

En mer omfattande undersökning av de danska statsfinanserna vid denna tid ligger också utanför ramen för denna undersökning. Den kungliga kassan och Christian IV:s kapitalpolitik insatta i ett större sammanhang skulle dock kanske kunna möjliggöra ytterligare bestämmningar av de motiv som styrde kungens penningutlåning. Hade han t. ex. politiska bevekelsegrunder att genom dessa lån göra adeln politiskt beroende? Kan stödet till handelskompanier osv. i linje härmed ses som försök att vinna sympatier hos andra grupper inför en kamp mot rådet och högadeln? Ladewig Petersen hävdar att det inte förelåg några motsättningar mellan kungamakt och aristokrati vad gäller den ekonomiska politiken under Christian IV. Frågan är emellertid om inte t. ex. diskussionerna i samband med adelsbevillningen 1623–24 – då rådet uppenbarligen ville sätta skrankor för den kungliga maktutövningen inom finanspolitiken – visar på ekonomisk- och därmed också maktpolitiska motsättningar mellan Christian IV och adeln. Ytterligare forskning i detta större politiskt-finansiella perspektiv skulle kanske kunna kasta ytterligare ljus över dessa frågor och därmed syftena bakom Christian IV:s kapitalpolitik.

Sammanfattningsvis vill jag framhålla att Erling Ladewig Petersen med denna undersökning behandlat ett mycket intressant ämne. Framställningen redovisar inte endast viktiga resultat inom ett hittills försummat område. I stor omfattning stimulerar därtill resultaten till ny forskning inom angränsande ämnesområden. Ladewig Petersen har med detta arbete fogat ytterligare en viktig pusselbit till sitt utforskande av dansk ekonomisk historia under 1600-talet.

Berndt Fredriksson

*Anders V. Kaare Frederiksen: Familierekonstitution. En modelstudie over befolkningsforholdene i Sejro sogn 1663–1813. Københavns Universitet, Demografisk afdeling, Statistisk institut: Publikation nr. 1. Kbh., Akademisk forlag 1976. 176 s., 50,00 kr.*

*Kjeld Villadsen: Pest, skatter og priser. Befolkningsudviklingen i Køge 1629–72. Skrifter udgivet af lokalhistorisk afdeling nr. 7. København, 1976. 96 s., ca. 30,00 kr.*

During the second half of 1976 two short books have been published on population developments in small areas of preindustrial Denmark. Even though these studies have a common research theme – population structure – and common basic sources – church records – their design and presentation are diametrically opposite. Each of the researchers represents one branch of historical population literature. Villadsen belongs to the group who attempt to answer historical problems through statistical observation. Frederiksen belongs to those who improve the statistical basis for demographic observations and who emphasize methodological aspects. This opposition regarding research strategy and goals is well known; there is even a terminology: V. can be said to be a demographic *historian* while F. can be called a historical *demographer*. The strength of these two groups is far from equal: the statistically and methodologically oriented historical demographers have long dominated the field.

Putting researchers into categories is no simple matter. The case of F. is particularly complicated. He is an economic historian working in close co-operation with professional demographers. What he presents in this book is clearly methodological. However, his original aim was to write demographic *history*: to show "hvorledes befolkningen ... reagerede på skiftende økonomiske vilkår." He cites with approval Pierre Goubert who is a hero among most of us who desire to do more with population studies than assemble hard facts. F. stresses that most of the literature has previously neglected "at se en sam-

menhæng mellem socio-økonomiske faktorer og de demografiske hændelser". In this review, however, I must neglect F.'s ambitions and deal with the book as it stands. Later on I will take up some of F.'s methodological standpoints which I feel cannot rhyme with a goal of setting demographic information into a social and economic context.

V. is much easier to deal with since his aims and his presentation coincide. He begins with a problem formulated by Niels Steensgaard in this journal a few years back. Steensgaard questioned whether there was a direct connection between the population decline in Denmark and other Western European countries during the 17th century and the growing financial demands of the state. Researching this problem involves combining demographic information with various sources dealing with personal taxes plus quantitative data. Køge is an ideal town for such a study: the church records are among the earliest in Denmark – the burial register begins in 1629 – there is considerable economic and financial data preserved in the *Kæmnerregnskaber*. Køge is also an extreme case of demographic and economic decline: at the beginning of V.'s study it was among the 10 largest Danish towns at the end it had slid down to 14. Trade through the port declined with about 100,000 daler "i løbet af en menneskealder". Actually the extremity of Køge's situation makes for problems in testing Steensgaard's thesis: it cannot be clear what played the greater roll for population regression – the growth of taxes or the general decline of the grain trade.

Before discussing these works any more I shall present their main findings in the order the authors present them. F. begins with a short description of Sejerø parish based on literature and a few unprinted sources: præsteindberetninger, amtsregnskaber for certain years. Sejerø is an island off Odsherred NW of Sjælland. Although administered as part of Sjælland it differs economically and socially from conditions there. Up to the middle of the 17th century shipping was very important, throughout the period agrarian production was probably inadequate for needs. Barley was the crop most widely grown and some trade was necessary to procure other grain sorts to get a good flour mix. In addition fishing ought to have had a place in household consumption. Since the end of the 17th century the island was entirely in noble hands. However, the geographical situation meant that *vornedskab*, *stavnbaandet* and *corvée* duties had little effect here.

On many occasions F. contrasts Sejerø which he calls a "closed society" with Tårnborgh parish outside Korsør which he calls an "open society". The term "closed society" is perhaps not adequate since F. produces figures for migration based on the number of baptised who are not found in the burial book, and the number of buried not found in the baptismal book. These are figures for net migration only, the brutto turnover must have been greater. He finds a considerable out-migration for men and some in-migration for women. Of all married women 1663–1813, 40 % were born outside the island. The strong male out-migration is connected by F. with shipping, which must have functioned as a safety valve for those who could not set up a household on Sejerø. Instead of "closed society" I would prefer to call it a "centrifugal society".

The method F. uses to organize his demographic data is family reconstitution. He does this partially with the use of dataprocudures developed at the Statistical institute in København. However, a considerable amount of manual work is also needed before the material is in condition for calculation. F. presents graphs over the total population of Sejerø 1663–1813 and Tårnborgh 1633–1813. His previous study of migration allows him to complete figures based on yearly baptisms and burials. From the late 18th century census he goes backward in time adding and subtracting births burials and migrants. With reservation for the condition that he only takes up permanent migration and neglects

the cyclical migration of servants his trends are reliable. There was a considerable stagnation in Sejersø after 1680. In Tårnby there was a vasculating stagnation from 1650 to 1700, thereafter the total population began to rise. He then calculates different crude rates: births, deaths and marriages related to total population.

In chapter 4 and 5 he compares family reconstitution results with various population lists. He finds, as many others, that the skattemandtal of 1660 is not especially reliable except in local studies where control is possible. However, regarding the censuses of 1767, 1787 and 1801 he finds close agreement both as to population number and age pyramid.

In chapters 7 through 9 F. presents the calculations made on the actual family reconstitution: age specific marital fertility, mortality in different age groups, nuptiality. He finds very high fertility on Sejersø. However, since the age of women at first marriage is relatively high and infant mortality very high the natural growth of population was moderate. A thousand Sejersø women in 1769–1787 gave birth to 4665 children; of these the effective population increase was only 2275 (brutto) – the net increase – in this case the number of surviving female babies – was a bit over 1100. This means that had fertility been just a little lower the island's population would not have been able to maintain its level without net in-migration. Infant mortality was never below 200 per thousand. Only about half of all children survived to age 20. The expected average length of life at birth on Sejersø was 30,7 for 1787–1801 which was much less than in Tårnby which had 39,1 at the same time.

Villadsen is methodologically less ambitious than F., but he studies a problem – the integration of economic and demographic data – where methodology has just begun to emerge. In the first two chapters he presents and evaluates the sources. V. rightly is cautious in judging all the material available. The essential moment in his source-criticism lies in a comparison between the church registers kept by the clergy and the church accounts kept by the church wardens. He finds many cases where the burial register did not correspond with the fees for burials recorded by the wardens. Neither source, however, appears better than the other. This situation is especially problematic in the several plague years. Although neither list is perfect the two lists supplement one another. V. finds underregistration in the baptismal list: of still-born, and twins. Unfortunately, he does not make the same minute source analysis of the economic documents. He accepts the kærnerregnskaberne's information on the intake and expenditure of the town. From tax registers V. gets data on the number of households – both solvent and insolvent – plus the number of vacant dwellings.

Chapter 4 is the main part of V.'s study and presents the empirical results of the demographic research. He begins with trends for legitimate and illegitimate births. Illegitimacy was as high as 15–17 % of all baptisms – this is a very high figure indeed. An interesting correlation is that legitimate and illegitimate fertility fluctuated positively: when the number of births inside marriage went down the illegitimate births also decreased. This is a connection which Peter Laslett and Karla Oosterveen have recently revealed for England in an article (not cited by V.) in *Population Studies* 1973. During the 1650's Køge experienced a severe subsistence crisis. V. can effectually show that this crisis led to a reduction of births in 1652–53. It is possible, he states, that this is a result of family planning. V. even finds a marked decline for well-off large-family burghers at that time. Therefore the fertility decrease cannot be solely attributed to undernourishment. Here V. could probably have profitted by citing J. Meuvret's study of contraception during a subsistence crisis in France 1694–96 printed in *Population and History* (1965): it appears to be a parallel case. After this crisis 1652–53 fertility regained its earlier high intensity.

Mortality and especially infant mortality was very high in Køge during the 17th century. From 1645 to 1672 there were 3132 burials in the town, simultaneously there were only 2364 baptisms. Epidemics and wars kept the death rate high. V. attempts to measure migration indirectly through the lists of new and resigning burghers. Up to 1648 there were about 22 new burghers per year, thereafter only 11 annually. Tax lists show an increase of payers up to 1643, but in the 1650's the number sank rapidly. Out-migration from Køge is connected with an increasing number of military financial burdens beginning in the 1640's and intensified in the 1650's: there were years with up to 6 separate extra levies. In the 1660's a slight in-migration resumed. V. calculates that Køge's population went from 3348 in 1637 down to 2322 in 1655. Here he multiplies the number of households by 6,5 up to 1643 and 6 thereafter. I dislike multipliers in principle – especially imprecisely motivated ones such as used here. The decrease in the number of households is, to my mind, sufficient to show a decline of this magnitude.

Chapter 5 briefly sketches the economic and financial situation of Køge. The town was an important grain shipping and transit port for the Sjælland surplus. This was quite large up to the middle of the 17th century. V. relies heavily on G. Olsen's study for general trends of the grain trade. He gives a description and some figures on the decline of the grain trade which he sees begin in 1647. The burden of taxes increased in the wake of Christian IV's unsuccessful foreign policy ventures. The "normal" economic problems were exacerbated by a catastrophic fire in 1633. Those burghers who were not affected by the fire, had to assume the tax burdens for those who got tax liberty for rebuilding.

By far the most important event was the occupation of Køge by Swedish forces at the end of the 1650's. Taxes had been increasing steadily since the 1630's but the occupation was something very special. In the first year 32,000 rdl. were taken in brandskat alone. In the 1640's the towns tax amounted to 4,000 annually. In the 1660's the financially weak Danish state continued where the Swedes left off. Higher taxes were demanded of the ever fewer inhabitants who were getting less and less income. The situation did not make Køge attractive for merchants, a notoriously mobile group.

V. succeeds in connecting economic factors with population development. He shows how the decline after mid-century must be seen against the background of unsatisfactory economic developments both in Denmark and throughout Europe. In this way he has been able to revise Steensgaard's thesis that worsened economics lead to a reduction of population through widespread use of contraception. This may have been the case in some areas, but in Køge decline was more a matter of a negative balance of births and deaths plus a large migration. Steensgaard was probably too quick to relate reduction of population to a fertility decline. Fertility decline gives a very slow and gradual effect on population size. Such a decline can, however, explain why recovery was so slow after the initial dramatic fall.

I shall now return to some basic problems with Frederiksen's book. In an introduction to *Familierekonstitution* lektor Hans Oluf Hansen of the Demografisk afdeling emphasizes the importance of F.s research "i erkendelse af kildekritikens generelle betydning ved historisk demografiske studier." I think it therefore important to discuss the type of source analysis presented here. F. represents a source critical attitude commonly held by statisticians: if there is a suspicion of a fault in a source, the source must be isolated and avoided. In the case of church records the process takes the form of peeling an onion. All families in those layers which are suspected of having some technical fault are removed until at last there is a hard kernel of families about which all information

is above suspicion. F. also has a rule that no sources should be combined with each other: he mentions that there is a risk that a fault from the one series could be carried over to the other. I think this is a point where many historians can have a different opinion: it is often necessary to use all information available from closely related sources as long as they are reasonably reliable. Many of us feel that no historical time series can ever be perfect in the sense that statisticians aspire to. All sources have their problems: the source criticism historians are trained in allows them to combine sources because they have learned how to compensate for the *known* faults in the material. To me F.'s cybernetic source-criticism and methodological purity is abstract and unmotivated. F. discusses for instance Wrigley's study of family planning in England during the late 17th century. He notes that there is some underregistration of still-births in the church records and quickly draws the conclusion that "Wrigley's fertilitetsanalyser falder til jorden". Not at all! This is a matter of a small adjustment and has little consequence for the more complicated (not related by F.) analysis Wrigley makes. F. is apparently unaware of Wrigley's recent work on underregistration of baptism in the journal *Local Population Studies*. This is a typical example of how the slightest doubt becomes disproportionately magnified in the methodology presented here.

One other standpoint taken by F. is especially irritating. He discusses whether or not a population historian can divide his families into social groups such as "gårdmænd" and "husmænd og inderster". On page 83 he calls such a division "værdiløs". Many of the propertyless were formerly farmers, and F. feels that the risk for this sort of career mobility was so great that all attempts to isolate a group of cottars would be futile. As support for this reasoning F. cites F. Skrubbeltrang, however, this must be a misreading. What Skrubbeltrang says is that at any one moment the propertyless might include up to 20 % retired farmers. This situation has little relevance on the value and possibility of distinguishing between propertied and unpropertied *young* families. F. could have tested his theory of career mobility on the various farm histories published by local historian Rasmus Nielsen in *Sejerø sogns historie II*. Swedish experience is that when such career mobility exists there is a close connection to nearness to a large estate. I cannot accept that Sejerø was such a case without much more evidence than is presented in this book. In addition, if F. denies the value of social differentiation in Danish agrarian demographic history, it seems pretty impossible that he could fulfil his aim of relating demography with social and economic conditions.

*Famlierekonstitution* has the subtitle "modelstudie". Perhaps this means that the Demografisk afdeling plans to publish many more similar books in the same pattern such as is done in France after Henry's model. It would be unfortunate if this pattern were to dominate. We need to know more about demographic variations over time in relation to other social changes. The great number of church records and censuses in Denmark are not the only relatively valuable sources for such studies. There are also maps, financial documents, fæstekontrakter, estate archives, probate records plus much more which in combination can really enable a Danish population researcher to penetrate this important topic.

David Gaunt