship on the job.

The central aspect of such research is that the research assistants should not become either survey interviewers or independent investigators. The responsibility for formulating new hypotheses, or at least for identifying those that should be explored further, and for exactly specifying them, must remain with the principal investigators. This is achieved partly by placing substantial emphasis on the importance of ideas and hypotheses and



encouraging assistants to report all their ideas and hunches. This can be institutionalized in the form of nightly sessions where the whole team discusses the experiences of the day and the new ideas thrown up. To some extent, it also happens when the principal investigators debrief assistants after household visits. Nevertheless, the chief instrument for developing new hypotheses is long, investigatory interviews by the principal investigators themselves where new leads are identified and followed even when dealing with themes which had not been previously strictly specified as part of the investigation. Thus, the body of hypotheses expands continually with a mounting feeling of excitement in the survey team, and each day is clearly a new one and more advanced than those that preceded it. Clearly, the original body of hypotheses owed much to reading and the field demographer misses a great deal, and undertakes much unnecessary work, if he starts his research without being saturated with the anthropological, historical, economic and agricultural literature on the region.

Even at the best, a team of assistants cannot fully communicate all their thoughts to each other. Nevertheless, even a team of eight or twelve cannot cope with getting to know well more than a fraction of 800 families. We have constantly wondered how the single anthropologist can do much more than scratch the surface. Does a man, for instance, largely get to know those adult males who often appear in public places? Does he get to know well a small selection of families who are atypical in that they are easily accessible? We were determined that this would not happen and accordingly used a sampling approach to determine which households we would investigate. However, at the outset our enquiry into fertility control had adopted a much more deterministic approach, namely the investigation of all households characterized by having at least one woman 30-39 years of age, currently married and living with her husband, and with at least three living children. This was fairly satisfactory until we wished to extend the enquiry to cover other aspects of demographic behaviour when it became clear that we had devoted a great deal of effort to familiarizing ourselves with an atypical cross-section as far as these additional studies were concerned. Even when we studied fertility control and decision-making, it had the drawback that we could not get to know the small but important group who begin to think of fertility control or practise it at lower parities. However, there were always other families outside our sample whom we came to know well from everyday contact. They were often above average in education, loquacity or self analysis and necessarily contributed to our hypotheses if not to our detailed study files.

With our expanding range of hypotheses and matters to investigate, we interviewed households repeatedly, often for hours at a time. At first we intended to talk to each individual separately, but such interviews formed only a small proportion of the total. More often we spoke to a group whose number and composition changed over time according to work needs. Our suspicion of group interviewing lessened as it became obvious that the interaction of a number of people led far more often to the memory of newer or more exact information than it did to the suppression of lesser personalities by more forceful ones. Where necessary we followed into the fields individuals who appeared to have something more to disclose or we made a later separate appointment. Often the group sent for someone who had specialized knowledge.

At first we tended to maintain the distinction between interviewer and interviewee and not to disclose our full hand with regard to the research. As we became more experienced, and on subsequent interviews, we tended more and



more to take them into our confidence. We revealed our working hypotheses and our lines of thought and they either provided additional information or produced evidence to show why these ideas were untenable. Often they worried about interpretation, consulted other people, and came back with new ideas.

Interviewers kept notes, to be later written into household files which were periodically synthesized and summarized. The principal investigators also kept anthropologists' note books which proved to be indispensable not only for recording important points but more significantly for providing the material from which many hypotheses were later developed. The original census in the larger village was not extensive because we wished to obtain as much material as possible from the in-depth interviews and because we did not wish to force people to adopt fixed responses before the more flexible discussions. Both these concepts proved to be wrong. Unless one employs a fixed schedule in the anthropological interviews then the simpler and more quantifiable data are not always collected completely and are frequently not comparable. There was also little evidence that previous responses were either remembered or consciously adopted. Consequently, we expanded the base-line census-survey for the smaller villages and supplemented that of the large village. This proved to be invaluable for obtaining an ecological perspective of the whole area, for obtaining a pattern of systematic responses, and for testing such responses against the evidence obtained from the alternative approach.

As we began to examine new selected topics, we increasingly undertook small highly focused surveys on such matters as the arrangements of marriage, the partitioning of households, and the enrolment in and the removal of children from school. In all cases, such focused surveys were preceded by anthropological approaches and were followed up by more work of this type.

The heart of much of the work was the participant observation enshrined in anthropological methodology. We found that only some aspects of matters which interested us occurred sufficiently frequently or sufficiently publicly for the method to work without a good deal of contrivance especially in the form of the directing of conversations. The team lived in a range of households and this provided different participatory experiences. We decided against the employment of paid informants, but their role was replaced by various constant companions who took an increasing interest in finding out exactly what had happened in their community and how change had taken place.

On occasion, we took the opportunity of some unusual event to redirect research in the hope of understanding that event and drawing some more general truths from the experience. Perhaps the best example of this was the redeployment of forces during 1983 as the drought in the southern Deccan worsened, in order to understand the nature of risk and the implications of risk aversion in more normal times.

Our central concern was with change, both demographic and related socioeconomic change. We necessarily had an historical perspective. Increasingly, we found it of value to discuss all our topics with the more elderly people and the very old became constant contacts. In this type of investigation, supported as they were by much about the past that had already been learnt, they became articulate and often provided better descriptions of the situation decades ago than of the present.

In all our questions, there was a continuing emphasis on change, on the sequence of events, and on the reasons that things altered. There is very little distinction between good social science and social history.



### The nature of evidence and proof

The more detailed field work of this type that one does, the more everything begins to fit into place. The proof of this lies in the fact that the investigator can eventually provide reasonably accurate answers when new questions are posed about the society without having to rush off to find respondents. This knowledge of the society comes from a preceding density of questions that no survey can possibly approach. It also arises from the flexibility of the interviewing method whereby new leads can at once be followed. One increasingly wonders whether the relationships shown by the statistical associations between variables in a large-scale survey are much more than abstractions associating pairs of reflections or proxies with each other.

Two other problems associated with the survey approach become ever more apparent. The first is the inevitable tendency to identify the respondent as the chief actor in the events. Usually, she has been forced to supply evidence and views about matters which are regarded by the patriarch or his wife as none of her business and as outside the area where she could make decisions and act. Increasingly, one wonders how the survey can understand anything without a family, neighbourhood and community context. The second is that the necessary rapid interviewing and the search for a limited number of answers, usually in some priority order, mean that respondents give a completely artificial impression of conflicting with each other in their testimonies. It is the mechanism of the survey itself which produces the wide distribution of evidence. Thus, in the work on the 1983 drought, we found a surprising consensus, but no large-scale survey would have achieved this.

The most important issue is whether we just receive and record a body of impressions or whether we can establish some findings with a high degree of probability. Our census-surveys or subsequent surveys of much larger populations can produce some satisfactory significant statistical associations, although, even in this case, it is the micro-approach work which will provide the best evidence on the direction of causation.

However, the justification for much of the effort expended on micro-approach investigations must be of a different type, that which justifies anthropological research. This method of proof is a form of the scientific method. It rests upon the formulation of hypotheses and the subsequent attempt to disprove them by accumulating evidence. It rests also upon prediction based on the reasonably secure informational base already established and the investigation to ascertain whether the predictions hold good.

One problem is the type of evidence adduced for proof or disproof. Much, although not all of it, is necessarily subjective and judgmental. Nevertheless, its total mass can be very convincing. Nor does one fall into the survey trap of basing proof only on the most quantifiable evidence when such data are often of only secondary relevance.

Perhaps a more serious problem is the generation of hypotheses. One solution adopted by anthropology has been to focus on a limited number of questions and to build up a major body of knowledge. This may one day be possible also in the area of the cause of demographic change but as yet there is nothing comparable with the body of research findings and resultant theory which has been amassed, for instance, on kinship. One way out is to mine the literature available on demographic change in the writings on the region (or further afield) by anthropologists and others. Nor should one underestimate historical or religious writings. In South India there is a body of social



analysis which extends back for 200 years, much of it connected with the need of the East India Company to understand the society. Much can also be gleaned from religious writings, which, in India, stretch back for four millennia.

Nevertheless, we found that the major source for our hypotheses had to be our own research. Some were generated from conversation and chance discoveries during a single interview. However, the most important source was the files which were accumulating on each family. Originally, we had regarded these files much as demographers are prone to do, and had thought of coding their contents. Fortunately, we realized that their information was too important for this. We repeatedly browsed through them, recalling as frequently as not the discussions which had given rise to them, and jotted down rudimentary hypotheses. We sharpened these in group discussions and in our next interviews until we had a formulation which could be fitted into our check lists for use in household discussions. These check lists were never employed as a list of questions but sooner or later we got around to all of them in probing discussions.

Some hypotheses were discarded as they proved to be incorrect or insufficiently general. Most were made more specific. Some were used as the basis for questions in small focused interviews. Ultimately, the intellectual responsibility for deciding upon the extent to which hypotheses have been disproved or still largely stand must be borne by the principal investigators and reported by them.

It is also possible to test the reconstruction of the past and of change which has occurred with historical and archival material. In India these sources are massive and are under-used by social scientists.

When we began the work, we did it with a strong conviction that one of our major purposes and justifications was to build a better questionnaire for large-scale sampling surveys. We still think that all such surveys should be preceded by and accompanied with micro-approach work. Nevertheless, we have increasingly come to agree with the anthropologists that the findings of the micro-approach work are an end in themselves. The minute examination and the interrelations shown build up a body of knowledge that is valid in itself and probably can be predicted to be largely true for other areas with changes arising from local circumstance. Other local work at a later date may confirm this but it is not the responsibility of the initial investigators to have to organize this work. Similarly, the investigators can search survey and other official data for patterns which could be predicted or explained by their findings, taking equally careful note where they seem to be negated. The primary subsequent task of the investigators is to contribute to the building of theoretical frameworks which can be employed by themselves or others for designing further research and tests or for constructing more general theory.

This is not to say that such investigators may not also have an interest in designing micro-macro-mix research where both components can throw light on each other and contribute to the other's research design in the one program. Such work can help throw light on the extent to which the small area studies can be generalized. If an archival component is also built in, it can help to assess the retrospective data derived from both the micro and macro approaches. Often it can provide guidance for the interpretation of those retrospective data; it cannot replace them because many of the concerns of those interested in demographic change are about matters which are almost entirely omitted from the written historical record.



Some findings, with attention to the extent to which a micro approach was necessary

The most important finding of the micro work was the extent to which many types of change are interrelated. Social, political, economic and educational forces exist in the one context and affect each other and eventually produce demographic change. There was a not unconnected parallel between mortality and fertility decline, and significant statistical associations could be shown, but the connections were common relationships to factors like increased schooling, related to the Independence Movement's determination to put all children into school, and to the greater development of an exchange economy at the expense of the subsistence one with its fatal closeness to the cycle of plenty and deprivation. We built up a significant picture of changing relations between the generations, especially the erosion of patriarchal authority, as education, transport, the growth of non-agricultural employment and the coming of the mass cinema changed expectations and opportunities. We followed among a still religious people a transformation of religious interpretation as a broadening spectrum of behaviour was felt to be significant only in the secular sphere. These are matters which may later be probed by the survey but which necessarily must have their forms delineated previously by more intimate work.

Among the old we were able to view an earlier society where sexual relations were postponed after marriage among Hindus so that no birth could take place during the first year and where sex was infrequent or non-existent between spouses long before the wife reached menopause. It was a society where weaning decisions were usually dictated by the next pregnancy and where that pregnancy was postponed not only by lactational amenorrhoea but also by post-partum sexual abstinence. This was a society where the state had few views on any of these matters, but where religion, especially in its most local manifestation, certainly did, and where relatives, particularly of the older generation, were the instruments for policing that morality. That policing weakened, first in the area of the control of coitus. It was changes of this kind that made it easier for the older generation among Hindus to withdraw to an agnostic position once the state intervened with a new morality with regard to fertility control. Among the Muslims, who held that they had a revealed and written morality that impinged on these specific questions, the old did not abdicate so readily and Muslim fertility remained well above that of Hindus. Their resistance to what they regarded as the blind Hindu acceptance of a state-led socio-theological morality explained the religious fertility differential which we found in the area and which we believed threw light on such differentials reported across India.

It was first through our work on marriage and the desired arrangements that we learnt just how deep was the fear of periodic adversity and the desire to link some of the family fortunes to non-agricultural sources of income. Given this new lead, we were able to link declining farm size, arising in general from population growth and in specific cases from land reform, to the increased desire and opportunity for some family members to work outside agriculture; we could then perceive the links to rising levels of education and a stronger demand for fertility control. We understood the local people's fear of life-cycle bottlenecks threatening their plans to educate children and to secure the desired jobs or marriages for them. It was only the 1983 drought which starkly threw into unusually high relief the operating of all these processes. The micro approach allowed us to distinguish between the ideal and the actual in the performance of the family planning program. It revealed the role played by the program in intervening



in family decision-making and in making concrete vague feelings of economic apprehension. It made more concrete our own feelings about the role in fertility decline played by the program when we found how few local people doubted the primacy of their intervention as a causal mechanism. We understood too the resistance to the program when we found that not only ordinary people but also the elites, and even those in the program when not acting in their official capacity, believed that it was only too probable that sterilization would maim a woman for life and could easily kill a man.

We had come to the investigation with apparent strong evidence that fertility had declined steeply in the second half of the 1970s in South India even though the Emergency had not impinged strongly in the area. We found that there had been sufficient additional pressure to explain the steepened fertility decline and we found also why the public picture of events in the region during those times was different.

Perhaps the most revealing fertility discoveries were in the area of family planning decision-making. It became possible to document the throwing up of the idea of sterilization, and of the hints, nagging and pressures. It was clear just how pressures could build up on the daughter-in-law, how she might fear her husband's vigour, health and employment being endangered by vasectomy, how his death would profoundly alter her position, and how a sacrificial role, well in keeping with social and religious tradition, might appeal to some. Yet, in most families the ultimate decision was hers, as was so often and so relentlessly stressed, and a situation as simple as this was all that was likely to be reported to a survey interviewer enquiring about who made the decision. The survey analyst would then report an autonomy of females of the younger generation greatly at odds with the actual situation.

Only participation in the community could reveal just how strong the pressure of the 'establishment' was in ensuring that only the family planning message was heard in public, whether at a school meeting or that of the panchayat (village council). Even complaints about family planning are quickly stifled by the bureaucrats, the educated and the rich, who frequently publicly advocate the use of the program. They partly regard themselves as the representatives of the government or of the national interest and they partly have been convinced by the population debate of the truth of earlier lurking fears, namely that they have much to lose from the unbridled reproduction of the already numerous poor. During the Emergency the landlords of the larger village agreed not to employ agricultural labourers who had been targeted by the family planning program for having three or more living children but still resisted sterilization. It is this same group, close to those who control the health unit, who are likely to experiment with the IUD while agreeing that there is little point in the program advocating any method other than sterilization to the feckless poor.

It was soon clear that any study of either fertility change or the decision-making structure of families had to take into account the persistent rise in average female age at marriage, now nearing 19 years. Only detailed enquiry about successive marriages within families revealed the persistent swing from a nominal bride price to a kind of open market dowry system within each caste. This had first appeared among Brahmin families, then among peasant castes, and was now invading the backward castes but not as yet the Harijans. The reports of the late eighteenth and early nineteenth centuries revealed a time when bride price had been sufficiently high to deter many marriages. The elites, well aware of the opposition of government and the nationalist movement to dowry, told us that they paid none, and certainly would have reported this way in a survey. Only when we questioned them about



their obvious financial worries over a forthcoming marriage or the lavishness of the marriage when it occurred, did they explain that they gave presents rather than dowry. They were proud of the expenditure in comparison to what their peers made, but most regretted that the situation had come about and few offered any argument along the lines of Sanskritization that it improved the religious or social standing of their caste. They explained the rise of dowry in terms of a marriage squeeze rendering bridegrooms scarcer (a demographically plausible argument) and the greater market value of the educated young men now becoming available in greater numbers. It was the endless discussions on this latter point that proved to us just how valuable was a relative with a secure income from outside agriculture and how apprehensive were families of facing periods of crisis with only agricultural incomes.

If it had not been our experience over the years that only one non-arranged marriage had taken place, an occurrence which gave rise to profound problems not yet solved, we might have looked in the data for evidence to support a tendency to regard arranged marriage as an institution likely to collapse.

Marriage was seen in a new light when its need was expressed most commonly in discussions of the household labour force and of why it was the departure of a daughter-in-law or the sickness of her mother-in-law that meant a new marriage must be arranged. Meeting the dowry for such a marriage was an argument understood by all as to why the partition of the larger family should be postponed longer so as to retain a single economic unit with maximum efficiency for saving.

It was conversations with teachers, lamenting the withdrawal of their best girl students just before the important Senior School Leaving Certificate examination, which brought home to us the strength of the fear still felt over freedom for a girl who has reached menarche and is not yet married. One family consulted a member of the team, as an outsider, about whether, in such circumstances, menarche could or should be hidden for a short time by postponing the related ceremonies.

Observation and conversation with regard to specific marriages revealed financial aspects which should be quantifiable but where full disclosure might be impossible to obtain. Clearly one should attempt to distinguish between wedding costs and those constituting the dowry, and also between dowry costs before or at the time of the wedding and other subsequent payments of gifts. It is also clear that much of the payment finds its way to those who provide services or goods for the wedding and increasingly to the younger generation who are marrying, so marriage payments are not merely a rotating fund that subsequently allows another marriage as is largely the case, for instance, among many pastoral people of the African Savannah.

The research where the micro approach had the clearest margin over the survey, and where we concluded that most surveys must yield close to meaningless data, was in the area of morbidity and mortality, especially the former. So many sick people believed themselves to be experiencing either hot-cold imbalance from incautious eating or other behaviour, or thought that they were suffering from divine punishments or other inflictions of extra-human origin, that even the most careful cross-questioning failed to secure a statement of much of the illness. People who were clearly very sick reported that they were not ill, and babies died even though they were said to be in good health. Indeed, the term for describing a complex of infantile disorders, including diarrhoea and infantile cirrhosis of the liver, was balagraha or 'divine visitation upon children'. Not only were the causes of



much sickness deeply embedded in religion, but, as a consequence, so was the treatment. It was a long time - far longer than most surveys are in a specific area - before we convinced most people that we were also interested in afflictions that were not the kind appropriate for treatment in the health centre and that we were interested in alternative treatments. They believed their reaction to be just common sense and politeness, for researchers from the modern segment of society are likely to be concerned only with the institutions deriving from that segment. Indeed, one reason why those institutions are used is that those who have gone to school believe that they have entered a world of such institutions. Some refusal arises from a fear of modern medicine and a belief that the interviewers would order such treatment. Sometimes it is the investigators who are wrong. Survey workers in India often assume that the alternatives to modern medicine are the practices associated with the Hindu or Muslim religions, Ayurvedic or Unani medicine. In our district the input from practitioners of these therapies was trivial compared with temple rites, charms, herbalism, and a wide range of magical practices and services offered by a new type of 'quack'.

These are not the only difficulties in explaining the underutilization of modern health services. There are costs in terms of payments and presents which are not officially supposed to be made, of income forgone not only by the sufferer but also by those who accompany him, as well as transport and food costs. It is difficult to obtain adequate quantification even by methods much more tedious than those employed by surveys because of a reluctance to count a present given reciprocally in return for assistance as tradition dictates, let alone to estimate for the sick how much income they really did forgo.

There are matters of fundamental importance when assessing morbidity and mortality which can only be examined by a participatory approach but which are exceedingly difficult even by these means. Feeding occurs in successive order by time so that the men who eat first do not know either what is left for the women or whether they ate anything during food preparation. Children may be given the same amount at first but little girls are restrained from demanding more to a greater extent than little boys as part of their training for femininity rather than as a conscious effort to achieve feeding differentials. Mothers traditionally did not draw attention to their sick children or demand treatment until their mothers-in-law or husbands took notice and action. In terms of mothers-in-law this makes some sense as they care for young children when the mothers are in the field. The more education the daughter-in-law has the more the balance of decision-making changes but this is regarded as so inevitable and obvious that it is very rarely reported to investigators.

Attitudes towards health and treatment change with secularization as some spheres of action are removed from the religious domain. This may well happen as a person becomes apparently more religious in a movement toward greater devotion to a major Hindu god at the expense of adherence to the village goddess. Most survey measures of religiosity would fail to record this as a secular shift. With the secular shift, there may be no intensification of pollution avoidance but that avoidance will be more closely related to bacterial rather than spiritual pollution. Whether this shows up in a survey as greater cleanliness depends very much on the number and type of questions asked.

The work on family structure brought out clearly the life cycle movements from more complex to nuclear families and the persistence of one stem household in each larger family to provide for the older parents. It



showed how the proportion of nuclear families gives little indication of the family situation when most conceptions and first movements toward fertility control take place. Much of this work can be done by well constructed surveys provided that they find ways of accurately measuring family and residence by more than crude cooking-pot measures.

#### Concluding note

Much of what is in surveys is not wholly wrong. Nevertheless, the picture is usually so marred by important omissions that it gives a completely wrong impression. What the survey does contain is a kind of lowest common denominator measuring those phenomena which occur everywhere. What it leaves out is the cultural and social diversity which really explains why fertility or mortality levels stay up in one place rather than another. What they lack is not only a sense of place but also a sense of time which is disastrous because adequate explanations of social change are essentially historical ones.

It is impossible to undertake any intensive micro approach work without increasingly wondering what a large-scale survey moving quickly through the area would pick up. Yet even the micro approach researcher is tempted by a curiosity about larger patterns and numbers (convincing by their very size) to employ the small-scale findings for designing better sets of questions for the surveys. However, this is done in the knowledge that much of the interpretation will depend on an understanding from alternative sources of just how the family and community operate so as to produce this result.

However, the prime object of micro approach research, beyond reporting immediate findings, should be the building of a better body of theory, which has a value both in its own right and as a stepping stone to more generalized theory or other micro research or more satisfactory macro research.



## FOOTNOTES

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<sup>1</sup> References 8 (to be tabled in the Workshop) and 9.

<sup>2</sup> General: reference (1); fertility and family planning: references (4), (6) and (7); family structure: reference (5); marriage: reference (3); morbidity and mortality: references (2), (10) and (11).

<sup>3</sup> Cf. Registrar General of India, Sample Registration Bulletin, 17, 1, June 1983, Ministry of Home Affairs, Government of India, New Delhi:3-4; Department of Family Welfare, Family Welfare Programme in India: Year Book, 1980-81, Ministry of Health and Family Welfare, Government of India, New Delhi, 1982:29.

<sup>4</sup> i.e. in two contrasting villages over 100 kilometres to the south by members of the Population Centre staff, Sumitra K. Potnis and P.N. Sushama; in a village area over 100 kilometres to the north-east and in a Tamil Nadu village by A.N.U. Demography Department Ph.D. students, A. Shariff and M.Guruswamy; and in two areas within Bangalore City by Population Centre staff, Sheila Chandra Mauli and Sashikala Kempawadkar. Important supplementary enquiries were also carried out in Bangalore by Revathi N. and Kusum Kumari.



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