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HOW DID THE GREAT DECLINE IN FERTILITY START?
A study based on retrospective interviews

Sune Åkerman

There has been a great deal of discussion as to who it was that in fact developed the so-called 'family-reconstitution technique' within microdemography. This, as we know, is a method of reconstructing families and households from rather scattered information about private individuals in different sorts of Church Archive material. There seems, however, to be an international consensus as regards designating Louis Henry, the grand old man of French demography as the real pioneer. From the point of view of Sweden we can modestly point out that K.A. Edin used identically the same technique as early as the beginning of the 1910's, and that Hannes Hyrenius also worked in a very similar way in his thesis on Estonian Swedes which was published in 1941.

Be this as it may concerning the claim to its origin, the method has revolutionised research work, especially in the so-called pre-statistical period from 1500 - around 1800 in many countries. We should also, from the Scandinavian standpoint, be so generous as to recognise that Henry has meant a very great deal as a source of inspiration and propaganda for the new methods. He has also, in several important respects, refined them. Incidentally, the 'Uppsala Group' has been able to make a contribution towards this development during our study of families and households in a historical perspective. I am referring to the so-called family cards which represent a better way of clearly recording family changes over time than the technique which was used by French researchers for example. The latter have, furthermore, mainly been concerned with building up aggregated statistics and tables on the basis of the reconstituted families.

Our method with life-lines synchronised with each other, and with the successive introduction of important demographic events (see figure 1),
has come to be used within, for example, the presently active German historical-demographical research. It is, moreover, highly suitable for 'display' in connection with data processing, as we have been able to demonstrate at several conferences both in the Scandinavian countries and internationally. Here one can in an instructive way visualise in just a few seconds what has happened in a family in the form of important demographic events stretching over decades.

Figure 1. Example of family card display.

**Family diagram**

Möklinta A:1 page 44, 41 and 8 no. 20 Forneby, Hillersbo and V. Berntsson. Peder Olofsson (farmer)

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**Demographic data**

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<th>Name</th>
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<th>Mother's age</th>
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<td>Wife</td>
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<td>1832</td>
<td>1853</td>
<td>21</td>
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<td>67</td>
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<tr>
<td>1) Son</td>
<td>Olaf</td>
<td>1856</td>
<td>22</td>
<td>1656</td>
<td>9</td>
<td></td>
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<tr>
<td>2) Daughter</td>
<td>Elisabet</td>
<td>1855</td>
<td>23</td>
<td>1656</td>
<td>1</td>
<td></td>
</tr>
<tr>
<td>3) Daughter</td>
<td>Brita</td>
<td>1859</td>
<td>25</td>
<td>1659</td>
<td>12</td>
<td></td>
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<tr>
<td>4) Son</td>
<td>Mats</td>
<td>1858</td>
<td>26</td>
<td>1658</td>
<td>10</td>
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<tr>
<td>5) Daughter</td>
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<td>1670</td>
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<td>6) Daughter</td>
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<td>1859</td>
<td>30</td>
<td>1670</td>
<td>10</td>
<td></td>
</tr>
<tr>
<td>7) Daughter</td>
<td>Anna</td>
<td>1865</td>
<td>33</td>
<td>1670</td>
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<td>8) Daughter</td>
<td>Malin</td>
<td>1870</td>
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<td>9) Son</td>
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<td>40</td>
<td>1670</td>
<td>10</td>
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<tr>
<td>10) Daughter</td>
<td>Margareta</td>
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<td>41</td>
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The advantage of family cards is, moreover, that they so easily lend themselves to a direct analysis of the families at any given stage of growth or decrease. One thus obtains a rare sharpness of detail in the information, which can illustrate the approach which within international research is usually described by the expression that one sees the 'family as a process'. It is only with the help of such information, combined with important background data of an economic and social nature, that one can express oneself with any degree of certainty about such important questions as possible family limitation in the society of the past, or household structure and function, or the occurrence of so-called extended families in the traditional agricultural society.

But, unfortunately, even the family cards have their limitations. One might therefore maintain that a great number of interpretations of historical demographical changes and conditions have come into being rather too quickly (with or without the family cards as a unit of analysis). It is symptomatic that researchers not uncommonly arrive at completely different conclusions when trying to interpret the same empirical observations.

This uncertainty as to which factors, attitudes and values may have been able to lie behind the dry data, has led me into an experiment which may, perhaps, be able to give the answers to at least some of the questions which are most important to have answered in connection with our research into the great decline in fertility during the period from the 1880's up until and including the 1930's.

Why should one not simply be able to go out and ask the people who lived through the drastic change in values and behaviour which the decline in fertility constituted? On the condition, of course, that a sufficient number of individuals are still alive and in such a condition that they can be expected to answer our questions. On this point there could be cause for scepticism. The situation was not improved by the fact that the questions that must be asked would be of such an extremely delicate nature and concern the most intimate relationships in human coexistence.

In my opinion, however, there was a great deal to be gained and it could therefore be worth making a good deal of effort and to run the risk of failure. In order to carry out an investigation by interview of any extent one needs economic resources on quite another scale than, for example, for archive studies. Would it be possible to interest a research fund or other financiers in such a risky enterprise as the one sketched above?
Resistance to oral history

The greatest problem in starting what would later come to be called the 'Rätan Study' was still largely of an intra-disciplinary nature. It is a well-known fact that not least Swedish historical research for a very long time has been influenced by resistance, not to say hostility, towards oral tradition material. Classical source criticism was practised primarily on written sources and archive documents and was given a very strict structure in the so-called Weibull School. The Weibull brothers excelled at theses and essays which would become standard reading material for several generations of students in a severe criticism of historical reconstructions which were based on second-hand sources and other information which did not completely fulfill the criteria of contemporaneity. There were also many examples of the tendentious structure of tradition material whether it at any point in time had been noted down or transferred orally between generations. This applied not least when it was a question of the literary dimension of the text.

An attempt on the part of the 'Uppsala Group' together with the Emigrant Register in Karlstad, to make a systematic collection of emigrant memories and recollections from the extensive labour migrations from Värmland was only partially successful. Especially labour migration, which from a social and civic perspective was of such great importance, had only left behind scattered fragments in the memories of the Värmlanders. The fact that the emigration to America has survived in the minds of the population considerably better is easy to understand: partly because the memories have been reinforced in different ways, among others by those who returned, correspondance with relatives over there, newspaper articles and books and not least by Vilhelm Moberg's emigrant epic, and partly because emigration across the ocean was so much more dramatic than migration to the saw-mills along the coast of Norrland or to the timber-yards inland.

This collection campaign was undertaken in 1971 and was considered by us as a suitable project for cooperation between university researchers and local historical researchers. The project showed then, among other things, that a large and substantial part of contemporary history had been able to disappear almost completely from the memories of a population in a considerably shorter time than one hundred years. If we had lacked other possibilities of reconstructing the course and extent of labour migration, our picture of this phenomenon would undoubtedly have been distorted, if, indeed, we had been able to form any idea at all of what seasonal mi-
igration meant for the early industrialisation of our country.

With the emigration to America the situation is a little different. Here the difficulty lies rather in that people's own more or less direct recollections have been mixed together with literary material and myths surrounding the great migration. Now this is actually very well-documented in a number of sources which complement each other and allow for cross-checks and quantifications. Notes from memory play therefore a mainly marginal part in emigration, even if in some cases they can be rather important. Our collecting work in Värmland can, however, serve as a guide for anyone who wants to base large parts of an account just on oral information in retrospect, and a long time in retrospect. It is, for example, striking how the conception that whole communities were more or less depopulated or evacuated arises now and then in such enquiries. This is an assertion that we now know to be fabricated and can thus almost be described as a myth, a dramatisation of the transocean movement, which indeed was an upheaval on a massive scale.

There is, then, no reason to underestimate the difficulties in practising 'oral history'. Still it can be argued that Swedish historians have kept rather too low in this area, while increasing interest within international research, and this applies by no means only to African studies, has been shown in collecting oral material. The criticism which has been aimed at our caution and conservatism as regards oral sources during recent times is therefore on the whole well-motivated.

Retrospective interviews

Intra-disciplinary resistance is not only localised to the ranks of historians. Among sociologists there seems to be a widespread suspicion of interviews in retrospect, and also especially if a long time has passed since the events one is trying to reconstruct occurred - and memory can, of course, in such cases as these, play tricks. This scepticism reminds us a good deal of the discussions which were held among the stiff source critics of historical research. This is probably not a coincidence as it is quite common that sociologists with an interest in a long time-perspective have also previously studied history, or at least acquired an elementary knowledge of the methods which are used in historical research.

During the long planning work with the Råtan Study, I time and time again came up against this scepticism about retrospective enquiries and I
also found it quite difficult to seek arguments for the technique, as I myself had the same suspicions with me from my past. But there was one argument to resort to and this was no weak argument, that the type of information I wanted to get at could not be obtained by any other method. This fact, I thought, should motivate making a serious attempt at conducting retrospective interviews with a group of people.

Questioning the elderly

To conduct an interview is something of a science in itself. There is a good deal of literature about the methodical difficulties which are connected with interview work. Within academic subjects such as ethnology, social anthropology and sociology, interviews have become the normal way of gathering data on an object of study, often, however, in combination with other information.

But sociology has been rather persistently concerned with problems of the present time and the near future. This fact can be illustrated excellently with the help of a problem area which lies very close to the Räitan Study, namely the current decline in fertility, which at the moment is the object of intensive interest on the part of researchers and politicians. Even here one meets great hesitation about 'historically' taking a chance and, among other things, questioning 45-year old women, as intended according to the original plans for this large project within the framework of S.C.B. (Swedish Central Bureau of Statistics). Any genuine interest in what happened in the 1920's can hardly be expected among researchers who work with such a short time-perspective. Still one can assume that basically it is the same process as we are studying.

Within the ULF Committee, on the other hand, which is investigating the living standards and environments of different groups in the present society, interest has also been shown in the elderly and pensioners. Unfortunately, the interviews which up until now have been carried out have not been very successful. This could partly be due to the fact that primarily it has chosen to work in a large-town environment. Unwillingness to cooperate with the researchers has been considerable, some people have not even let them in over the threshold. These experiences could also dampen the possible optimism which we could have had before the Räitan project. The risk of refusal and of a large non-response was considerable.
Predecessors

Are there, then, no encouraging experiences from anywhere where retrospective interviews have been tried out in the area of family planning? The answer to this is that there are, in actual fact, a few examples of successful interviews to be found. Now I am referring primarily to John Caldwell's and Helen Ware's investigation into the use of contraceptives in Melbourne, which was carried out at the beginning of the 1970's, and was presented in Population Studies in the year of 1973. This, however, is also concerned with quite young people. But even Caldwell-Ware complain of 'the surprising lack of information on changing contraceptive practice over time in Western societies and on the use of retrospective data'.

There is no reason here to go into the results of this team of scholars. They concentrated more on the particular techniques of contraception (the use of different methods of preventing conception) than we have done in the Rätan Study. Above all they wanted to clarify how these techniques changed over time. But on the level of methodology, it is encouraging to notice that they succeeded in conducting their investigation without too much non-response. It was also highly encouraging to find that several groups of immigrant women had taken part in the interviews (partly with Catholic and Greek Orthodox backgrounds). These were thus groups which normally have cause to be especially wary about the form of enquiry in question.

At a late stage of the planning of the Rätan Study I also learned of Chris Van den Broecke and his research in Belgium. Van den Broecke has up until now only produced a preliminary research in Dutch, but from this it appears that he, with a very small non-response (3%, N=400) managed to question elderly people in several towns in Flanders. This mainly concerned working populations. What Van den Broecke has particularly established is that the people who married during the mid-war period and from the start practised strict family planning constituted a middle generation, who experienced great insecurity in many respects, not least, of course, due to the international conflicts and the occupation during the second World War. But also on a sociopsychological level there were great strains on these people, according to the author. Most of them had been brought up in large families, but could not, or would not, have more than one or two children, which apparently implied that security for the future was threatened. Society's old age welfare, pension scheme etc. had not yet been satisfactorily established. Moreover, mobility was a negative factor. The hope of obtaining support and care from children and grandchildren
had now been jeopardized fundamentally. Future social policies could only be guessed at.

As regards the methods of enquiry of the pensioners, Van den Broecke chose quite directly to go into the delicate questions about sexual relationships and family limitation and different techniques of arriving at the desired number of children. Despite this the non-response was, as mentioned, small. The fact that the Belgian study was able to successfully account interviews with women and men and that Van den Broecke had, among other things, used these interviews with couples to a source-critical purpose, which is important in this type of enquiry, all these matters were important for our final planning. It was also positive that a group of workers could be canvassed. Previously it has mostly been the middle class which has been the object of interest.

When reporting on the background of the Rätan Study it is natural to mention the interviews of women carried out by Nils Söderström, a head physician in a clinic situation at the Sahlgrenska Hospital at the beginning of the 1940's. On the whole Söderström reached the marriage cohorts that we later tried to interview.

His enquiry concerned if family limitation was voluntary or not, and if it took place in connection with examinations of previous gynaecological complaints. He also tried to discern other circumstances around family limitation to try to clarify its causes. Söderström's working hypothesis was that one must distinguish the 'historical' fall in fertility, which was 'a social mobility phenomenon, a violent change which must have been forced on by strong powers which were active in society during this period of time'. He names 'urbanisation, increasing information, the class struggle', etc. The importance of these factors relatively can be discussed, 'but altogether they constitute a constellation of causes which explains why the previously high birth rate dropped and was replaced with its opposite'.

According to Söderström it is not sure that the causes behind this historical development were effective at the beginning of the 1940's. 'Individuals of today have little feeling for this history, they consider their attitude towards the question as exclusively determined by the very current realities of the day'. As an example Söderström took a family with 7 children at the beginning of the falling fertility curve and considered it completely natural that one 'with the disadvantages of a 7-children system before one's eyes' easily in the next generation could go over to what he called the 'one-child system'.

The important question at the beginning of the 1940's was, however, what it was that maintained the new system of having few children. In
these very rapidly performed interviews (with women only) Söderström thus tried to get to grips with a new complicated constellation of causes.

What Söderström could discern in his conversations with several hundred women was that, in the majority of families, there was a basic value judgment which he chose to call the '2-children axiom'. They simply considered it obvious that in a family there should be two children, neither more nor less.

'There was never any need to become conscious of more distinct causes such as economy, personal convenience etc. as a decisive motive, and as there was no conflict with custom they never made themselves felt in the discussion'.

The stability of this well-established viewpoint was, according to the author, considerable. Only in a minor number of interviews could a consciousness of other reasons be noticed and these were mainly economic. The periods of unemployment at the beginning of the 1930's seem, above all, to have had great importance for the low birth rate within the cohorts of women interviewed. In a few cases more personal reasons, such as illness and previous difficult deliveries were brought forward. According to the author, however, the fear of becoming tied down to a baby was not a central motive, even if this reason did appear at times. Housing conditions seem to have been considered an important restriction surprisingly rarely.

Despite the fact that it is of a rather impressionistic character, Nils Söderström's study is, to a large extent, relevant to our investigation. It is natural to take a position on Söderström's interpretation of the whole process as well as to the particular reasons behind the system of having few children.

Why the parish of Rätan?

There were several reasons for choosing the parish of Rätan in southern Jämtland for our study. In actual fact any rural parish in the interior of Norrland without large densely-populated areas would have been suitable. This matter is easy to verify by observing the change in birth figures at the beginning of the 20th century with the aim of localising the great decline in fertility. The fact that Rätan appeared particularly interesting is connected with the fact that the area has been the object of intensive genealogical studies, in combination with research into local history.

Axel Grinde, who was himself born in the parish of Rätan and grew up there, has for many years carried out systematic studies with special atten-
tion paid to the people of his home district. One can quite easily work with Grinde's elaborate family trees, and in that way quickly obtain a great deal of demographic information, which it would otherwise take months to gather and process.

This is, in actual fact, precisely what I have done, in collaboration with Lars Rumar at the Regional Archive in Östersund. Thereby we have been able to draw a rough sketch of family size and family structure in Rätan since the 1720's (among other things the low infant mortality rate proved interesting).

The 'modernisation' of behaviour began, according to unanimous testimony, earlier in larger densely populated areas and towns than in purely rural districts. In the former one can no longer reach the inhabitants who lived through the great decline in fertility. They died long ago and are forever out of the reach of our curiosity.

Despite the fact that Rätan (see map) has some villages resembling densely-populated areas, especially the Church village of Rätansbyn, they

Map 1. The parish of Rätan in the province of Jämtland
have been altogether too small to lose their basically rural character. There has been no question of any great differentiation of the population, which is typical of an urban environment. Only a handful of people belonged to the middle class stratum for example.

Neither has there been any question of a clearly delimited labour stratum appearing in the area. The nearest Rätan came to this was in connection with the late extensions of some power stations at Handsjöfallet and Överturingen, but those who took part in the construction work hardly lost their footing in the agricultural society there. They kept their small homesteads, crofter's holdings or other homes exactly the same as before and thereby presumably the attitudes and values which belonged to that environment. Moreover, these extensions came far too late to influence family raising in the cohorts we have examined.

The growth of the welfare sector in connection with the growing investment in social and health services has been of importance to our age cohorts, and not least for the choice of occupation, and opportunities of coming out onto the labour market of the children of the people interviewed. This was of particular importance for the women. The significance of this has been that many of the tasks which were performed within the family, or with the cooperation of relatives and neighbours, were now taken over by society. In connection with this a certain centralisation of municipal services also took place. Rätan is now a part of the extended municipality of Berg, with its centre in Svenstavik.

There is, then, reason to emphasize the fact that Rätan must be a rather typical district in the interior of Norrland, with lingering high fertility at the beginning of the 20th century and an insignificant degree of industrialisation, however, with indirect contacts with the expansion of the timber industry in the coastal zone. We are dealing with a forest district which was transformed from an area of immigration to one of out-migration, a development which has been described by Mats Rolén in his penetrating thesis Forest District in Transformation which deals with an adjacent district in Jämtland with a very similar economic structure and similar problems typical for this thinly-populated area.

Rolén's research shows that many parishes up here could be interchangeable as study objects from our point of view. One should, however, be careful about exaggerating the isolation and 'backwardness' of the district. One of the many indicators of contacts with the outside world and possible impulse channels is constituted by the not inconsiderable trans-oceanic emigration to the U.S.A. and Canada. This occurred, moreover, to a large extent, at the time the people we investigated were growing up.
The so-called company expansion which made itself felt as early as the second half of the 19th century meant that many of the parents and grandparents of the people we investigated came into an often brutal contact with the exploitation impulse of modern capitalism, which affected farmers' forests in the interior. The wood was transported down to the coast by river-floating and the majority of able-bodied men participated in this work. They thereby came, for a short period each spring, to be directly employed by the companies' floating organisations. As we shall see later, most of the men participated straight from school age in the extensive felling of timber which took place in their own and adjacent parishes. For better or for worse, the large companies and the fluctuating economic situations on the export market made their mark on this remote district.

The research team

It is impossible for one researcher within a reasonable period of time to interview the rather substantial number of people necessary to form the basis of any sort of statistical analysis. This applies especially if one is forced to choose such a remote place as Råtan, which moreover consists of so many scattered villages and small units of settlements.

One must therefore find collaborators, and, taking into consideration the degree of difficulty of the work, good collaborators. It would not have been suitable to recruit interviewers among inexperienced students, as often happens in sociological investigations, for example those run by the Central Bureau of Statistics. Such interviews are usually short and it is a matter of keeping down costs, which are usually high enough anyway. A certain tact and finesse and research experience were considered to be indispensably essential. It was to be expected that the patience of the interviewer would often be tested, that some interviews would probably have to be interrupted, to perhaps be continued later, at a time more convenient for the person being interviewed, etc. We particularly expected that illness, diminished physical function e.g. poor hearing and frailty, in certain people being interviewed would be able to complicate the work considerably.

All this we discussed thoroughly at a planning seminar in Östersund in the winter of 1978/79, at which Axel Grinde participated, apart from several ethnologists with experience of interviews and knowledge of the conditions in Jämtland.
From several directions a certain scepticism or pessimism about the whole enterprise was expressed. Quite rightly the fact that the proposed interview victims had probably never experienced any sort of interview before and that they definitely could not be used to talking openly about questions regarding e.g. family planning, was pointed out. Even if these assumptions were correct, one could still expect a certain curiosity among the people to be interviewed. It could also be argued that the time-distance to the period of family raising in the lives of these people was so great that they should, in most cases, be able to see old problems and difficulties in a fairly unemotional way.

Respect for our task caused us to choose to work with a small well-prepared team of interviewers who had a good deal of experience from their own research and were well-read in the research area in question. Looking back this appears to have been the correct strategy and it was probably the most important reason that the whole enterprise was eventually able to be carried out.

The research team consisted of four people, the ethnologist Eva Sjögren, the economic historian Ulla Olsson, and the historians Ulla-Britt Lithell and Mats Rolén. I myself found it to be both natural and necessary to participate in quite a large number of interviews in order to thereby form an impression of how our questionnaire worked, how the people questioned reacted etc.

All the interviewers took part in the important work on the questionnaire, although Ulla-Britt Lithell and Eva Sjögren were responsible with me for most of the 51 questions (see supplement).

What type of interviews?

As has been mentioned, there was never any question of putting short, concentrated questions to this group of elderly people which we chose to study. On the contrary, we wanted to take our time and not intimidate them with sudden hard and indiscreet personal questions about intimate relationships. This was so much more natural in that the decline in fertility would be placed in its broader social context. It required, under all circumstances, a certain broadness in the questions, and made it possible for us to alternate delicate questions with less delicate ones.

Against this background it would perhaps have been motivated to have resorted to a completely open type of enquiry, where one could have allowed the people interviewed to talk freely about their experiences in con-
nection with the raising of their families and the time that followed. Values and conditions that we could not have anticipated would then have been able to be revealed.

We considered, however, that such a type of interview would be very difficult to process. Be it sufficient to refer to Söderström's experiences which were mentioned previously. We decided that a backbone of common questions would be constructed. Thereby, and by constant contact between the interviewers, one could have a guarantee that the enquiry on the whole was carried out in a similar and comparable way. We did still try to satisfy the need for improvisation by, among other things, choosing to perform long deep interviews. Thereby we hoped that the dimension of the unpredictable could at least be included in some of the interviews.

As far as possible we have tried to keep the interviews 2.5 - 3 hours long. To this has been added an almost compulsory coffee break, which was almost always informative in several important respects. Normally we would only have time for two interviews per day for each interviewer.

This meant that we allowed a good deal of talking 'off the record' and that we have tried to be sensible of and open to relationships that one can hardly ask about directly with any hope of success.

Such matters are, for example, if a person interviewed liked children at all, if a certain degree of equality existed within the marriage, if one looking back would like to characterise one's own marriage as successful etc. Another matter of the same kind is whether a person's general power of reflection seems to be good and even if his or her behaviour inspires confidence. Obviously we could have referred to school marks and other information to have these questions answered, but one should not underestimate the direct contact which long interviews give. The fact that there was no question of deep psychological observations is obvious, but one actually becomes quite convinced during the interviews that many clues to personality and relationships within a marriage usually do emerge. We have consistently applied this technique of, so to speak, reading between the lines and it has given rise to a partially new interpretation of the whole process studied.

The long, and partially open, interviews made it necessary to quickly note down all useful information. By useful information we here refer not only to what can be related to family and family limitation, but all information about relatives, childhood, schooling, working life, being a pensioner etc., which can be thought to be useful for coming generations of researchers. Within our research team we, in fact, hope that the material gathered by us, together with similar new projects, will be able to constitute a
new type of data bank for social scientific and humanistic research.

It also proved necessary to quickly complement and write out the interviews, because a number of the people interviewed showed hesitation and blockages in connection with too ostentatious note-taking on our part during the actual interview. In these cases we had to take very scanty notes, and immediately after the interview get down to the process of writing out and filling in, which then had to be built up around hastily noted key words and other reminders.

This reaction had been anticipated. This was exactly why we chose not to work with tape-recorders, which we believe would have had an intimidating effect on some people.

In one way this precaution was regrettable, and we refrained from using tape-recorders very reluctantly. If, in fact, we had recorded all the interviews on tape we would now have been able to hand over to language and dialect researchers and others (local radio for example) considerably more than 150 hours of southern Jämtland dialect conversation from the end of the 1970's. A detailed study of these interviews would, moreover, certainly have been able to give a great deal of elucidative information for our own use. The interviewer easily becomes a little tired towards the end of a long interview and can then miss information important per se, or maybe misunderstand what the person interviewed is trying to say.

It is, however, a known fact that tape recordings are a source which it is very difficult to work with. Large resources are necessary in order to be able to afford to write out these endlessly long interviews. The amount of time needed for the processing is another troublesome problem. All this contributed to our decision to refrain.

As mentioned we tried to find time to write out, or at least complement and elucidate the interviews the same day as they were carried out. This is an important principle. We used, also, as a rule, when a couple had been interviewed, to exchange experiences and compare interpretations of the events and situations which we then had obtained in two versions.

Practical arrangements for field studies in a rural area

The team of interviewers was kept together in connection with the field work. Interviews are, as mentioned, tiring not only for the people interviewed, even the interviewers became quite worn out after a number of interviews. At the most we therefore planned 7-8 interviews per interviewer and journey. On five different occasions we visited the area of investiga-
tion, apart from an introductory reconnaissance in connection with selecting the suitable marriage cohorts at the parish registration offices in Råtansbyn. On a later occasion the loss was also reconstructed as far as possible with the help of parish books and oral information from the vicar and his wife.

All the practical problems of transport to the investigation area and between the many villages at long distances from each other will here only be hinted at. Undoubtedly it can often be difficult to procure suitable lodgings for overnight stays and for evening work in a remote district without a hotel etc. We managed this time to have at our disposal a spacious cottage in a mountain village in Storhöga, only 20 kilometres from the parish of Råtan.

As we did not have the possibility of having a car each at our disposal, two interviewers travelled together in the area of our investigation. This required a good deal of planning, as distances between the houses of the people interviewed could be considerable and it was not always possible to arrange the interviews at the same place and same time. Also, the married couples who lived together could pose problems. It was usual that the length of the interviews differed greatly, maybe 2 hours in one case while the other took 3.5. This could cause a dislocation of the whole plan. Once or twice a person fell ill, or a person who lacked a telephone and whom we had not talked to refused to participate or had gone away or was busy with something else. The other interviewer then had to improvise, sometimes by revisiting a specially interesting person already interviewed or someone where there were still some questions to be answered or clarified.

After the day's work the interviewers gathered again and had the opportunity of comparing their observations, which from the beginning mostly concerned whether the questionnaire worked, if the length of the interviews was suitable, to what extent one could trust the information given, etc. We were often curious as to whether pensioners in their 80's would feel up to the interviews at all.

Even in the first stage of the work we could remark that our visits for the great majority seemed to be a stimulus, that they gladly sat down and tried to recreate situations, events and conditions from old times. Life in the meadow pastures, looking after the animals, not least the problem of milking, the fetching of water and fire-wood, finding a house of one's own, floating, chopping and driving in the woods, charcoal burning, conveyance and cart transport, hunting and fishing and picking berries - we have heard stories (often in rich detail) about all this, which meant so much to the district and its people. Many of the people interviewed exhibi-
ted a talent for story-telling and an ability to express themselves which was directly impressive. It must here also be pointed out that we, in con-
trast to the Nordic Museum's well-known project 'Workers' Memories', did not at all consciously select the best story-tellers or people who have the ability to sit themselves down and write a long story. Such a method of procedure would probably have given extremely poor results in the popula-
tion of our investigation.

How to approach the people interviewed?

The really difficult and delicate problem proved to be making the first contact with the people interviewed (compare the experiences of the U.L.F. Committee above). We prepared this important step by informing about our research in a couple of programmes on local radio. This happened as early as spring/winter 1979. On this occasion Axel Grinde and Lars Rumar from the Regional Archives in Östersund also participated. The latter also in connection with a lecture in Rätansbyn brought up our interview project and talked about it.

A couple of planning seminars in Östersund were mentioned earlier. In connection with these some quite long articles were published in Öster-
sundsposten and Länstidningen (local newspapers) with pictorial material which introduced the interviewers and the research we had specialised in. As the 'Jämtlanders' are a newspaper-reading people and are usually atten-
tive to the coverage of their own parish it is probable that a number of persons interviewed had found out about our project just through this information from our seminars.

A week before each interview session a circular letter was sent out to those whose turn it was for a visit. In this letter (see supplement) we emphasized above all the importance of questioning the elderly in order to gather knowledge about circumstances and events which otherwise risked becoming completely forgotten.

As our interviews would deal with so many questions other than just family relationships, we considered it motivated to express ourselves in general terms in this introductory letter, in order thereby to avoid putting off people to be interviewed in advance, as they could otherwise have re-
acted negatively or suspiciously towards an investigation entirely directed at the family planning aspect. This, at least, was how we reasoned. Per-
haps we exaggerated the need for caution. It is not easy to judge.

A negative result of the general terms of the letter was that several
people imagined that we were looking for clever story-tellers with good memories and people with a documented interest in local historical research and genealogy. We had to correct that mistake on our first telephone contact.

Even if it was only one person out of 30 who in the first session absolutely refused to receive us for an interview, in many cases a certain persuasion was necessary for us to be allowed to come. People 'were worried' about the questions as several persons said. During later sessions the number of refusals grew, but never became great enough to in any way jeopardise the whole enterprise. Altogether 66 people out of a possible 73 agreed to an interview.

Of the nine who constitute the non-response possibly a few more could have been persuaded to take part, but we only tried to persuade those people who during the telephone conversation revealed some kind of interest in our project. They often, for example, suggested other people who might be suitable spokesmen, but that they themselves had nothing to tell. Sometimes they blamed their poor memories, but at the same time said that they would otherwise be willing to take part.

A common pattern was that the wife in a family where both partners were still alive thought that it was the husband who should answer first. He represented the family outwardly, so to speak. When they realised that both partners were equally interesting for us to talk to the wives, however, agreed to take part, with a few exceptions. The fact that the women proved to be considerably more rewarding as informers, especially about family life hardly came as any surprise.

Who then were those who refused to take part? First and foremost the small non-response was constituted by women who had been widowed and now lived alone. Their general situation had apparently reinforced a certain hesitation which was found in women in general. But this should probably not be interpreted by their feeling intimidated by the interviewers in the same way as was noted in large-town areas. Despite the fact that many of the people we interviewed lived alone and rather isolated in the forest, we noticed little of the fear that would otherwise be understandable and justified considering the coverage given by mass media to attacks on pensioners etc.

Not all the people interviewed had a telephone of their own, but all had access to a telephone. Sometimes the telephone was registered in the name of a son-in-law who lived in a neighbouring house, for example, which made it more difficult for us to obtain the right number. In such cases we had to pay a direct home visit (but even these people had been reached
by the circular letter first). We interviewers felt this was a little unpleasant, even if the risk of being unwelcome proved not to be great. We did not want to seem like door-to-door salesmen and there was, after all, the possibility that we came inconveniently.

This happened several times: among others when we turned up an hour or so after the slaughter of a pig which had taken place in one of the small peripheral villages. But even in this case we were shown great hospitality and they took their time in answering our questions, although we had, of course, offered to come back at a later time.

A general observation which can be worth passing on is that the summer months could be less convenient as a period for interviewing, especially for the women. They often had children, grandchildren and great grandchildren paying a visit or staying for the summer. There was obviously a great deal for the old people to do, and now they moreover had us interviewers on their backs too. The interview situation itself could, then, sometimes become a little messy with a couple of little children, who great grandma had to keep an eye on, playing in the kitchen, and with an adult son or daughter who was a little inquisitive about secrets (from their own childhood) that were being mentioned in the interviews. All this could sometimes have an inhibitive effect on the person interviewed, but as a rule we managed to be alone with the old folks during the interview.

A large number of the interviews were carried out in October-November, which also proved to be a more advantageous period than the summer. During the autumn we were obviously felt by many to be a welcome break in daily routine and the conversation to be stimulating. To be able to talk about old times (and for that part of opinions about the society today and tomorrow) with a person who was all ears was quite an ideal situation. For many people we quite clearly drew out a latent pleasure of retelling about the past, and they have often offered to help us again if we should want to complement the investigation in any respect. We almost always felt welcome back again.

The people interviewed and their status

At the planning stage of the investigation we were, as has been implied, very unsure of whether people in their 80's would function particularly well in the interview situation. Would they feel up to the interviews if they were as long as planned? Would not many of their memories be poor and fragmentary? Should one keep to the questions or, with the best inten-
tions, sabotage the interview by talking about other things which might occur to the person interviewed at the time. Would they become very embarrassed about some of the questions? Would these questions conjure up unpleasant and difficult memories in some cases? These were troublesome question marks.

Our general uncertainty caused us not to include the 1920–24 marriage cohort. Within this many people would have been well in their 80's. It is possible that we made a little too pessimistic a judgement here. The interviews would in fact show that a 75–80 year-old in Sweden in the 1970's was by no means particularly 'aged'. The fact that memories could, however, sometimes be deceptive or shaky in many details, or that it was not seldom troublesome to give the exact date or correct chronology when the events were maybe 50 or 60 years back in the past, was not surprising. But it must here be emphatically underlined that our questions and our interest did not at all touch on questions of the type what exactly happened at the time of All Saints in 1912 in some committee meeting in Parliament, (Sw. Riksdag) or what such and such a minister said hour by hour at some transit crisis during the second World War. It has been completely different and considerably more stable information we have been looking for, e.g. how many children were born in a family, if the mothers used to breast feed and for approximately how long, how a couple lived during the first part of the marriage, etc. etc. If a retrospective interview investigation is to be successful the questions must be of this stable sort and, moreover, it is important that one has some kind of source material that can serve as a control of the informers. We shall soon be returning to this point. In our interview work we have mostly met bright, interested and open people who, moreover, seem to be well-integrated into society. Several of the people who have been over 80 years of age have been among our informants. Naturally there have been in some cases (see below concerning loss) people who have had serious arteriosclerosis and/or reduced health and fitness due to illness, serious operations etc. But this does not alter the favourable impression which I would like to characterise as one of the principal results of this study directed at method as well as results.

The strong impression that all the interviewers have had of a group of people who are well-integrated into society should be combined with a previous statement that the district of Råtan is by no means a strictly isolated area, even if it might look that way when studying a map. We shall soon find out what close contacts the people interviewed had and have with the outside world. We can add to this that several of our families have started taking in tourists after having equipped and renovated
old wooden storehouses and cottages or built new ones for overnight stays. This has probably not meant very much financially but it has caused an appreciated contact with people who often come from far away and occasionally from abroad. The tourists sometimes come back again summer after summer, and this has meant that they have become acquainted with each other and often become good friends.

Advance knowledge of the people interviewed

The preparation of the interviews is very important. This has already emerged from what has been said previously. One part of this preparation was the completion of family cards (see above) for each family with exact data concerning the date of marriage, birth places and dates of birth of the partners, the number of children, age intervals between them, moves, divorces etc. All this can, as we know, be extracted from parish books. Occupational information from different stages of the family cycle can also be found in the parish books.

It has been valuable to have access to all this information before the interview situation. The interviewers have simply been able to 'learn about' each family before the interview. Afterwards the family cards have been of help in trying to judge a person's ability to remember and general credibility. They can indeed be useful in interpreting the results and the source criticism which it is necessary to perform.

In this way we have also been able to avoid initiating interviews with childless families or those where there are adopted children. We have furthermore become forewarned about cases of pre-marital or illegitimate children, which could have made the interviews sensitive.

Interviews with people outside the investigation cohort

In the original plan it was included that a number of interviews would be carried out with people outside the circles of the people interviewed. Mainly we would look for well-informed people and those who have had positions of trust in the parishes and, not least, those who participated in child and health welfare, such as midwives, district nurses and provincial physicians.

We have not yet quite had the time to carry out this programme, but we have received a good deal of information from Axel Grinde, who has
been mentioned above. The latter, however, has not lived in the parish since his youth and therefore we needed more current information about the conditions in the parish. An ex. council representative Nils Sundquist could step in here together with the vicar and his wife, who were all very familiar with the local people.

We have furthermore had the opportunity of meeting some representatives of the part of our age cohorts who never married at all. We were allowed to go and talk to two bachelors or 'old-timers' who live in one of the more remote villages in the parish. Their standards of living and comfort could in no way be compared to those of the other people interviewed, who will be portrayed in the following. It is very probable that this observation can be generalised to apply at least to single bachelors. People without children will naturally as a rule live in a thinner web of social contacts. This will be apparent, if not before, then at least during their old age.

Academics out among 'ordinary people'

The Nordic Demographic Society held a conference in Hurdalssjöen north of Oslo recently. One of the points for discussion referred to fertility studies based on interviews. Several Norwegian researchers called attention to how problematic contacts between academics and average people for interview can be. They believed that terms of reference and values, use of language and vocabulary etc. can differ so much between the two groups that communication could become a serious obstacle. It is obvious that the choice of a rural investigation area may be thought to make communication even more problematic.

We have been aware of this problem from the very start, but none of us interviewers has found it directly troublesome. Perhaps this could be due to the fact that nobody in the research team emanated from an upper class milieu, that we simply have parents and uncles and aunts etc. who strongly reminded us of the people questioned in their way of life and outlook on life. We quite simply felt at home in the social environment, even if it did, of course, have its special characteristics too.

This problem should, however, not be underestimated. Differences in appearances between questioner and questioned can, perhaps, in certain situations make a study difficult or even impossible. There must be many examples of this.

One does come up against one particular complication when on locating
interviews in a remote rural area. One has to take into consideration that particularly older people speak a genuine dialect. It turned out that the southern Jämtland dialect we found, sometimes mixed up with Härjedal dialect, was comprehensible even for 'southerners'. When special words or idioms cropped up we simply had to ask the person interviewed to explain them. They did this often with an amused smile.

We did have the advantage of having Mats Rolén in the research team. He was born and brought up in Revsund, just an hour's car journey from the investigation area, and could sometimes serve as 'interpreter' for the rest of us, when were at any time confronted with troublesome language problems. Rolén could also certify that most of the people interviewed 'put it on' a little bit and spoke more with accepted pronunciation than usual to help us on our way. It is important to point out that we also made a great many observations apart from the actual verbal ones. It was included in our research plans that the pensioners' present situation in a rural area (also) particularly would be focused on. It was easy for us to receive an impression of the housing conditions and social contacts just by looking around and acting as a 'candid camera'. We asked directly about other aspects e.g. the municipal service for the elderly. As there did not seem to be very many studies of this kind, our observations could be of some value.

The delicate questions

In advance we were most nervous that the battery of questions concerning family planning would be partly troublesome to ask at a given time, and partly embarrassing for the people interviewed and decrease their willingness to cooperate with us. It probably does not hurt to underline the fact that a situation such as this can become embarrassing both for the person asking and for the person answering the questions.

Soon after the first few interviews it was clear that our misgivings were exaggerated. Even if there was a lot of hush-hush about sexual questions in the 1920's and 1930's, as several of the people interviewed put it, they had apparently kept up with the general development towards greater openness and we received, in the great majority of cases, frank answers. There were few people who completely refused to talk about these matters with us. In some cases the initial resistance was quite considerable but they still answered the questions, in some cases with reservations. When both partners in a marriage were questioned at the same time, the men,
for example, could be quite hesitant and reticent, while the women talked quite undauntedly. Thereby information could be secured for the observation unit in question, which was, of course, the family.

It is no after-sight to say that we had expected that at least some of the people interviewed would be able to look at the period of family raising with objectivity and quite unpassionately. This we suspected would be able to favour our investigation. The fact that this would be the rule was still a positive surprise. But even if the distance in time in all cases was very great, this was not able to prevent us arousing unpleasant memories sometimes, which brought back tensions, unsympathetic actions, illness and death. In these cases the interviewer had to be alert and quickly retreat. Except, of course, in cases when we felt it could be positive for the person interviewed to be able to talk.

The question of integrity in local historical research

Now we have come close to an important problem area in all research directed at the present time. We are presented with difficulties on two levels. To be able to analyse and understand certain social processes one must go down to the level of the individual, with the risks and complications this implies. Much of the knowledge then gained can be transformed to recognisability into tables, diagram, figures and mathematical models which only the researchers themselves and a small elite group outside the research society can appreciate.

If, however, one wants to reach out with one's results, one is forced to exemplify and be concrete, and then there is the risk that one reveals what has been told in confidence about other peoples' private lives and other doings and sayings. If, moreover, one writes legibly the risk is even greater of the text reaching outside the academic reading public and ending up in front of the eyes of people who possibly could misuse the information. The risk of this happening is especially great in research which has a local character. It can often be easy to identify the people concerned, even if the writer does his best to conceal who has given him the information. It can be added that research of the type we have carried out concerning family life can be extra sensitive.

This problem has been discussed within Norwegian research circles around the publication Heimen, but as far as I know it has not yet engaged the interest of Swedish scholars, despite the fact that we too have quite extensive local historical research. It is, of course, of the utmost
importance that one checks the accounts of results which are reported from
the aspect of integrity and, in doubtful cases, asks the people interviewed
themselves if one may publish a certain passage. It must indeed be impor-
tant to discover what can be considered as sensitive and what less sensi-
tive in a special social context.

ACCOUNT OF RESULTS 1

The population investigated

It has already been mentioned how the population investigated was selec-
ted. The very small non-response has also been commented on. In the
following, certain information relevant to this study will be given on a
sample from a local population which we have been working with, and also
what happened to those who we have not been able to reach. A large num-
ber of calculations has been able to be performed with the help of infor-

mation which has concerned the loss. We have thereby been able to expand
the statistical basis, which always in this type of intensive studies tends
to become fragile.

The target group which our study has been concerned with firstly grew
over time, as the yearly cohorts of married couples have been added to
the material (compare figure 2). The number of marriages has a connection
to demographic conditions, but is also socially and economically condi-
tioned. A certain connection with the business cycle can usually be shown.

It is clear that people were not at all limited to their own parish in
their choice of marriage partner, even if, as is natural, many couples did
come from Råtan. This can be seen from the birth field (see figure 3). Not
only neighbouring villages appear here, but also places at great distances
from Råtan.

Even during the build-up stage, such an investigation cohort as this
goes to be reduced. The causes for this are mortality and migration. In
these days mortality plays a minor part during the woman's fertile period; the
great majority of deaths occur after the age of 50. Later, however, its
importance increases at an accelerating speed. As is shown in the diagram
(figure 2), it would have been suitable to perform our interviews before
1960. Then the loss due to death would have been minimal. On the other
hand, if we had waited another 5 - 10 years our type of retrospective
study of family planning would have become completely impossible to carry
Figure 2. The process of growth and loss in the marriage cohort 1925-35. Reported annually. (Compare table 1).
Table 1. The process of growth and loss in the marriage cohorts 1925-1935. Reported annually (compare figure 2).

<table>
<thead>
<tr>
<th>Year</th>
<th>Men Married 1925-30 Rem</th>
<th>Men Married 1931-35 Rem</th>
<th>Women Married 1925-30 Dead</th>
<th>Women Married 1931-35 Dead</th>
</tr>
</thead>
<tbody>
<tr>
<td>1925</td>
<td>5</td>
<td></td>
<td>5</td>
<td></td>
</tr>
<tr>
<td>1926</td>
<td>13</td>
<td>13</td>
<td></td>
<td></td>
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<tr>
<td>1927</td>
<td>16</td>
<td>23</td>
<td></td>
<td></td>
</tr>
<tr>
<td>1928</td>
<td>11</td>
<td>32</td>
<td>1</td>
<td></td>
</tr>
<tr>
<td>1929</td>
<td>16</td>
<td>43</td>
<td>6</td>
<td></td>
</tr>
<tr>
<td>1930</td>
<td>18</td>
<td>52</td>
<td>8</td>
<td></td>
</tr>
<tr>
<td>1931</td>
<td>15</td>
<td>28</td>
<td>11</td>
<td></td>
</tr>
<tr>
<td>1932</td>
<td>15</td>
<td>40</td>
<td>12</td>
<td>1</td>
</tr>
<tr>
<td>1933</td>
<td>15</td>
<td>50</td>
<td>12</td>
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<td>1934</td>
<td>15</td>
<td>60</td>
<td>12</td>
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<tr>
<td>1935</td>
<td>15</td>
<td>70</td>
<td>12</td>
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<td>1936</td>
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<td>15</td>
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<tr>
<td>1940</td>
<td>15</td>
<td>120</td>
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<td>1941</td>
<td>15</td>
<td>130</td>
<td>12</td>
<td></td>
</tr>
<tr>
<td>1942</td>
<td>15</td>
<td>140</td>
<td>12</td>
<td></td>
</tr>
</tbody>
</table>

Note: The table continues with the same structure for each subsequent year up to 1950.
Figure 3. Areas of birthplaces and areas of migration for the marriage cohorts 1925-35.

Area of birthplace

Ratan
67 men and 76 women were born in the parish of Ratan

- 1 Man
- 1 Woman
Area of migration

1 Family
Table 2. The structure of interviewed and non-interviewed persons.

<table>
<thead>
<tr>
<th>Total number</th>
<th>284 persons</th>
<th>142 couples</th>
</tr>
</thead>
<tbody>
<tr>
<td>of which:</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Not registered in the parish</td>
<td>-12</td>
<td>- 6</td>
</tr>
<tr>
<td>Migrated within one month after marriage</td>
<td>-28</td>
<td>-14</td>
</tr>
<tr>
<td>Woman 45 at marriage</td>
<td>-10</td>
<td>- 5</td>
</tr>
<tr>
<td>Childless marriages</td>
<td>-18</td>
<td>- 9</td>
</tr>
<tr>
<td></td>
<td>216</td>
<td>108</td>
</tr>
<tr>
<td>Of which:</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Both migrated</td>
<td>-62</td>
<td>-31</td>
</tr>
<tr>
<td>Both dead</td>
<td>-26</td>
<td>-13</td>
</tr>
<tr>
<td>1 dead 1 migrated</td>
<td>- 6</td>
<td>- 3</td>
</tr>
<tr>
<td>Divorced</td>
<td>-16</td>
<td>- 8</td>
</tr>
<tr>
<td></td>
<td>106</td>
<td>53</td>
</tr>
<tr>
<td>Possible units for interview of which:</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Both interviewed</td>
<td>42</td>
<td>21</td>
</tr>
<tr>
<td>Man interviewed, woman dead</td>
<td>6</td>
<td></td>
</tr>
<tr>
<td></td>
<td>1 &gt; 8</td>
<td></td>
</tr>
<tr>
<td>Woman interviewed, man dead</td>
<td>15</td>
<td>&gt; 66 interviewed</td>
</tr>
<tr>
<td></td>
<td>1 &gt; 16</td>
<td></td>
</tr>
<tr>
<td></td>
<td>-</td>
<td></td>
</tr>
<tr>
<td>Woman interviewed, woman dead</td>
<td>3</td>
<td>&gt; 20</td>
</tr>
<tr>
<td></td>
<td>-</td>
<td></td>
</tr>
<tr>
<td>Woman dead, woman interviewed</td>
<td>6</td>
<td>&gt; 26 dead</td>
</tr>
<tr>
<td></td>
<td>-</td>
<td></td>
</tr>
<tr>
<td>Man in disposed, woman interviewed</td>
<td>1</td>
<td></td>
</tr>
<tr>
<td></td>
<td>-</td>
<td></td>
</tr>
<tr>
<td>Man refuses, woman interviewed</td>
<td>-</td>
<td></td>
</tr>
<tr>
<td></td>
<td>-</td>
<td></td>
</tr>
<tr>
<td>Woman in disposed, man interviewed</td>
<td>1</td>
<td>&gt; 7 in disposed</td>
</tr>
<tr>
<td></td>
<td>3 &gt; 4</td>
<td></td>
</tr>
<tr>
<td>Man refuses, man interviewed</td>
<td>-</td>
<td></td>
</tr>
<tr>
<td></td>
<td>-</td>
<td></td>
</tr>
<tr>
<td>Woman refuses, man interviewed</td>
<td>1</td>
<td></td>
</tr>
<tr>
<td></td>
<td>-</td>
<td></td>
</tr>
<tr>
<td>Woman refuses, woman interviewed</td>
<td>2</td>
<td>&gt; 7 refused</td>
</tr>
<tr>
<td></td>
<td>2 &gt; 6</td>
<td></td>
</tr>
<tr>
<td></td>
<td>1</td>
<td></td>
</tr>
<tr>
<td>Of 73 possible for interview 66 were interviewed = 90.4%</td>
<td></td>
<td></td>
</tr>
<tr>
<td>&quot; &quot; &quot; &quot; &quot; &quot; 7 refused = 9.6%</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>
out! That is how quickly the loss for each year is increasing now.

As regards migration, on the other hand, this took place at quite an early stage of the marriage, while the couple were young and had often not had time to have children. By the time the children had reached school age the inclination to move normally decreased. In order to reduce the loss of migrants, we would have had to follow the raising of their families in other places and carry out interviews there. Even if we had started the research work a couple of decades sooner it would not have sufficed. The destination areas are specified in figure 3. As can be seen, a great deal of the migration was to distant places both in Jämtland and outside this country.

The process of growth and loss in the marriage cohorts 1925-35 (11 years) has been accounted for annually up until and including 1979 in Table 1. The number of divorces has also been noted there. The number of broken marriages was not great and the loss which hereby arose has had little significance.

A special survey has been carried out of all married couples in Rätan 1925-35 and how this cohort was reduced, even due to other causes than migration and mortality (Table 2). It appears that a great number of people actually had no strong ties to the parish, but only happened to be staying there temporarily or had just chosen to marry there.

The cases where both members of the family have been interviewed, and several other combinations, have also been noted in the table. It appears that the number of indisposed (for health reasons) is as large (7) as the number of refusals. Of those who could at all be considered as the object of an interview, just over 90% have cooperated. If one considers the number of families and counts them as the real observation unit then 45 out of 53 have been able to be persuaded to participate.

How representative is the population investigated?

It must be kept in mind that the aim of the Rätan Study has been to isolate a population which experienced the great decline in fertility. Preferably this should have taken place between two generations. Even if those who migrated could be interesting to follow up, one can claim that they never really belonged to the population investigated. Another precondition was actually that the marriages have had time to be completed i.e. the women have become 45 years old while we have been able to observe them.
(Thus we here refer not to the legal term 'consumated marriage' but to the
Table 3. Marital fertility, number of children, as born by age groups.

<table>
<thead>
<tr>
<th>Marital fertility</th>
<th>No. of child.</th>
<th>Woman's age at marriage</th>
<th>Marriage with children</th>
<th>Year Difference between 1st-2nd child</th>
<th>2nd-3rd child</th>
</tr>
</thead>
<tbody>
<tr>
<td>Interviewed families</td>
<td>438.3</td>
<td>226.7</td>
<td>126.8</td>
<td>90.4</td>
<td>67.9</td>
</tr>
<tr>
<td>N=45</td>
<td>1</td>
<td>19</td>
<td>40</td>
<td>45</td>
<td>45</td>
</tr>
<tr>
<td>Not interviewed but followed until the women is 45 years old</td>
<td>479.6</td>
<td>386.5</td>
<td>200.7</td>
<td>150.0</td>
<td>41.7</td>
</tr>
<tr>
<td>N=29</td>
<td>0</td>
<td>4</td>
<td>22</td>
<td>28</td>
<td>29</td>
</tr>
<tr>
<td>Migrated before woman is 45 years old</td>
<td>360.8</td>
<td>276.6</td>
<td>307.1</td>
<td>125.5</td>
<td></td>
</tr>
<tr>
<td>N=25</td>
<td>1</td>
<td>8</td>
<td>11</td>
<td>9</td>
<td>6</td>
</tr>
</tbody>
</table>
English term 'completed marriage').

The choice of investigation area was the strategic problem. The people who have had the period of the raising of their families located to Råtan are those who are interesting for us. Among them many have already died. Would this group have been composed differently or have had a different pattern of behaviour and values than those we have been able to interview?

This question is not so easy to answer. What one can clarify (Table 3) is that the number of children in the marriage (2.6) is exactly the same for both the groups. Otherwise the mortality group had higher age specific fertility, most tangibly for the age range 25-29 but marked also for 30-34 and 35-39. The average age of the women at marriage (28.9) was high, and considerably above the group we have been able to question. (26.0)

They also had a somewhat higher proportion of pre-marital children and often longer periods of cohabitation before marriage (compare Figure 4a and 4b). The number of children who were conceived before marriage, on the other hand, lay at the same high level (2/3 of all marriages started in this way) for both groups. The age difference between the first and the second child was also considerably shorter within the mortality group (3.2 as compared to 4.0 years) and the same applies to the difference between the second and third child (3.0 as compared to 4.4).

It seems probable that the group which we have called the mortality group reflects a somewhat earlier phase in the decline in fertility in the parish of Råtan, due to the fact that it consisted of generally older people. But the difference from the investigation population should not be exaggerated. As we shall soon see it is in relationship to the previous generation that the figure of 2.6 children in a marriage should be seen. This is when the really drastic change occurs.

If this suggested interpretation is correct, a questioning of the 'loss' people we have discussed would probably have led to the fact that we would have found a somewhat smaller proportion of consistent family planners with the two-children axiom as their guide mark. (As regards the migrants, one can perhaps venture to assert that they at least do not seem to have been fanatic family planners. The number of observations is, however, too small here).

One characteristic of the marriage pattern which might appear a little peculiar is the very large proportion (see. Fig. 4) of children born before marriage (but with the same parents). The proportion of pre-marital conceptions is also unusually high. This, however, is typical of the large northern inland regions of which Råtan is a part.
Figure 4. Pre-marital children and pre-marital conceptions.
   a. people interviewed
   b. not interviewed but followed up until the woman is 45 years old

\[ N = 45 \]

Pre-marital children or children born before 9 months after marriage 31/45 = 69\%
Pre-marital children or children born before 9 months after marriage. 19/29 = 66%.
Thus one obtains a sort of early cohabitation relationship long before the Swedish expression 'sambo' ('cohabitant') was minted. The closest equivalent to this would be the so-called 'Stockholm marriages' which were an established term as early as the second half of the 19th century. (Even the percentage of illegitimate children was rather high). Against this background it is a little strange to hear several of the people interviewed complain bitterly about the system of cohabitation which has developed over recent years, but this is surely quite simply proof that our memories are sometimes very short.

With this type of family structure, the actual occasion of the marriage is not so decisive and important. This applies especially to cases where the dean quite simply put on pressure so that people would get married. Thus, in actual fact, family raising started in many cases before 1925, which otherwise is the starting year of this study. Some couples with children who did not go to the vicar until after 1935 should really have been included in our examination cohorts. This can hardly have had any great influence on the result however.

ACCOUNT OF RESULTS 2

The introductory question which we asked was if one, with the help of interviews, can obtain a more detailed picture of the complicated process which the historical decline in fertility constitutes. Particularly we were then going to compare what one can extract from the per se excellent family cards and the information which the interviews would possibly be able to render.

In the following we will examine some of the explanation variables that have been discussed within the extensive scientific literature which deals with the development towards the so-called zero growth in European, North American and other populations. At least as much attention internationally is paid, of course, to the conditions and tendencies in the present or in the very near future. Research interest is often focused on young women at the start of their fertile period and their attitude towards having children (often at a certain parity) related to economic and social variables as well as education, occupational activity and sex roles. Söderström's suspicions from the beginning of the 1940's that there are two relevant but basically quite different family planning situations becomes even more plau-
sible. With the very efficient methods of contraception of the 1960's and 1970's it is no longer a question of keeping down the number of children, but of actively deciding to have children. It here becomes particularly important to study the basic attitude towards the child on the part of the (future) parents - and to alter it if one wants to conduct a pronatalistic policy. This aspect has been dealt with, above all, by the psychologists Hoffman and Hoffman within international research.

In the historical context there is, however, also cause to ask the question of how the attitude towards children as an asset or a burden has been developed e.g. from the end of the 19th century and up until the mid-war period. There are many aspects to ponder over here, other than the economic benefits, which usually steal all the interest when one is dealing with the relationship parents and children (often in a time-perspective with the parents' security in old age as an important factor).

Economic development and economic differences as explanation variables

It will be necessary to recapitulate briefly on some of the major characteristics of economic research concerning family planning and fertility. One of these has been to try to specify the role of urbanisation and industrialisation in the development of a new family pattern. It has been considered that the named radical social processes actively contributed towards the breakdown and disappearance of a more extended family system where the conception of extended families as a norm is in the background. Le Play shaped these ideas in detail as early as the 19th century and they have later been developed by Goode among others. The latter asserted that it was industrialisation that created and upheld the 'small, independant and mobile family', the nuclear family. Birth control is also considered to have been favoured by industrialisation.

Historical research has shown that these explanations from economists and sociologists are either grossly wrong or at least greatly simplified and partly misleading as regards family structure and the growing increase in migration. Much indicates that the connection with birth control does not stand up to closer inspection either.

Another economic explanation complex is the so-called need theory. This is mainly connected to Maslow and involves an analysis of which basic needs people in general have. These needs are thought to be regulated hierarchically so that physiological needs are primary and must be satisfied first. This, is, of course, a reasonable thought. On the next level then comes the need of safety and security followed by contact and solidarity
and appreciation from the environment and lastly the rather more luxurious need of self-fulfillment. It is considered that simplified the development leads from phase to phase in step with the modernisation of society. In industrial and service states the need for children - or at least several children - automatically becomes greatly limited. The security aspect and other basic needs are taken care of by the public and welfare sectors.

There is also a school that asserts that fertility constitutes a function of social class. In order to be able to keep up a certain socio-economic standard or possibly be able to manage to move up into a higher layer, sacrifices are demanded, which require a planning mentality incompatible with a large family. Family planning thus becomes a link in an upward social mobility.

In this way a heterogeneous family pattern is developed on different levels of society. Here the theory of innovation-spreading helps us to predict how e.g. the pattern of 2-children families spreads from the more well-situated to lower economic and social groups, with a certain time lapse due to the fact that not all take part in the profits of societies in economic growth.

There is, finally, a group of economists who work with a consumption approach in Becker’s wake. According to the consumption theory there is a certain supply of goods and services and the individual chooses the combination which gives the greatest satisfaction considering the limited resources which are at his disposal. The decisive factors for the ‘consumer’ are price, taste and resources. The fact that within this genre a child undauntedly is weighed up against a refrigerator etc. might shock a humanist, but it should be added that this theoretical model can be applied in quite a sophisticated way. This applies e.g. to the influential American economist R. Easterlin who works with the idea of the objective level of consumption in a larger labour market context taking into consideration household structure and demographic mechanisms among others the changes in conditions and aspirations between two consecutive generations - all this effects on fertility.

Easterlin’s basic precondition is that the economy is, in the long run, expanding. Can this be considered correct in our investigation area, and can, moreover, his basic ideas and those of other economists be applied to the decline in fertility in Rätan? The position on this question seems rather doubtful, even if economic conditions in general are admittedly decisive for the chances in life of both the individual and the family.

There are, in fact, no indications that the population of Rätan during the period 1900 - 1940 actually experienced a gradual improvement in eco-
nomic conditions. On the contrary it can be said that a period of expan-
sion, naturally in the shadow of the saw-mill industry just before the be-
ginning of the 20th century, turned into a long period of decline with
violent fluctuations in the economic situation. The preconditions which the
economic theory has worked with, almost without exception, have therefore
not applied. Still we have a steep decline in fertility as will soon be shown.
The explanation for this must obviously be looked for on other levels.
Possibly one can allow the economic variables a chance by posing the
hypothesis that the small stratum of farmers in the parish was first to
start with consistent family planning? As forest-owners and with larger (if
not particularly impressive) farms than the crofters and considerably better
situated than workers-lumberjacks, this group should have had special
motives for keeping the number of children down also for aiming their
ambitions at social goals, e.g. for their children.
Table 4 shows that this hypothesis does not gain any support in our
material. On the contrary, one finds that the farmers’ families had some-
what above the average number of children, and in not a few cases had
very large numbers of children by the new standards which started to
apply in the 1920’s.

Table 4. Number of children within different social strata

<table>
<thead>
<tr>
<th></th>
<th>Farmers</th>
<th>Crofters</th>
<th>Forest workers</th>
<th>Others</th>
</tr>
</thead>
<tbody>
<tr>
<td>Interviewed 4.0 Ch./fam.</td>
<td>3.6 Ch./fam.</td>
<td>2.8 Ch./fam.</td>
<td>1.3 Ch./fam.</td>
<td></td>
</tr>
<tr>
<td>N = 45</td>
<td>11</td>
<td>7</td>
<td>24</td>
<td>3</td>
</tr>
<tr>
<td>Not interviewed but followed up until the woman 3.6 Ch./fam.</td>
<td>3.4 Ch./fam.</td>
<td>3.1 Ch./fam.</td>
<td></td>
<td></td>
</tr>
<tr>
<td>N = 28</td>
<td>11</td>
<td>5</td>
<td>12</td>
<td></td>
</tr>
<tr>
<td>Total</td>
<td>3.8 Ch./fam.</td>
<td>3.5 Ch./fam.</td>
<td>2.8 Ch./fam.</td>
<td>1.3 Ch./fam.</td>
</tr>
<tr>
<td>N = 73</td>
<td>22</td>
<td>12</td>
<td>36</td>
<td>3</td>
</tr>
</tbody>
</table>

Ch./fam. = Children/family

In some of the interviews this is explained by the fact that people con-
sidered that they could afford many children. This emerges from the fact
that people in this group also took attempts to plan the number of children
lightly, if indeed they even tried. The economic aspect, as can be seen,
does come in, but with quite another significance than we expected. The fact that several of those who were in a really poor economic situation did tender this as a motive for limiting the number of children can, on the other hand, hardly be considered surprising: but this has apparently been more as a consequence of almost permanent under-employment, despite the fact that people were willing to take all imaginable opportunities of an extra income - than of unemployment e.g. at the beginning of the 1930's. This is hardly ever stated as a special motive, which can be compared to Söderström's results above.

When our marriage cohorts in general reached a tangibly improved economic standard (seen from a particularly low starting-point in youth) the children had already grown up and the post-war welfare society was fully established. Standards of housing, especially, had then been raised considerably. Families who, at the beginning of their marriages lived in a couple of small meagrely equipped rooms at one end of a shed, had now been able to build a house of their own, with running hot and cold water and acceptable heating during the winter. The increase in living space was also dramatic. Standards of housing and living space are never given as a reason for limiting the number of children, strangely enough (Compare Söderström above).

The reaction against 'too many' children

It was mentioned earlier that Söderström thought it natural to try to keep down the number of children if you, in your own childhood, had lived with the disadvantages of the '7-children system' in front of your eyes. In Rätan there was this 7-children system at the end of the century. The people we interviewed thus grew up in families with an average of over 7 children. In their own marriages they had an average of 3 children which in fact meant that most of the families consisted of, at the most, 2 children. They thus reached in one single generation the 2-children axiom, which means that we have been able to locate precisely the decline in fertility.

Everything indicates that the reaction against large families with all the problems of clothing and feeding the children, was an important explanation variable. It is also often stated in our interviews as important. But a cross-tabulation of the number of children in the parental home and the number of children in the families interviewed hardly gives any indication that the extra large number of children in the parental home would lead to the strictest form of birth control later. (N) here is, however, very small in each cell (see Fig. 5). This figure can, however, be expanded with some
Figure 5. Number of children in family of origin with own family.

<table>
<thead>
<tr>
<th>Number of children in the parents of the people interviewed had</th>
<th>Men</th>
<th>Parents of the people interviewed:</th>
</tr>
</thead>
<tbody>
<tr>
<td>1</td>
<td>2</td>
<td>3</td>
</tr>
<tr>
<td>---</td>
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<td>---</td>
</tr>
<tr>
<td>1</td>
<td>1</td>
<td>2</td>
</tr>
<tr>
<td>2</td>
<td>3</td>
<td>2</td>
</tr>
<tr>
<td>3</td>
<td>1</td>
<td>1</td>
</tr>
<tr>
<td>4</td>
<td>1</td>
<td>1</td>
</tr>
<tr>
<td>5</td>
<td>1</td>
<td>1</td>
</tr>
<tr>
<td>6</td>
<td>1</td>
<td>1</td>
</tr>
<tr>
<td>7</td>
<td>1</td>
<td>1</td>
</tr>
<tr>
<td>8</td>
<td>1</td>
<td>1</td>
</tr>
<tr>
<td>9</td>
<td>1</td>
<td>1</td>
</tr>
<tr>
<td>10</td>
<td>1</td>
<td>1</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Women</th>
<th>Number of children in the parents of the people interviewed had</th>
</tr>
</thead>
<tbody>
<tr>
<td>1</td>
<td>2</td>
</tr>
<tr>
<td>---</td>
<td>---</td>
</tr>
<tr>
<td>1</td>
<td>1</td>
</tr>
<tr>
<td>2</td>
<td>1</td>
</tr>
<tr>
<td>3</td>
<td>1</td>
</tr>
<tr>
<td>4</td>
<td>1</td>
</tr>
<tr>
<td>5</td>
<td>1</td>
</tr>
<tr>
<td>6</td>
<td>1</td>
</tr>
<tr>
<td>7</td>
<td>1</td>
</tr>
<tr>
<td>8</td>
<td>1</td>
</tr>
<tr>
<td>9</td>
<td>1</td>
</tr>
<tr>
<td>10</td>
<td>1</td>
</tr>
</tbody>
</table>

Parents of the people interviewed:

**Men**
- No. of children: 195
- N: 27
- 7.2 children/family

**Women**
- No. of children: 237
- N: 32
- 7.4 children/family
of the loss. The fact that in large parts of the country this decline to
2-children families took place from a considerably lower starting point (3-5
children) is, perhaps, more important as a counter-balance. In such cases
the 'reaction against too many children' can hardly have been a decisive
variable.

Table 5. Information given by people interviewed about contraceptive mea-
urses.
Married 1925-30 N=18

<table>
<thead>
<tr>
<th>Technical contraceptives</th>
<th>Coitus interruptus</th>
<th>No contraception</th>
</tr>
</thead>
<tbody>
<tr>
<td>No. No. of children group</td>
<td>No. No. of children group</td>
<td>No. No. of children group</td>
</tr>
<tr>
<td>14 3 Farmer</td>
<td>2 4 Farmer</td>
<td>1 3+5 Crofter</td>
</tr>
<tr>
<td>19 3 F.W.</td>
<td>3 3 F.W.</td>
<td>21 1 F.W.</td>
</tr>
<tr>
<td>22 1 F.W.</td>
<td>11 1+1 Farmer</td>
<td>27 1 F.W.</td>
</tr>
<tr>
<td>28 4 F.W.</td>
<td>15 7 Farmer</td>
<td>30 2+5 Farmer</td>
</tr>
<tr>
<td>35 2 F.W.</td>
<td>33 1+5 Farmer</td>
<td>37 6 F.W.</td>
</tr>
<tr>
<td>41 1 O.W.</td>
<td>34 1 O.B.</td>
<td>No. No. of children group</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

N=6 14 2.3 children/ family
N=7 24 3.4 children/ family
N=5 23 4.6 children/ family

Married 1931-35 N=27

<table>
<thead>
<tr>
<th>Technical contraceptives</th>
<th>Coitus interruptus</th>
<th>No contraception</th>
</tr>
</thead>
<tbody>
<tr>
<td>No. No. of children group</td>
<td>No. No. of children group</td>
<td>No. No. of children group</td>
</tr>
<tr>
<td>5 5 Crofter</td>
<td>6 1+2 F.W.</td>
<td>7 1+5 Farmer</td>
</tr>
<tr>
<td>10 3 Crofter</td>
<td>8 5 Farmer</td>
<td>9 1+5 F.W.</td>
</tr>
<tr>
<td>12 2+2 F.W.</td>
<td>44 2 W.</td>
<td>20 2 W.</td>
</tr>
<tr>
<td>26 1+1 O.B.</td>
<td>48 1 F.W.</td>
<td>23 1+1 F.W.</td>
</tr>
<tr>
<td>29 2 Farmer</td>
<td>54 1 Farmer</td>
<td>38 2 F.W.</td>
</tr>
<tr>
<td>31 1+1 Crofter</td>
<td>55 1+1 W.</td>
<td>43 3 W.</td>
</tr>
<tr>
<td>39 3 Crofter</td>
<td>56 1 Crofter</td>
<td>57 4 W.</td>
</tr>
<tr>
<td>45 1 F.W.</td>
<td>58 1+3 W.</td>
<td>No. No. of children group</td>
</tr>
<tr>
<td>51 1+2 Farmer</td>
<td></td>
<td></td>
</tr>
<tr>
<td>52 2 F.W.</td>
<td></td>
<td></td>
</tr>
<tr>
<td>53 1+2 Crofter</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

N=11 30 2.7 children/ family
N=8 19 2.3 children/ family
N=7 25 3.6 children/ family

W. = Worker
F.W. = Forest Worker
O.B. = Own Business
Access to contraceptives. 'They were sold like drugs'.

During the largest part of the family raising period in question the selling and spreading of contraceptives was prohibited. People had to buy them 'like drugs' as one of the people interviewed put it. What happened was that someone made a joint in Östersund when he went into town, or by postal order. This was a business that the men looked after, and it was the men who mainly seem to have discussed contraceptive techniques. Several of the people interviewed also told us about 'pedlars' who sold watches and other things. Condoms were also sometimes to be found among their wares.

It appears from the interviews that couples seldom talked together about contraceptives. The man's world was strictly separated from the woman's world, it seems. One exception was the 'egalitarian marriages' which we shall return to at a later point.

A survey of the information given by the people interviewed about contraception is given in Table 5. It appears from this that most families in the marriage cohorts (which hardly differ one from another) practised contraception. A not altogether insignificant minority denied, however, any use of such measures whatever. Older methods such as coitus interruptus still relatively meant a good deal, if we are to believe the people interviewed. Among 'technical contraceptives' the condom has meant the most, even if douches and such can be shown to have been used.

On the other hand the people interviewed deny that abortion in any form used to take place in this area. A few exceptions have been mentioned, but it has obviously been considered that these exceptions only served to prove the rule. They knew that it was possible to travel to Stockholm 'and have it arranged', but said that this traffic was almost non-existing.

What one can conclude from this is mainly as R. Adorka maintained in another context, that the will and the motives behind family limitation are the essential point, not how people practically went about it. One can note that the table lends certain support to the fact that the information of the people interviewed is, on the whole, correct. There is a clear correlation between those who stated that they used technical contraceptives quite regularly and small families. Reversed, those who maintained that they never used such methods, or even practised coitus interruptus or 'safe periods' have had larger families. The fact that our information about contraception still may be underestimated has to be reckoned with.

An important assertion is that one cannot simply equate the one-child families with consistent and successful family planning. In some cases we
found that the woman after a difficult delivery quite simply could not have any more children. The couples tried to have more children, but without success, thus the opposite of family limitation. Reversed, we have some cases where couples had had many children but still claimed to have practised family limitation, at least during certain periods, such as the end of the fertile period and before marriage. One the whole it was common for couples to have been careful when the woman was 35-50 years old.

Are we dealing with innovation spreading?

Even if one cannot grant the spreading of contraceptive devices a decisive role in family limitation, it can still be argued that an innovation process did exist. The ideas which are connected with family limitation, the whole syndrome surrounding this, have, then, been spread from certain (probably urban) centres. Is this a reasonable interpretation of what has happened?

The answer to this must be no. One can, of course, characterise the investigation area as remote (even in relationship to the central town of Östersund), but it has in no way been so isolated that ideas and propaganda would be late in reaching there.

In our interviews one receives a congruous picture of a group of people who indeed travelled little during their childhood and early youth, but who kept up with what was happening outside their own district by, among other things, a very widespread reading of newspapers. Practically all the families while growing up subscribed to at least one newspaper.

During the period just before and at the beginning of the raising of their families they have thus had active contacts with Östersund and places in the Milar Valley e.g. Stockholm, and in a number of cases even worked for a time in the U.S.A. or Canada. Such an area can hardly be described as isolated.

This impression is reinforced when one asks about the inclination to migrate within the investigation group. This was very strong, and not least North America has loomed large. The fact that most of these planned moves were not realised is due to social obligations at home or obstacles that were put in the way e.g. by dominating parents. Especially the women have apparently had difficulty in breaking loose from the social network in their home villages. Instead of taking work and maybe receiving an education in Minneapolis, Seattle or Edmonton, they stayed at home and looked after their ageing parents. Particularly if other siblings had had the chance
to travel out into the world this naturally created bitterness. Sometimes they actually tried to change their environment, tried working at an iron-mills in Bergslagen, for example, but after a time returned to the home district.

The strong family ties which were indicated made their mark on the environment during the childhood of the people interviewed. We have found a very high proportion of three-generation families in the different social groups: if one takes into consideration even shorter periods in a three-generation family or extended household, this has apparently been a norm in this area. This result will be discussed in a later research report.

What is even more remarkable and which we touched upon above is that ties to family and relatives have remained as strong as ever before, despite children and grandchildren moving away, often far outside the boundaries of Jämtland. Here 'industrialisation has not undermined the traditional family system by mobility' as has been persistently asserted. Through increased car ownership (many of the people interviewed had their own cars) and otherwise improved communications (particularly the telephone has meant a great deal) even the elderly in our investigation area have an almost continual contact with the outside world. The visits of children and grandchildren is legion particularly during the summer season. A number of children live, moreover, nearby or at least in the same province. (The inclination to move back to Jämtland even in younger generations is great). If one now contrasts these well-integrated pensioners with large external means of contact (travel, not least abroad, was common) with the picture given by mass media of the situation and isolation of the ageing in our society, the result is astounding. This must, as mentioned, be considered as an exciting research result which we will later have reason to return to in another context. Indeed, there are so many ins and outs in the interview material that we can here only touch upon.

Can the education variable have been decisive?

Within fertility research education (and information in general) has been considered a key variable. Expressed in simple terms, the connection between education and ambition, and a planning mentality which normally led to small families has been readily admitted.

To maintain that increased education (in any stratum) would have been able to basically change people's attitude towards the family and the number of children, however, is unreasonable as regards our investigation
area. Only a handful of people have received anything like a long education. Even then it has not been up to 6th form level. On the other hand several of the siblings of the people interviewed went to seminars etc. This even applies to some women who have otherwise been ill-favoured in this respect, which they, in several cases, complained bitterly about in the interviews.

We have, however, also asked about involvement in organisations and positions of trust in the municipality etc. to be able, if possible, to discern a group which could be described as particularly active and informed. It is here rather a question of linking together types of personality and behaviour. This information has not yet been worked on. Reading habits and other interests can also complement the picture here.

What do 'Role Theory' and the emergence of 'Egalitarian Marriages' give us?

In Rainwater's wake the question of whether or not roles in marriage and the pattern of sexual behaviour can strongly influence fertility can be asked. Rainwater has, as we know, been able to find support for the theory that in marriages where the couple together share the organisation and responsibility, this is where family planning has also been introduced at an early stage and led to small families. When, on the other hand, the couple have had clearly defined roles with largely different types of tasks, when interest is greatly directed towards children and a home and where sexuality has played a leading role, this is where we have as a result comparatively large families. Even e.g. Scanzoni has obtained results which support the basic assumption that 'egalitarian marriages' work in the way that Rainwater supposed.

It might seem rather bold to go out and look for egalitarian marriages in the age groups that we have studied and in the type of social environment that has been described above. The interviewers, however, can relate at least a general feeling that the relative equality in these marriages (where both partners have been able to be observed) has probably had a certain significance on the fertility. Here, however, we are faced with difficult method problems. How can one find useful indications of equality, and does one not risk confusing a woman's present emancipation in relation to her husband with the situation forty years ago, which might have been quite different?

The answer is that the interviews give several indications, both from
now and in the past, about the relationship between the couple (even
where considerable discord occurred). There is, however, a great deal of
work left before we can possibly isolate a small group of 'egalitarian'
marriages and compare the number of children in these to others. Reversed,

it should not be completely impossible to discern a group of marriages
where the husband was very dominating.

The attitude towards children

Another factor which is difficult to capture but highly relevant is how the
couples felt about children. It is very possible that the difference between
the past and the present is greatest precisely on this question of whether
the couple wanted children at all.

Even if the people we interviewed were quite unconscious of Hoffman
and Hoffman's seven dimensions when children are concerned, they have
apparently always been decided upon having children, at least one. On the
other hand, it is doubtful, if they planned on the long term, that they
thought about the value of children (and grandchildren) in their own old
age, even from other aspects than that they could help their elderly pa-

rents economically.

Childlessness was as a rule considered as directly tragic. Most of the
childless marriages in the area had foster children, in some cases fetched
from Paris (!) at the end of the war.

This basic attitude guaranteed a certain minimal number of children in
the new generation which grew up during the mid-war period, but it has
not as a rule led to large numbers of children. People could apparently
feel just as satisfied with one or two children as with seven or eight.

Finally it can be emphasized that the norm of 2-children families ('like
everybody else in our circle of friends') was well-established, exactly as
Söderström found in the city environment. This was despite the fact that
the change of norms occurred so rapidly in Rätan. On the other hand,
people do not seem to have found 4-5 children to be 'almost unlimited fer-
tility' as Söderström expressed the matter. The memory of really large
families was still alive in this forest district in the interior of Norrland.
CONCLUSION

1. The Råtten Study shows that retrospective interviews with certain given preconditions (mainly 'stable questions' which lie centrally in people's consciousness) are practicable.

2. It has also shown that even delicate matters such as child limitation and sexual aspects of married life can be illustrated with a moderate loss in the form of non-response.

3. The study has, on important points, been able to detail and complement our analyses of family structure and fertility behaviour, studies which are normally based on so-called family cards. The picture of causes can thereby become different, not least the role division in marriage and the attitude towards children can receive more attention in our attempts to grasp what happened during the great decline in fertility.

4. In passing, some partially surprising results have been accounted, which show that 'stationary' populations should not be contrasted with 'mobile' populations quite without reservation as often happens within migration research: the three-generation family should perhaps not be considered a myth in all economic and social environments even in the Scandinavian countries: the isolation and pitiful situation of pensioners (in a society completely built up around the occupationally active and productive citizen) should perhaps not be allowed to appear as a generalisation without modification.

5. Cooperation between genealogic research and university research can sometimes be established.

6. Data banks of a new type more based on the kind of oral information (of 'soft data' type) such as the Råtten Study gives a taste of, should be established in the future. A more coordinated and perhaps institutionalised programme for the collection and storage of such information should be established. This is an urgent task.
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Information from Chris Van den Broecke about ongoing research based on interviews.
SUPPLEMENT

Characteristics of person interviewed:

Name:
Parish of birth:
Year of birth:
Married to:
Own Occupation or work duties:

Husband's/wife's occupation:
1. At start of marriage:
2. During marriage:
3. Period of unemployment:

Childhood and relatives:
1. Where did your parents come from?
2. In what way did they support themselves and their family?
3. How many children were there in the family?
   What placing did you have among the children?
4. Did any of your brothers and sisters die in infancy or childhood?
   What were the causes of their deaths?
5. At what age did you start to help at home? With what?
6. What contacts did you have with your maternal and paternal grandparents? Were there any other close relatives who lived nearby or with you? Did you see them regularly?
7. How many years did you attend school?
8. Have you completed any other education later?
9. How did you receive information about the outside world?
   Did you have a newspaper in your home?
   When and where did you do your military service?
10. Did you travel outside the parish when you were growing up?
   (Östersund, Stockholm)
11. When did you start work?
   Where? As what? Did you live at home? Did you keep any of your pay?
12. Why that occupation?
Adult life

13. When, where and how did you meet your husband/wife?
14. Were you engaged for long?
   How long?
15. When did you marry?
   Why did you decide to marry just then?
16. Did you/your wife/ stop working when you married?
17. Did you get a house or household of your own when you married?
18. What did your parents think of your marrying?
19. Was your husband's income from the farm (forest or other source) enough? or did he obtain money elsewhere?
20. Did your husband have to stay away from home longer or shorter period due to his work?
21. Did you have to do jobs that were really man's work?
   What jobs did you/your wife do?
   Could men do the milking?
22. How many children did you have?
   When? When did you have your first child?
23. Where did the birth take place?
   At home, in hospital?
24. How did you feel about having a baby?
   Were you scared?
   Had anyone told you about the delivery, so that you knew what to expect?
25. How did the delivery take place?
   Hygiene, preparations, who delivered?
   Could one in any way make the birth easier, and relieve labour pains?
26. How did you feel about the next birth?
   Were you as scared or not scared then?
27. Did you breast feed your children?
   If so for how long?
28. How were children treated in those days?
   Did one prefer boys or girls?
   Were boys and girls brought up differently?
   How were babies looked after at the beginning of the 20th century?
   Were there any differences compared with today?
29. Did you and your husband (reversed) at any time talk about how many children you wanted?
   Did you at any time during your marriage use any method to prevent
the children coming too close together or to avoid too many children?
How did you receive the information?
If so what method? (Coitus interruptus, refraining, condom, cap, etc)
Was it otherwise common for women to have miscarriages?
Could one induce a miscarriage? (Abortion)?
30. Did one pity people who could not have any children?
31. Did one think that the cause of childlessness lay with the woman or with the man?
32. Did one think that there were people who voluntarily refrained from having children, if so why?
33. Was it unsuitable to have 'too many' children?
If so, how many was 'too many'?
34. When should one have one's first child (after marriage), why?
Was it embarrassing to have children before marriage?
35. How many years should there be between the children, why?
36. What time of the year was it best to have a baby, why?
37. Did it ever happen that anyone divorced?
38. What did one think of divorce?
39. What was your situation economically during the time your children were growing up?
Could you manage during the 30's?
Was there much unemployment during the 20's and 30's?
Were you or members of your family affected?
What did the war and military service mean for the family?
When did the children start work? Did they have a longer schooling and education than you yourself?
Could they keep their pay or did it go towards the family's economy?
When did they leave home and what do they do now? Where?
Did you ever have plans to leave your home district? Where to if so?
40. Have you been active in any organisation? Which and when?
Have you been politically active? At what time?
Were questions concerning the family e.g. family planning discussed in these organisations?
41. Have you been interested in books or cultural activities in general?
How could one obtain books and literature?
Was there an organisation of lectures?
Did you ever go there? Have you, if so, any memories of interesting lectures or lecturers?
Life as a pensioner

42. If you compare your situation as a pensioner with that of your parents when they got old which do you think are the most important differences?
43. What activities (interests, hobbies) do you take part in as a pensioner?
44. Can the Municipality make these activities easier?
45. Have you travelled as a pensioner?
   Where to? Abroad? On charter flights etc; do tell us.
46. What was the housing and service situation of the person interviewed?
   To be described afterwards by the interviewer.
47. What social life do you have?
48. How often do you meet your children and grandchildren?
49. Have you had trouble with your health, been in hospital etc.?
50. What should be done to make life easier for the many elderly people in the society of the 80's?

Other views and comments

51. In most interviews one can almost count on views and information being given that can hardly be predicted by us. It is very important that we sensitively take care of this further knowledge given here and write it down in connection with the interview questionnaire.
THE LIVES OF THE INHABITANTS OF RÄTAN

During the summer of 1979 a small group of researchers from Uppsala University and the Regional Archives/County Museum in Östersund will be carrying out interviews in the parish of Rätan. We are first and foremost interested in the generation that married during the mid-war period in the 1920’s and 1930’s. We hope to be able to ask questions about life, work and family relationship in order to obtain a more complete picture of the conditions of life in a district in southern Jämtland than we can otherwise obtain from statistics and printed sources. We think it is important that people’s experiences and memories are taken care of for future generations who otherwise can hardly imagine what it was like to be a child when the century was young, or to come out onto the labour market or be a maid-servant or housewife in the 1920’s.

We have selected you, and many of your contemporaries, for an interview and we hope you will take part and answer our questions. Our interviews will be starting the week after Midsummer and we will be getting in touch with you by telephone to tell you more about our research and to come to an agreement about a suitable time for a talk.

Uppsala, 11/6 1979

Sune Åkerman
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