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The Rich Past and Dessicated Future of Parish Register Demography

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My contribution to these proceedings is rather like *deja-vue, all over again*. I wrote a short paper in 1988 when David Weir and I were pitted against one-another in a "where do we go from here?" discussion. My position was that of the pessimist, acknowledging past discoveries that had emerged from the first generation of scholarship in parish register demography but warily concerned that the best was not yet to come but had already arrived.

I have chosen to present that paper in its original form. I decided against re-writing the text, because I think that it should stand as is - also, I'm concerned that, to quote The Bard:

"untune that string,
And, hark, what discord follows! each thing meets
In mere oppugnancy"

In any event, the paper was never published and I thought that it was lost. But it turned out that a former graduate student (Ernest Benz who is now teaching at Smith College) had a copy. So, my contribution to this "fetus Christ" (Best auto-correct ever! - "*fetschrift*"!!!!) for Roger Schofield - who was my supervisor when I embarked on doctoral research - will be my contribution to these proceedings.

Our session at the 1988 Social Science History Association meeting in Washington was held in a small room with limited seating. Roger Schofield was there. Looking back to that event, I want to quote The Bard again:

"When a man's verses cannot be understood nor a man's good wit seconded with the forward child, understanding, it strikes a man more dead than a great **reckoning** in a little **room**."

For me, however, the most memorable part of this session was that a few minutes after it started, an tall, elderly man with thinning white hair (he was, maybe, about sixty!) entered the small room, which was filled with a number of other Young Turks whose academic careers had been energized by the pioneering research being organized by Roger and Tony Wrigley. Finding no seat free, this man sat on the floor, just like a graduate student. That gave me the chance to tell the audience that this was going to be high point of my academic career because Lawrence Stone was, here-and-now, sitting at my feet. I knew Lawrence and knew that he would be humoured by such gentle joshing - he actually blushed!

In my argument, it seemed to me that two key questions had emerged from the "heroic" period of parish register research. First, of course, was the confirmation that Shakespeare was quite wrong in suggesting - or being mis-represented as having suggested - that in merry olde England teen-aged swains married teen-aged virgins. As John Hajnal had posited, the marriage pattern in early modern England - and north-western Europe, more generally - was very different. And, second, Louis Henry's claim that the pre-modern population practised "natural fertility" was very misleading yet his argument did force us to wonder how and why and when populations brought fertility within the realm of conscious choice. As we all know, our predecessors born in the 1860s had an average of seven children whereas two generations later - in the inter-war period - a wholly new fertility regime had taken hold and the modern family-size was two. This was very much the fertility pattern in my own family - my maternal great-grandparents were born in the 1850s and had twelve children; my parents were born in the first decade of the 20th century, married in the early 1930s, and had two children.

Parish register demography alerted us to these key issues. In effect, I suggested, we came to know what Hajnal and Henry had postulated but we didn't really know how to explain their postulations. And, I argued, there was little likelihood that those issues could be understood by heaping more and more studies into a larger data-base. The first generation of research was, like most opinion-polling or consumer-sampling, remarkably accurate within a small margin of error. Something new was needed. Historical demography needed to become social history because past populations not only lived in the past but also in radically-different social formations. Family formation decisions were calculated according to their own reasons which were dissimilar to those of their successors and/or predecessors. This was

why I believed that the situation in 1988 had reached a kind of dead-end because we had illuminated our ignorance but not explained it.

Why were Tudor-Stuart men and women marrying a dozen years after puberty? Why did their Victorian great- great- great-grandchildren limit fertility without recourse to either chemical intervention or physical means of prevention? This second process was a "compass swing" in which novel calculations of family formation took place. Jack Caldwell's explanation of that "compass swing" was based on aggregated data which signalled the contours of this world-historical change but didn't properly explain its micro-level mechanisms by situating the radically-new calculus of conscious choice in the bedrooms of the modern world. The fertility transition needed to be understood as the product of millions and millions of individual decisions so as to make sense of both central tendencies and standard deviations in the family formation strategies of the two generations which reproduced themselves in the context of the modernization of the western world. What were the novel ways that state intervention in health, education, and welfare were imbricated in their family formation behaviours? And how did these behaviours reflect class and status, cross-cut by rural and urban differences? For me, however, this was a road not taken - access to the relevant data was at that time both proscribed for privacy reasons and so expensive that it was impractical.

So, let's turn to my 1988 TEXT

I have come here neither to praise nor to bury parish register demography. Rather, this forum provides an opportunity to understand its contributions while suggesting how those contributions point the way ahead in the study of historical populations. It is a way ahead that can best be pursued with new paradigms rather than with further research into primary materials. I am not arguing against the pursuit of new research *per se*, even though to my mind it won't significantly extend our understanding. Simply adding more case studies to the impressive mountain that already exists seems to me to be a rather mundane activity unless it is accompanied by the realization that the people who married, had children, and died in full view of our demographic microscopes experienced much more besides. Demographic statistics are revealing, no doubt, but their collection should not be allowed to justify the field nor should it be allowed to be its *raison d'être*. The problem is not to count the world we have lost but to understand it.

I should mention at this point one very significant caveat to my remarks: while I am hardly sanguine about the prospects for historical demography in the parish register period (i.e. c. 1550-1850), the era of the "demographic transition" is really another matter. Research into local sources must proceed with celerity; the enormous research effort of the "European Fertility Project" is by no means the last word on that subject, as the marvellous 1984 paper by Jane and Peter Schneider on Villamaura, Sicily, must make clear. The unitary "demographic transition" must be broken into a thousand pieces which reflect the combined and uneven nature of the process masked by the Princeton Project's aggregated statistics. But that is really another matter. I will, therefore, try to restrict myself to the parish register period.

The pioneering stage of historical demography is over; now we have to come to terms with it. Setting structure and process in a single plane of focus is easier said than done. Yet it is imperative to do so, because we need to understand the relationship of the parts to the whole. Within the small field of historical demography, we have learned quite a lot about the interaction of the various constituents of the population equation across time and space and, if we will never have really accurate measurements, we now possess reasonably certain approximations of both central tendencies and standard deviations. Each measurement is important; central tendencies give us a singular indication of the centripetal forces bringing together cultural, biological, and material considerations, while standard deviations tell us that that singular computation was the product of contested interpretations.

I am now going to turn the normal social science of historical demography on its head - instead of establishing a data-base and analyzing it by age and sex, I am going to postulate that we set aside the search for the perfect number. This is hardly a counsel of perfection; one might in fact say that it is a counsel of desperation. There is to my mind no question that unless we address directly the larger questions that frame our empirical analyses, we cannot really gain a feeling for the organism. From an empirical standpoint, there is nothing to recommend this procedure. But, to my mind, there is nothing but a fractal recursiveness to be found in the continuation of early modern parish register demography in the absence of a confrontation with the brute facts uncovered by the pioneering generation of researchers.

I think that we can and must use the essentially arithmetic mode of reasoning inherent in demographic analysis to interrogate our other forms of historical documentation. Much the most important and most interesting area for consideration lies in precisely that circle of confusion beyond the focal length of our normal optic. Just as we were taught that social history was built upon the persistent interrogation of representativeness in both sources and argument so, too, is population history built upon the Malthusian recognition that when little changes accumulate, they have big results. Let us have a brief look at this territory.

Uniquely, north-west Europeans married late. To be more precise, the link between puberty and marriage was dramatically more attenuated in north-western Europe than elsewhere. In an abundance of studies this austere, 'Malthusian' regime has been identified as a primary characteristic of the parish-register period between the Reformation and the Industrial Revolution. Indeed, its delineation has been the greatest achievement of early modern historical demography. The average age at first marriage for women fluctuated around twenty-five; about two-thirds were between twenty-two and twenty-eight. A few teen-aged brides were counterbalanced, as it were, by a similar number of women who married for their first time after their thirtieth birthday. Furthermore, many remained spinsters or, as it is so inelegantly and (maybe) incorrectly inferred, 'permanently celibate'. Historical demographers have become familiar with these statistics; it hardly needs emphasizing that they are quite remarkably unlike anything found elsewhere in the world.

Obviously, one wants to know what was uniquely "European" about this mode of cultural reproduction and, more particularly, what characteristics were specific to the northwestern areas of Europe. Isolating the issue in this way has the very great merit of drawing our attention to the fact that late marriage does not seem to have been a Mediterranean experience at the end of the Middle Ages even though there does seem to have been a "convergence" of demographic systems in the course of the early modern period so that the stark differences between Northwestern and Mediterranean Europe were more muted by the end of the nineteenth century. Nevertheless, answering the questions posed by the singular fact of a mean age at first marriage of twenty-five will not be the province of further research into parish registers. If, as seems reasonably clear to me, this north-western European marriage pattern was to be found among the peasantry of the High Middle Ages, then for all intents and purposes, we can forget about finding much documentary evidence with which to analyze its origins, much less to explain them. One will have to look elsewhere: to an historical

biology or to an ancient and medieval social history informed by a sensitive reading of cultural anthropology. More on this later. The problem is not to describe this unique development so much as it is to explain it. Such an explanation will not come from the reconstitution of parish registers.

If we switch from nuptiality to that other region of demography within "the calculus of conscious choice", we know that the so-called "natural fertility regime" was very significantly below the physiologically possible level of childbearing. Here again it is salient to mention that any average measurement is the product of contested interpretations. It is taken for granted -accepted as a 'fact of life'--that pre-transition populations displayed tremendous internal variation. But one wants to know if this was really as ancient as the hills or part of a human social construct like the terraces of Macchu Picchu. If one were to interrogate family histories with standard deviations as firmly in mind as central tendencies, then our study of pre-transition fertility history would become more complex. But that is not to say that that added complexity would give us much insight into the motivations of those who chose small as opposed to large families. Was there, in fact, an element of choice in the size of family before the "demographic transition"? In what manner was "natural fertility" natural? Most think not; I remain guardedly agnostic on this issue and would like to see it considered more carefully. To do so, one cannot render transparent the "proximate determinants of fertility" - as Henry did when he lumped them together in a mixed bag of cultural residue - but consider them as McLaren has done, as reproductive rituals in which they were active elements of agency of those involved. Micro-level analysis of mortality would similarly reveal a bewildering complexity of experience which has been homogenized in our standardized measurements. But because, for all intents and purposes, mortality is outside "the calculus of conscious choice", we might leave it aside from the kind of argument I am developing about the consciousness of calculation and choice.

In addition, we most definitely need to gain further insight into both region- and class-specific interpretations in so far as they deviated in systematic ways from the average. This is likely to be easier said than done. Having spent an enormous amount of time in the creation of four family reconstitution studies using recalcitrant English registration materials,--the thought of doing a fifth would make me reach for a gun if I were not such a peaceable guy--I can only gasp with amazement at the tricks that Bernard Derouet has been able to make his French materials perform. His 1980 article in *Annales, E.S.C.*, and the accompanying piece

in *Études Rurales*, are beyond the reach of my material. I have tried with Terling, Essex and lately with Whickham, County Durham, to squeeze the same sort of information from my family reconstitution forms but it has been a sad reminder of the narrow frame of reference inherent in a parish register to discover that only 3/97 miners from a 1752 pay bill could be linked with the appropriate demographic materials in an attempt to gauge miners' ages at first marriage. Of course, it could be said that with more data the numerator might be sufficiently large to permit meaningful analysis, but if you had spent several years assembling the family reconstitution forms and had a colleague spend several more years sifting through other documentation, then such a counsel of perfection would likely ring hollow. Perhaps a luckier researcher - or team of researchers - might be able to assemble bigger numerators. At what cost? And, for what purpose?

Perhaps the trick might be to avoid English materials with their notably laconic descriptions and their irritatingly characteristic population turnover which renders a small 'reconstitutable minority'. Yet the very fact of extensive population turnover which was such a problem for demographic analysis proved to be salient to understanding the process of class formation in the pivotal region in the early industrial world. Indeed, because of my teleological belief that one of the primary reasons for studying parish register demography is to explain the emergence of the first industrial society, I remain convinced that the social and economic history of early modern England is more important than that of other countries. So, to my mind there is little comfort to be gained from using better documentation from Thimerais, Liège, or Baden-Württemberg to address issues that are really within the orbit of English history and its peculiar variations on a wider theme.

Let us leave aside that particular point and return to the more general one concerning the unique configuration of family formation in north-western Europe. We might well ask: how can further studies of parish registers contribute towards solving the relationship between marriage, domestic organization and economic opportunity, the central *problématique* in early modern demography? Where did the apparent 'rules' which stressed economic independence before marriage come from? We might also ask: how can we make use of parish register demography to gain an insight into the mechanisms--some like to call them 'cultural factors'--which made prolonged breastfeeding an acceptable behaviour in some areas but not in others? As we know from a large number of studies on "the proximate

determinants of marital fertility", this is the key to understanding the hinge on which the so-called "natural fertility regime" swung.

The answer to these two kinds of questions, it seems to me, is that more parish register demography will be more "normal social science", when what is required is, in Kuhnian terms, a paradigmatic shift or "revolutionary social science". Indeed, in acknowledging the value of what we have learned and what we can add to that knowledge, there is the likelihood that we will get carried away by our pursuit of perfect numbers. In a field of study as rife with ambiguity as historical demography, that is a waste of time. We are, I think, closing in on the margins of diminishing returns even though we will no doubt learn a great deal more about micro-level demography from additional local studies. Furthermore, to my mind, and in the Aristotelian sense of 'opinion' rather than 'knowledge', that micro-level demography will be interesting in its own right but not otherwise.

Another important point needs to be made in this context: much of the most stimulating work in parish register demography has been promoted by just such paradigmatic shifts: I think here of Hajnal's seminal article on the European marriage pattern and Laslett's "null hypothesis" which spurred a massive amount of work on household organization. Can we expect another 'great leap forward' if we forswear research? Probably not. But I am reasonably certain that only by rethinking the results the first generation of research has now made available can we do more with them.

Indeed, we need to break out of the puzzle-solving mode which addresses only those issues that can be both stated and solved within the existing scholarly tradition. Since no discipline is an island, there is an urgent need to read our demographic results against the grain of other ways of seeing. This is risky business, to say the least. It guarantees nothing.

I don't want to end on that note, however. I want to strike another, more personal, and probably more discordant one.

One of the great joys of being an academic is to learn about one's ignorance. That is not a statement of humility so much as one of fact. Having spent most of the last twenty years crunching and squeezing, finessing and teasing, parish register demography in the search for answers to some perennial issues in the making of modern society, I have decided to set such

research aside. In truth, I've had enough of it. I won't be doing a fifth family reconstitution study. There are other things to read about and other ways to do research while maintaining an allegiance to the questions that brought me to Silver Street, Cambridge, in the fall of 1970.

My next book which I am now beginning to write (n.b., it never saw the light of day!) was to be about the revolution in the family occurring in this century, or at least since the birth of my grandparents in the 1880s. During this time the control of reproduction has come almost wholly within "the calculus of conscious choice". Indeed, it is taken for granted in our everyday lives even while remaining a source of contention in establishing reproductive control as a 'natural right'. But in beginning this new book, I found myself returning to a series of problems that have occupied my attention during the whole of my professional career. In a wider context, it became clear all over again that what is truly remarkable about the demography, the biology, and the economics of the past century is its "modernization" (a word I use advisedly and with caution), a "modernization" built upon the peculiar organization of production and reproduction revealed in the findings of the pioneering phase of parish register demography. Early modern demography has all the characteristics of a 'chaotic dynamic'--a disorderly system of behaviour acting as a creative process. It generated complexity, richly organized patterns which were sometimes stable and sometimes unstable. The instability - and its products - have always fascinated me. Yet, as always, my teleological biases push their way to the surface.

In wanting to explain the "modernization" of family life, I have found myself driven back to a prior set of questions concerning the origins of the north-west European marriage system and the proximate determinants of fertility in a so-called "natural fertility population". These prior questions have been begged by the pioneering research in parish register demography, and I am reasonably sure that it will not be possible to find answers to such questions in the completion of more family reconstitution studies. To find such answers, we need a new way of seeing because: "you don't see something until you have the right metaphor to let you perceive it."

In parish register demography of the early modern period the time has clearly come to acknowledge the verity of Stephen Spender's point:

"Of course, the entire effort is put oneself

Outside the ordinary range
Of what are called statistics."

POST-SCRIPT

Since 1988, when I began to absent myself from the field of parish register demography, it seems that the number of completed family reconstitution studies has more than doubled. There are now more than two dozen and that creates a huge data-base with which one can turn from aggregated to dis-aggregated inter- and intra-generational analysis. But before concluding my remarks, let me digress at some length and then return to the main, revisionist point that I want to make in response to my earlier arguments.

Many, many years ago I met Jacques Dupaquier, who had succeeded Louis Henry to become the head of INED. Since the meeting took place in Canada - and Canada is officially bi-lingual - the conversation took place in French, which was not ideal for me! After some informalities, I asked him about the research focus of INED, now that they had assembled a very significant *tranche* of family reconstitution studies. Dupaquier told me that they were now interested in "historical biology". What did he mean? It turned out that he answered my puzzled query about "historical biology" with an example - just 15% of the French population in 1789 had descendants living in the 1980s. Then he said something like, "you know, all of the Tremblays in Canada are the descendants of one man's DNA, a man who arrived in Quebec in the 17th century." Being a bit bolshie, I replied that maybe Madame Tremblay's DNA yielded the more significant inheritance. This exchange sparked later thoughts and about two decades later, after co-authoring a paper on the peculiar nature of the fertility transition in Quebec, I presented the result of my thoughts to the Cambridge Group seminar which had been provoked in my discussion with Dupaquier. It should be noted that Roger Schofield was not in attendance – he had a meeting with an overseas visitor.

The essential - and relevant - point was that right-up-through the 1950s, marital fertility in Quebec was very much a bi-modal distribution with considerably-more than half of all births occurring in extremely-large families - the *chanteuse* Celine Dion is one of fourteen siblings;

the parents of former Prime Minister, Jean Chretien, had thirteen children, a veritable baker's dozen; and so on. Furthermore, the out-sized influence of this experience of *sur-fecondite* had led to a number of statistical misrepresentations: there was the widespread belief in French Canada that "*la revanche de berceau*" ("the revenge of the cradle") could lead to an assertion of francophone political-power while outside Quebec this misunderstanding underscored the Anglos' belief that French Canadians not only "bred-like-rabbits" but they did so for both cultural and political reasons, influenced by the hegemony of the clergy and, to a lesser extent, nationalist fire-brands. All of these misreadings of the demographic data were actually wrong - apart from the hyper-prolific minority, a sizeable proportion of French Canadian women never married (about 10% were *religieuse*) while the majority of women had between one and four children, which was pretty-much the same as the rest of the Canadian population in the mid-century. Moreover, there was no indication that the hyper-fecund were either ultra-montane devotees of the very conservative clergy or followers of francophone, *quebécois* nationalists.

But this is not the end of the story. If it was the seventeenth-century Tremblays who were the original carrier of the DNA mutation that would provide their descendants with *sur-fecondite*, lets take note that it was no longer contemporary Tremblays who possessed it. Indeed, the question is begged how Celine Dion or Jean Chretien or hockey stars like Marcel Dionne or Maurice "The Rocket" Richard or the family historian Gerard Bouchard were connected to this line of descent. All of these people came from very large sibling-groups. Clearly, Dupaquier had made a telling point but also a misleading one - the genetic pre-disposition towards *sur-fecondite* passed through both the male line of the seventeenth-century Tremblays, *pere et fils*, down through the succeeding generations but also through their daughters' lines of descent with the result that in the twentieth century, there were Dions and Chretiens and Dionnes and Richards and Bouchards whose family formations might have inherited this biological tendency towards almost-untrammelled "natural fertility". It should also be noted that Celine Dion has had three children but that all of them were the product of IVF - a strange testament to "natural fertility". Even more astonishing to me was the experience of Nathalie Becasseau and Michel Champoux - she was one of five children born in Burgundy in the 1960s; he was one of fifteen siblings born between the 1940s and the 1960s in the Eastern Townships. They married in the early 1980s and together they have had one child. Truly, a compass swing in natural fertility.

With this digression in mind, let's return back to the issue of CamPop's data bank. Back in the late 1970s, I was at Princeton for a term and that was when I first met Lawrence Stone. I had taken along with me the family reconstitution forms for Shepshed, Leicestershire, to work with during my term at the Davis Center. As many of you may know, the top-corner of an "FRF" had spaces in which one could enter literacy information. After the 1753 Marriage Act, all officiants were required to have the bride and groom sign the official documentation or to mark it. Roger Schofield's earliest work in historical demography had directly addressed the questions arising from this information. It struck me that this literacy data was never put to use - it was entered on the FRF but the computerized print-out ignored it. So, painstakingly, I decided to create new data-sets in which these bits of literacy information would be the independent variable and fertility/nuptiality/mortality were dependent variables. The results of this exercise proved to be both surprising and very interesting - it seemed that far from being a prized privilege, literacy was passed down haphazardly from generation to generation and that even between siblings the possession of a rudimentary ability to sign the register - or to mark it with an "x" or a "sign" - was similarly haphazard. When I presented these findings to the Davis Center seminar, which was led by Professor Stone, he was flummoxed because it clearly contradicted his claims that education in pre-industrial England was an important factor in promoting social mobility. But micro-level research based on family reconstitution among the proto-industrial villagers of Shepshed did not bear this out - these families of stocking-weavers seemed to have been quite relaxed about the supposed-importance of literacy. It seemed to me that literacy was both acquired (or not) and then passed on (or not) "for their own reasons".

The point here is that the data-set of 26 family reconstitution forms still has potential. If we switch focus from aggregated information about the standard demographic measures of birth/death/marriage and turn to considering questions of inter- and intra-generational experience then the tens of thousands of reconstituted families in that aggregated data-set provides some interesting research opportunities. Some examples: Questions could be asked about infant mortality - did sibling-sets who escaped death in infancy have the same luck with their own children? Similarly, with regard to sibling experiences of marriage - did children marry in order? Did brothers always marry older than their sisters? With regard to inter-generational nuptiality experiences: did older-marrying parents have older-marrying children? Did the experience of one generation's fertility have an "echo effect" in their

children's behaviour - did children from large sibling groups have high rates of fertility? Conversely, did children from small families also have low fertility? Or, how varied was the family formation behaviour of brothers and/or sisters?

Obviously, there are a great many more of these highly-intricate questions of intra- and inter-generational demography and family history that might give us further insight into the experience of family formation. This kind of micro-level analysis is unlikely to address "big questions" like the impact of proto-industry on standard demographic measures like nuptiality - or pre-marital pregnancy, marital fertility, and mortality. Yet these issues are not only important in their own right but also as a corrective to that kind of teleology which sees family formation changes in response to widespread social and economic transformation which occurred in the parish register period from 1538 to 1837.

And, I would be remiss if I didn't put on the record another second-order question that has always betrayed a kind of ignorance for me - to what extent did marital breakup and desertion have a demographic impact? For example: how many years of "potential child-bearing" were lost when husbands absconded? Were there observable demographic implications for children growing up in single-parent families? And, of course, was there an observable impact that abandonment had on the wives who had been left to fend for themselves and their children?

In a similar vein, we know that the peopling of the colonies by migrants from early modern England - overwhelmingly a group of young males - was significant in explaining the rise in female age-at-first-marriage in the later seventeenth century so the associated question must be asked: was there a clear-cut impact when young men left the agricultural villages of their birth and moved to find work in factories, mines, shipyards, and, in particular, deep-sea voyaging from which so many never returned. Did the young males search for new lives in the American and Caribbean colonies or the towns and cities of industrializing England have an "echo effect" depressing the marital chances of female villagers or was the rise of the urban bourgeoisie creating new opportunities in domestic service for them? And, when these young women entered domestic service - as opposed to service-in-husbandry studied by Ann Kussmaul, did their constricted lives permit them the time and/or opportunity for courtship away from the prying eyes of their employers? With thirty years of hindsight, it now seems to me that answering these kinds of questions requires turning the previous suggestions about inter- and intra-generational experience in another direction - to do so would mean that other

data sources such as poor law records might provide a way into such research. In that case, the demographic statistics of nuptiality/fertility/mortality would be dependent variables which could illuminate how large social and economic forces impinged on the experiences of women and men in early modern England.

So, in closing, please forgive me if I am out-of-the-loop - it was never my intention to step on anyone's toes in these concluding remarks. But it's now been about three decades since I was actively (or even passively) engaged with these materials. What I have presented are some points that might be deployed against my assertion in the 1988 paper that the future of parish register demography was desiccated. I'd like to believe that the situation has not been so hopeless.