Is There Evidence of Birth Control in Late Imperial China?

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> Mrs. King was an especially bright women, whose children were all educated, even up to college grade.
>
> *"How many children have you?"*
>
> "Six, four students and two daughters," replied the mother.
>
> *"Have you lost any?"*
>
> "No, I am particularly fortunate. I have not thrown any away."
>
> *"You mean that in your whole life you bore only six?"*
>
> "Oh, no, but the four who died of wind don't count."
>
> Jean Dickinson, in a North China village, 1928[1]

THE RED FLAG of revolt has been raised in the field of Chinese historical demography. It has been claimed—against the authority of T. R. Malthus, Ma Yinchu, R. H. Tawney, Walter Mallory, Warren Thompson, John Lossing Buck, George W. Barclay, Ansley Coale, Ho Ping-ti, and almost everyone else who has ever studied the Chinese family—that Chinese couples deliberately controlled their fertility. Wang Feng, James Lee, and Cameron Campbell claim to have found, in the genealogies of the Qing imperial lineage, evidence that "Qing nobles reduced their fertility through a combination of late starting, early stopping and—most significantly—long spacing."[2] On the basis of a study of Han bannerman in Liaoning, Wang Feng, James Lee, and Cameron Campbell claim that "Chinese fertility rates are so low that they are almost inconceivable without assuming widespread sexual restraint and/or the technology for family limitation."[3] And, after reanalyzing the frequently studied 1982 One-per-Thousand-Population Fertility Sur-

vey, Zhongwei Zhao claims that in China "spacing and stopping behavior was clearly affected by people's intentional control of their reproduction."[4]

These are important claims. I also think they are mistaken claims. I cannot disprove them in the space allowed, but I will try to show that the evidence offered in their support is of questionable relevance and open to alternative interpretations. More importantly, I will try to make clear what exactly is at stake and why it deserves close attention. I will give my opponents an advantage in the debate that is likely to ensue by referring to their view as the revisionist view and my own as the received view. Defenders of received views are always at a disadvantage because they can never claim to be original.

Let me be clear at the outset about what is and is not at issue. The revisionists and I agree that in China marriage was early and nearly universal for females; we agree that as compared with premodern Europe, marital fertility was moderate; that birth intervals were longer in China than in Europe; that in China the interval from marriage to first birth was particularly long; that marital fertility in China followed a natural fertility trajectory; that it was higher among the wealthy than among the poor; that the Chinese used sex-selective infanticide to regulate family size; that they used both male and female adoption for the same purpose; that most Chinese recognized the relationship between breastfeeding and child spacing; that they used this knowledge to achieve fertility goals; that they also manipulated age at marriage to achieve fertility goals; and, most importantly, we agree that their attitude toward reproduction was eminently rational. Chinese peasants were neither the passive pawns of culture nor the unwitting victims of custom.[5] Their disposal of children through infanticide and adoption was rational to the point of being ruthless.[6]

At this point the reader may doubt whether there is anything left to debate. There is and it is consequential. The empirical core of the issue can be appropriately phrased in terms of Han Fei-tzu's fourth-century claim that "people at present think five sons are not too many." Was this the case in late imperial China? Was it the attitude of most Chinese people? The revisionists claim that it was not. I argue that it was. I claim that far from limiting the number of sons reared, Chinese families made every effort to maximize the number. In the north they often took fully mature girls as wives for their preadolescent sons to ensure they would produce a grandson at the earliest possible age; in the south they commonly gave away daughters at birth because they recognized that breastfeeding delayed conception and thus reduced the number of sons they could bear; everywhere they decorated their weddings with symbols intended to encourage the birth of many sons; and everywhere they celebrated notable success in this regard at funerals and recorded it for posterity on tombstones.[7]

One qualification to my argument must be noted. In 1959, in the course of a study of child training in northern Taiwan, I discovered that most women

weaned their daughters two or three months earlier than their sons. When I asked them why, they replied, "It is for their own sake. The sooner you wean a girl, the sooner she will stop ovulating and be done with the dirty business of bearing children." This does not indicate that these women did not want several sons, but it does suggest that they did not want as many as their husbands. Although I doubt whether women's efforts to limit their fertility reduced the aggregate rate substantially, readers should keep their attitude in mind in evaluating the revisionists' arguments. It accounts for the fact that a close reading of Chinese medical texts suggests a considerable under-the-counter demand for abortifacients.[8]

I call the revisionists' argument revolutionary because the implications drawn challenge long-accepted views of both traditional and contemporary China. Zhao suggests that the remarkable success of China's birth control program is only partly attributable to determined government intervention.[9] The government succeeded, he contends, only because Chinese families were not strongly pronatalist and were long accustomed to deliberate birth control.[10] This is a bold thesis but decidedly modest compared to the implications drawn by Lee and Wang, who conclude that almost everyone has been deluded by something they call "Malthusian mythology." The victims of this delusion include, in addition to a long list of eminent scholars, Mao Zedung, Hua Guofeng, and Deng Xiaoping. They were deluded into thinking that "one of the most draconian family planning policies in world history" was necessary to save China from an imagined Malthusian disaster. They are accused of having "launched the largest family planning program in the twentieth century on the basis of little more than nineteenth-century social theory."[11]

The evidence the revisionists offer comes almost exclusively from three sources—the genealogy of the Qing imperial lineage for the years 1700–1840, household registers compiled in a village in Liaoning in the years 1774–1873, and 30,000 of the women included in the Chinese government's 1982 One-per-Thousand-Population Fertility Survey. This last group were all women born in the years 1914–30 who were living in rural areas at the time of the survey. Their experience can be accepted as representative of rural women, *but we cannot assume that histories compiled as long as 50* years after the fact are entirely accurate. Jean Dickinson's Mrs. King was not unusual in failing to "count" all the children she had borne. In 1980–81 I interviewed 580 women born during the same years as the women included in the sample Zhao drew from the 1982 survey.[12] I interviewed only four women per day and was assisted by a local woman who was known to all of my informants and who knew most of their children. But I still missed recording births, even the births of living children. At the end of each interview I read back to my informant the list of births she had reported and asked her whether I had recorded the list correctly. Once, after the informant had assured me the list was correct, a young woman who had been

standing at the door during the interview interrupted laughingly, "You forgot me, mother."

The Qing imperial genealogies can be accepted as accurate because of their social importance, but the people included cannot be accepted as representative of the Chinese population. For one thing, the subjects of the genealogies were all urban residents, while the great majority of the Chinese lived in villages. For another, they were noblemen who lived on state emoluments, while the great majority of the population were farmers and laborers who worked for a living. And for yet another, the subjects of the imperial genealogy were not Chinese. They were Manchus.

The subjects of the Liaoning household registers were members of the Han Army Eight Banner living in Daoyi village near Shenyang City. They were representative of the larger population in that they were ethnically Chinese and earned their living as farmers, but their land was state owned and their social status that of state servants. According to Lee and Campbell, "Adult males were...not only liable for military conscription, but also for other labor services as well." A more serious problem is that the Liaoning registers were not carefully maintained. Lee and Campbell estimate that "roughly one-third of male births and two-thirds of female births were never registered," and these estimates are based on the assumption that all the males who survived to age 16 *sui* were registered.[13] The figures could be much higher. The state's demands for corvée labor were considerable at the beginning of the study period and "increased dramatically over time."[14] Thus it seems likely that some young men avoided registration by bribery or flight.

As mentioned earlier the revisionists' argument begins with the claim that Chinese fertility was so low that one must assume "widespread sexual restraint and/or the technology for family limitation." The argument is persuasive if one takes the behavior of the Qing nobility as representive of the population of China. Monogamous nobles who survived to age 45 years had only 4.5 children, and polygynous nobles had only 6.5 children.[15] But if one focuses on the behavior of the commoners, who made up all but a tiny fraction of the population, the argument is not persuasive. Lee and Campbell's adjusted marital fertility rate for the Han bannerman in Liaoning is 6.3,[16] and the rate for the older women included in the 1982 fertility survey is 6.7.[17]

These rates do not demand birth control as an explanation, and it is likely that they considerably underestimate Chinese fertility. The true rate was at least 7.4 live births per woman, which was the figure for the 71 women included in Dickinson's study of Jianyang.[18] In 1931 Chiao Chi-ming and Warren Thompson set up a registration system in Jiangyin County in Central Jiangsu and maintained it for four years. They obtained a total marital fertility rate of 7.41.[19] My 1980–81 survey of women who were then aged 55 years and older produced a rate of 7.03.[20] The most reliable data for any

early-twentieth-century Chinese population are those available in the household registers maintained by the Japanese colonial government in Taiwan. For 11 widely scattered rural communities, they give total marital fertility rates of 7.61, 7.78, 8.45, 8.47, 8.20, 7.23, 6.21, 7.93, 7.40, 7.58, and 7.87.[21]

The primary source for estimating Chinese fertility prior to 1900 is lineage genealogies. The pioneer in this research, Liu Ts'ui-jung, has studied eight genealogies covering most of the Ming/Qing period. Adjusted to take account of infant and early childhood mortality, her work produces total fertility rates of 7.48 and 6.85 for two Zhejiang lineages in the years 1725–1829, 8.76 for a Taiwan lineage in 1750–1849, 7.76 for a Jiangsu lineage in 1517–1877, 8.17 for an Anhui lineage in 1462–1864, 8.12 for a Hubei lineage in 1627–1912, 8.01 for a Hunan lineage in 1296–1864, and 6.74 for a Guangdong lineage in 1435–1869.[22] Even higher rates are reported by Ted Telford on the basis of his intensive study of a large number of Anhui genealogies covering the years 1520–1661. The average fertility rate for the smaller lineages included in his study was 8.35. The rates for the larger lineages were 8.13, 7.57, 7.24, 6.78, 7.92, 8.49, 9.56, 7.85, 7.15, 7.78, 7.55, 8.15, 8.82, 8.03, 7.87, 8.13, 5.77, 7.78, 7.01, 7.45, 7.78, 8.44, 6.77, 8.67, 7.03, 7.62, 8.55, 9.11, and 9.19.[23] Three of these rates are double the rate the revisionists report for monogamous Manchu nobles.

The revisionists are right in insisting that marital fertility was lower in China than in parts of Western Europe and very much lower than in such exemplary populations as the Hutterites. The reason, they say, was birth control. The reason, I say, was poverty. The fact that Chinese women bore 7 or 8 children rather than 10 or 11 is not evidence of birth control. It is evidence of chronic malnutrition, untreated diseases, hard manual labor, *and economically enforced conjugal separation. One of my 1980–81 field* sites was a cluster of small villages on the east bank of the Grand Canal opposite Yangzhou. The 103 women I interviewed there included 54 who had borne fewer than 7 children even though their marriages endured through age 45. I asked each of them why they had not borne more children. Several had no explanation that they were willing to share with a stranger. The others gave the following explanations:

Five children. Husband absent between ages 22 and 28. Stopped menstruating at age 37. Suffered from yellow fever.

Two children. Contracted hepatitis at age 25 and could not afford treatment. Stopped menstruating a year or two later.

Four children. One miscarriage. Husband had a venereal disease.

One child. One miscarriage. Suffered from yellow fever and stopped menstruating at age 37.

Five children. One miscarriage. Began using native oral contraceptive in 1958 at age 33.

Six children. One miscarriage. Stopped menstruating at age 35.

Three children. Not allowed to marry until age 27 because parents needed her help.

Six children. Husband moved to Shanghai and took a second wife when she was 35.

Four children. Suffered metrorrhagia (uterine bleeding) at age 31 after the birth of her last child.

Two children. Did not begin menstruating until age 30 and stopped at age 38.

Four children. Husband fell seriously ill when she was 25 and never recovered.

One child. Husband had another wife in Yangzhou and never came home.

Five children. Contracted hepatitis at age 31 and did not menstruate again.

Three children. One stillbirth. Husband worked in Shanghai and seldom returned home. Did not see him at all after age 33.

Three children. One miscarriage and metrorrhagia at age 25.

Six children. Four miscarriages and "female troubles" after age 40.

Four children. One miscarriage and one stillbirth. Stopped menstruating at age 29.

Four children. One miscarriage and one stillbirth. Stopped menstruating at age 40.

Four children. Contracted hepatitis at 35 and stopped menstruating at age 36.

Five children. Said she did not want the children she had but insisted that she had never used any form of birth control.

Five children. Two stillbirths. Remained unmarried for five years after her first husband died.

Six children. One stillbirth and one miscarriage. Began using some form of birth control in 1957.

Four children. Stopped menstruating at age 28.

Six children. Two miscarriages.

Six children. Had an abortion in 1959 and husband began using condoms in 1961.

Six children. Abortions in 1963 and 1964 and was sterilized during a campaign in 1965.

Five children. Abortions in 1958 and 1959 and began using a uterine coil in 1960.

Four children. Three stillbirths and one miscarriage.

Three children. Suffered from hepatitis and did not menstruate after age 33.

Five children. Stopped menstruating for two years at age 33 and did not menstruate again after age 38.

Three children. One miscarriage. Highly irregular menstrual cycle between ages 19 and 30 and stopped completely at age 37.

Six children. Three miscarriages. Very irregular menstrual cycle after age 32.

Six children. An abortion in 1966 and suffered from hepatitis after age 38.

Six children. One stillbirth.
Six children. Two miscarriages.
Five children. Three miscarriages.
Three children. Husband was a tailor and spent most of his time in Shanghai.

Five of these women had their childbearing careers cut short by the birth control programs initiated in the late 1950s. The others bore fewer than 7 children only because of illness or conjugal separation. Six of the 37 women who answered my questions had stopped menstruating before age 35 and 12 before age 40. I attribute this to poverty and suggest that the revisionists have mistaken its effects for birth control.

In 1935 the prominent eugenicist Herbert D. Lamson undertook to determine whether the reproductive behavior of the Chinese elite was dysgenic. Lamson wanted to know whether the Chinese upper classes, like their Western counterparts, had allowed their fertility to fall below that of the lower classes. What he found was that "in China the upper economic classes have more children per family than the lower economic classes."[24] Although no one now accepts Lamson's conclusion that this behavior was eugenic, his finding has been confirmed again and again. I have shown that in Taiwan the fertility of rich peasants exceeded that of poor peasants and laborers,[25] and Stevan Harrell has shown that in Zhejiang the fertility of a lineage's rich branches exceeded that of its poor branches.[26] The most recent evidence for Lamson's generalization comes from Lee and Campbell's own study of Han bannermen in Liaoning. They found that acquiring a position in the banner occupational hierarchy raised a man's fertility substantially.[27]

Why did fertility rise with social class? It is a difficult question for the revisionists because it forces them to argue that birth control was more common among the lower classes than among the upper classes. They have to maintain that despite the example of their social superiors and despite being illiterate and impoverished, the great mass of landless laborers and rent-racked farmers practiced effective birth control. Moreover, they have to accept that people who could afford to hire labor made little or no effort to control their fertility, while people who were dependent on their children for labor and support in old age limited their fertility. And, worse yet, they have to assume that although having numerous sons threatened to fragment their estates, the upper classes did not practice birth control, while even though they had no estates to preserve, the lower classes did practice birth control.

The revisionists counter these arguments with evidence purporting to show that however implausible it may seem, most Chinese couples employed one or more of three fertility-limiting strategies. They call the first of these "late starting." The argument is not, as one might suppose, that Chinese couples delayed marriage. Instead, the revisionists argue for the very unlikely possibility that, after marrying at an early age, many couples delib-

erately delayed having children for as long as three or four years. For example, Lee and Campbell write: "...in Daoyi many young couples started childbearing late, delaying their first birth until well after their marriage. Whereas in most European populations the mean interval to first birth was less than 2 years, in Daoyi it was almost 4."[28]

I do not dispute the revisionists' claim that in China the interval between marriage and the first birth was considerably longer than it was in Europe. The figure of 35 months for the women I interviewed in 1980–81 is lower than the figures from the imperial genealogy and the 1982 fertility survey but still much higher than the typical European figure. What I dispute is the claim that the first birth interval was deliberately delayed. Chinese families were distraught if a newly acquired daughter-in-law did not conceive within a year of marriage. They pasted fertility-inducing charms over the bridal bed; they made offerings to one or more fertility-giving deities; they hired a shaman or fortune-teller to locate and remove fertility-blocking fates; they induced the bride to drink fertility-enhancing potions; and, if all else failed, they adopted a daughter in the hope that this would induce a pregnancy. In northern Taiwan nearly a third of all women adopted their first child for this reason.[29]

My explanation of the long first birth interval is that it was the result of early marriage, late menarche, a long period of adolescent subfecundity, and a high miscarriage rate among immature mothers. Because of the extreme deprivations suffered by many Chinese children, particularly female children, menarche was very late by contemporary standards. Data collected in Taiwan, Fujian, and Sichuan suggest an average age between 16 and 17 years.[30] Given, then, that a late age at menarche is usually accompanied by a prolonged period of adolescent subfecundity,[31] the great majority of Chinese women married before they were fully capable of conceiving. The best evidence of this is Wang Feng and Yang Quanhe's demonstration that as age at marriage rose and living conditions improved, the mean length of the first birth interval declined—from 34 months among women married in the 1950s to less than 18 months among women married in the early 1980s.[32] Wang and Yang take this as evidence of a sexual revolution occasioned by increased freedom of choice in marriage. I attribute it to a social revolution that finally ameliorated the extreme poverty that both delayed menarche and promoted a long period of adolescent sterility.

The second fertility-limiting strategy identified by the revisionists is "early stopping." They claim that Chinese couples stopped bearing children at a much younger age than European couples and did so deliberately. The problem in this case is that the evidence they offer in support of their claim is contradictory. Wang, Lee, and Campbell give 33.8 as the mean age at last birth for the monogamous wives of the imperial lineage and 34.1 as the mean age for the lineage's polygynous wives.[33] Lee and Campbell report

that "the mean age at last birth in Daoyi was 35 *sui,* which is only 33.5 Western years of age."[34] But Zhao reports 38.2 as the mean age at last birth for the 30,000 women included in the 1982 fertility survey.[35] Because this figure is close to those I get from my 1980–81 survey and the Taiwan registers—38.0 and 36.6—I conclude that the figures for the imperial lineage and the Liaoning bannermen are either biased or unrepresentative.[36]

A mean age of 38 at which Chinese women bore their last child does not entirely refute the revisionists' argument. They are right in stating that "[s]tudies of historical populations in Europe have repeatedly demonstrated that the mean age at last birth...was usually within one year of age 40."[37] Why, then, did Chinese couples bear their last child a year or two earlier than European couples? There are, I argue, two possibilities, both of which are more likely than a deliberate decision to forgo childbearing. The first is suggested by William H. James's observation that "coital rates are much more closely related to duration of marriage than to age."[38] This is relevant because by age 30 most Chinese couples had been married twice as long as their European counterparts. Thus the fact that they bore fewer children in their later years is to be expected and does not stand as evidence of deliberate stopping. The second and more likely possibility is that because of their extreme poverty, older Chinese couples were less healthy and less vigorous than their European counterparts. The 103 women I interviewed in my Yangzhou site included 39 who bore a child at or before age 38 but not after. They included seven who had no explanation they were willing to share with me, six who had had an abortion or began using contraception in the late 1950s, and six who were permanently separated from their husband. The remainder included one woman whose husband was seriously ill most of his life, two who were themselves seriously ill, one whose husband had contracted a venereal disease, two who suffered more than one miscarriage after age 38, two who suffered from metrorrhagia in their 30s, two whose menstrual cycle was markedly irregular most of their lives, and ten who stopped menstruating before age 38.

The northernmost of my 1980–81 field sites was a village on the banks of the Qinghe River in Lichuan County in Shaanxi. The 84 women I interviewed there included 31 who had borne a child before age 38 but not after. Although none of these women had suffered from ailments common in south China, such as hepatitis and yellow fever, the reasons they gave for not bearing a child after age 38 were much the same as those given by my Yangzhou informants. The sole novel explanation was given by a woman who said that she bore only two children because her second child was born just as the sun set. The 21 women who answered my questions included one who had had an abortion in 1961, one who was totally blind after age 30, one whose husband had deserted her, one who was seriously ill most of her adult life, one who had experienced a stillbirth at age 41,

four whose menstrual cycles were markedly irregular for years at a time (including one woman who claimed to have cycled every four or five days), and 11 who stopped menstruating before age 38. Two of these women stopped menstruating before age 30 and seven before age 35.

The third fertility-limiting strategy identified by the revisionists—and in their view the most significant of the three—is "long spacing." I agree with their basic claim that in China birth intervals were longer than in Europe, but again there is a problem with the magnitude of the difference. Wang, Lee, and Campbell claim that in England and France intervals between first and second births were "only one-half to two-thirds" the length of those of the imperial lineage,[39] and Lee and Campbell claim that intervals in the same two countries were "only one-half as long" as those of the Liaoning bannermen.[40] Since these authors take 24 months as the average European birth interval, their claims put the Chinese interval at 48 months. Such long intervals may have been typical for Manchu nobles and Han bannermen, but they were not typical for the great mass of Chinese commoners. Zhao calculates an average birth interval of 38.8 months for the elderly women questioned in the 1982 fertility survey, and this includes marriage-to-first-birth intervals as well as the last interval.[41] I found an average interval of 33.2 months among the women I interviewed in 1980–81 and an average of 33.4 months among Taiwanese women who bore children in the years 1906–45. Thus the evidence says that birth intervals of 48 months were atypical. The typical Chinese birth interval was less than 36 months.

Birth intervals of 36 months, like total fertility rates of 7 or 8, do not stand as prima facie evidence of birth control, but neither do they exclude birth control. At 36 months Chinese intervals were still half again as long as those of Europeans. The revisionists are willing to attribute part of the difference to the fact that Chinese mothers weaned their children much later than European mothers, but they insist that their results "largely rule out" breastfeeding as an adequate explanation of the length of the Chinese intervals.[42] The explanation they prefer is deliberately reduced coital frequency. The explanation I prefer is some combination of poor health, inadequate nutrition, heavy labor, and poverty-induced spousal separation. The fertility histories of the women I interviewed in 1980–81 include 217 intervals of five or more years. In 118 instances I requested and received an explanation of why the couple had gone so long without producing a child. In only five of the 118 instances was the reason given some form of birth control, and in four of these five the couple had started using birth control only in the late 1950s or early 1960s. The one exception was a woman who said she and her husband had stopped sleeping together. The other explanations given were: away from home begging (3), husband in the army (2), husband away from home working (9), organic illness (16), mental illness (4), husband's illness (2), irregular menstrual cycle (25), one or more stillbirths (8), and one or more miscarriages (44).

My most complete data come from my field site in Dayi County in Sichuan. My informants there provided explanations for 32 of 45 birth intervals of five or more years. One told me that she had begun using an oral contraceptive in 1961. This was the only reference to any form of deliberate birth control. The other explanations were husband absent for two or three years (3), de facto separation as the result of a marital quarrel (1), stopped menstruating (1), irregular menstrual cycle (4), serious illness and irregular menstrual cycle (4), and one or more miscarriages (17).

Even if it were shown that the relatively long birth intervals found in China were the result of deliberate sexual restraint, this could not be taken as evidence that Chinese couples wanted to limit the number of children reared. It could just as well stand as evidence of a desire to rear as many children as possible. In China as elsewhere, the probability of a child's surviving the early years of life was a consequence of the length of the intervals preceding and following its birth. The longer the intervals, the better his or her chances of survival. We must assume that most Chinese couples recognized this and we must accept that if they reduced coital frequency as a means of spacing their children it was restraint in the service of a maximizing, not a moderating strategy. Regardless of the way in which wide spacing was achieved, it was not a form of birth control. It was a form of mortality control. Chinese couples could not afford to bear children at the European pace. The women I interviewed in 1980–81 had experienced 979 pregnancies following a European birth interval of 24 months or less. Forty-five percent of these pregnancies ended in a miscarriage, a stillbirth, or a death in infancy or early childhood. The comparable figures for 1,024 pregnancies with intervals of 25–36 months and 1,050 pregnancies with intervals of more than 36 months were 34 percent and 19 percent.

The revisionists' claim that Chinese couples employed late starting and long spacing to limit their fertility is even more radical than their claim that they practiced deliberate birth control. It challenges received wisdom in demography as well as sinology. Ever since Louis Henry's influential definition of "natural fertility" appeared in 1961, deliberate fertility control has been equated with early stopping. Demographers recognize that such social practices as postpartum sex taboos may reduce fertility, but they do not accept these practices as deliberate fertility control. In Henry's words, "Control may be said to exist when the behavior of the couple is bound to the number of children already born and is modified when this number reaches the maximum the couple does not want to exceed."[43]

The view of fertility control expressed in Henry's definition has been confirmed repeatedly in studies of such diverse populations as the eighteenth-century Genevan bourgeoisie and twentieth-century Thai peasants. The reason is obvious. People would not control their fertility if they did not have a clear idea of how many children they wanted. It is therefore nearly inevitable that deliberate fertility control will be "bound to the num-

ber of children already born." Late starting and long spacing are not found as the means of deliberate fertility control, because they are too risky. Why would a couple risk failing to achieve their goal because of unanticipated illness or death? Why would they delay unnecessarily reaping the benefits children would bring them? Why would they forgo until late in life the advantages of having additional labor at their disposal? Why would they take the chance of an accident's depriving them of children when they were too old to replace them? The revisionists' argument makes no sense if we assume—as they and I do—that Chinese couples were rational and risk averse.

This brings me to the most basic of my objections to the revisionists' position. The assumptions needed to maintain their argument make Chinese people look like an exception to what nineteenth-century social theorists called "the psychic unity of mankind." It is not just that the revisionists' claims about late starting and long spacing make Chinese people look irrational and risk prone. Their claims can only be maintained by assuming that the Chinese were a peculiarly asexual people. Wang, Lee, and Campbell believe that "the low fertility and long birth intervals of imperial parents were, at least in part, the result of their ability and even willingness to regulate coital frequency." They accept as a fact about Chinese behavior an elite "medical" recommendation that "coital frequency should be no greater than three times a month for young adults, less than twice a month for middle-aged adults, and once a month at most for the elderly."[44]

The revisionists' assumptions about Chinese sexuality are most starkly apparent in their claim that "late starting" was a form of birth control. To maintain this claim they have to assume that despite the promptings of propinquity and a rising tide of hormones, Chinese newlyweds refrained from sexual intercourse for as long as three or four years. It is true that in China a young woman began her married life in what Wang and Yang characterize as "a strange and often forbidding environment."[45] This may have inhibited her for a few weeks, but can we believe that the effect endured a few years? And what about the groom? What inhibited him? The assumption that Chinese sexuality was easily inhibited is contradicted by universal prostitution and an illegitimacy rate that far exceeded the European rate. The only solid evidence on the fertility of Chinese widows indicates that they bore almost as many children as married women of the same age.[46]

The revisionists recognize that, as aggregate facts, late starting, early stopping, and long spacing are not sufficient to make their case. There are too many alternative explanations of the kind I have just outlined. They therefore include in support of their argument comparisons of the behavior of women with different fertility histories. These tests of the birth control hypothesis all follow the same logic. They begin with an assumption—never tested independently—that women with certain fertility histories will be more motivated to control their fertility than women with other histories.

These groups of women are then compared and a difference in the predicted direction is taken as evidence of birth control. Conspicuously missing is any attempt to formulate and test alternative explanations of the differences.

I have extracted from the revisionists' writings 13 such tests of the birth control hypothesis. They are listed below and numbered to make a complex argument as clear and succinct as possible.

1. Birth intervals were longer among women who had borne at least one son than among women who had not borne a son.

2. Birth intervals were longer among women who had borne only males than among women who had borne only females.

3. Age at last birth was earlier among women who had borne only males than among women who had borne only females.

4. An additional birth was less likely among women who had borne only males than among women who had borne only females.

5. Age at last birth was earlier among women whose surviving children were all males than among women whose surviving children were all females.

6. Birth intervals were longer among women who had borne both male and female children than among women who had borne only males or only females.

7. Age at last birth was earlier among women who had borne both male and female children than among women who had borne only males or only females.

8. An additional birth was less likely among women who had borne both male and female children than among women who had borne only males or only females.

9. Age at last birth was earlier among women whose surviving children included both males and females than among women whose surviving children were all males or all females.

10. Birth intervals were as long among women whose last child died within a month of birth as they were among women whose last child survived.

11. Age at last birth was earlier among women whose children all survived than among women whose children all died.

12. Age at last birth was earlier among women whose first surviving son was born early than among women whose first surviving son was born late.

13. An additional birth was more likely among women whose first surviving son was born early than among women whose first surviving son was born late.

The first four propositions can be treated as a group. They share the assumption that women with sons were less likely to want more children than women without sons. The evidence offered in support of the first proposition consists of Wang, Lee, and Campbell's finding that in the imperial lineage "subsequent birth intervals for parents at early parities who had not

yet given birth to a son were shorter by three to four months than those for parents who already had a son."[47] The evidence for propositions 2–4 comes from Zhao's reanalysis of the 1982 fertility survey and is reproduced in Table 1. The differences are all in the predicted direction but are not large. The differences in age at last birth average .52 years; the differences in birth intervals, 1.34 months; and the differences in the probability of another birth, .024 times.[48]

One objection to the revisionists' claims regarding propositions 1–4 is that they take no account of one of the most obvious facts about late imperial China—namely, that girls were discriminated against. They were often killed at birth; they were commonly neglected and died as a result; they were frequently sold as servant slaves; they were often given away as "little daughters-in-law"; and they were usually weaned two or three months earlier than their brothers.[49] Thus a slightly later age at last birth, a slightly stronger tendency to bear another child, and slightly shorter birth intervals are all expected among women who have borne only girls. It is simply because their last-born child was a girl. The evidence shown in Table 1 does not say that Chinese couples practiced deliberate birth control. It says that regardless of how many children they had, Chinese couples wanted more sons and consequently sacrificed their daughters.

A more fundamental objection to the claims made on the basis of the evidence shown in Table 1 is that the argument assumes the conclusion. It assumes that Chinese couples were motivated to control their fertility and therefore sees anything less than maximization as deliberate fertility control. A far more plausible assumption is that Chinese couples hoped to produce several sons and became anxious when it looked as if they might not

TABLE 1 Mean age at mothers' last birth, length of last birth interval (months), and parity progression ratio by number and sex of children ever born

Number of children ever born	Age at last birth		Length of last birth interval		Parity progression ratio	
	Males only	Females only	Males only	Females only	Males only	Females only
One	38.0	38.4	38.7	37.3	.958	.968
Two	38.4	39.0	39.5	38.5	.953	.967
Three	38.9	39.5	39.0	37.3	.927	.944
Four	39.4	39.9	39.1	37.5	.885	.920
Five	39.9	40.4	37.5	36.5	.825	.872
Six	40.5	41.1	36.7	35.2	.764	.866
Seven	40.7	42.0	38.1	33.6	.724	.760
Eight	41.4	41.7	36.1	33.1	.583	.686

SOURCE: Zhao, "Deliberate fertility control," cited in note 4, Figures 2, 3, and 4, pp. 739, 741, and 743.

succeed. On this assumption the differences documented in Table 1 are not attributable to fertility control among couples with several sons. They are attributable to anxiety-driven reproduction among couples who are still without sons. The couples with sons were not practicing birth control. They had just relaxed a little.

The test proposed by proposition 5 is based on the same assumptions as the test proposed by propositions 1–4. The difference is that in this case women are categorized in terms of surviving children borne before age 30 rather than all children ever borne. Discounting children who died early is important because if the revisionists are right, the predicted difference should be substantially larger than those shown in Table 1, the comparison between those who might and might not be motivated to control their fertility being more tightly controlled. Thus it is embarrassing for the revisionists' case when Zhao finds that the mean age at last birth was 39.1 among women with surviving sons but no surviving daughters and 39.4 among those with surviving daughters but no surviving sons.[50] The difference is only three months, which is less than when the comparison is made in terms of children ever borne.

Propositions 6–8 assume that Chinese couples wanted both sons and daughters and therefore predict that couples who had achieved what Zhao calls "the perfect family" were more motivated to control their fertility than those who had borne only sons or only daughters. In light of what has already been said, the critical comparison is between couples with only sons and those with both sons and daughters. This comparison, again based on Zhao's reanalysis of the 1982 fertility survey, is shown in Table 2. It rejects proposition 7 conclusively. At no parity is there any difference between the

TABLE 2 Mean age at mothers' last birth, length of last birth interval (months), and parity progression ratio by number and sex of children ever born

Number of children ever born	Age at last birth		Length of last birth interval		Parity progression ratio	
	Males only	Males and females	Males only	Males and females	Males only	Males and females
Two	38.4	38.6	39.5	39.7	.953	.957
Three	38.9	39.0	39.0	39.8	.927	.927
Four	39.4	39.4	39.1	40.2	.885	.875
Five	39.9	39.9	37.5	39.7	.825	.803
Six	40.5	40.3	36.7	38.4	.764	.720
Seven	40.7	40.8	38.1	36.7	.724	.664
Eight	41.4	41.2	36.1	35.9	.583	.572

SOURCE: Same as Table 1. The figures for "males and females" are averages of the figures given by Zhao for women who had borne at least one son and at least one daughter.

average age at last birth for the two classes of women. Propositions 6 and 8 fare only slightly better. At parities two and three there is no difference in either birth-interval lengths or parity progression ratios. Differences appear at parity four, but they are very small. Substantial differences appear at parities five and six but do not remain substantial through parities seven and eight. At these later parities the birth intervals of couples who had not achieved the perfect family were actually longer than those of couples who had.

Proposition 9 is based on the same assumptions as propositions 6–8, but, like proposition 5, it compares women in terms of surviving children borne before age 30. This matters because while the data offered in support of the proposition fit the revisionists' prediction, they do not support the birth control hypothesis. Zhao's data suggest that women whose surviving children borne before age 30 include sons but no daughters bore their last child at 39.1, while women whose surviving children borne before age 30 include both sons and daughters bore their last child at 38.7.[51] What is most striking about these data is not that women who had borne both sons and daughters stopped bearing children five months earlier than those who had borne only sons. Most striking is that women with both sons and daughters continued bearing children eight or nine years after they had achieved the so-called perfect family. This can only be interpreted to mean that birth control was either uncommon or ineffective.

A general objection to the arguments represented in propositions 6–9 is that they take no account of male adoption. Chinese families rarely sold or gave away first- or second-born sons, but they commonly surrendered third-, fourth-, and fifth-born sons. In northern Taiwan the probability of out-adoption among boys born in the years 1906–10 to families with three or more surviving sons was .173.[52] Thus the fact that at any given parity families with only sons had more sons than families with both sons and daughters is probably adequate to account for the small differences offered in support of propositions 6–9. Giving away or selling a last-born son in infancy would inevitably shorten the next birth interval and thereby raise both maternal age at last birth and the parity progression ratio.

Proposition 10 assumes that if there is little or no difference between the birth intervals of women who do and not do breastfeed, this is attributable to deliberate spacing by those who do not breastfeed. The problem with the evidence offered in support of the proposition is that it is drawn entirely from the genealogies of the Qing nobility. I concede that the facts of the matter are as reported. What I do not concede is that they say anything about the practice of birth control among Chinese women who were not noblewomen. All the facts really say is that the Qing nobility was an extraordinary population. Among all other Chinese populations—as among all other peasant populations worldwide—an infant death reduced the length of the succeeding birth interval by several months. My data show the re-

duction to be 6.6 months among Taiwanese women in 1906–45 and 6.4 months among the mainland women I interviewed in 1980–81.[53]

Proposition 11 assumes that Chinese women whose children have all died were not motivated to control their fertility. This is undoubtedly true. The question is whether or not women whose children have all survived were motivated to control theirs. According to the data reported in Table 3 they were not. They may have relaxed a little after three or four successful births, but they did not deliberately curtail their childbearing. We can be certain that the next-to-last child of one group of women died and that the next-to-last child of the other group survived. Thus the minuscule differences between the two groups' mean ages at last birth are attributable to the fact that one group's last birth was delayed by breastfeeding while the other group's last birth was not so delayed.

The last two propositions in my list assume that the earlier a woman bears a son who survives, the more motivated she will be to practice birth control. Zhao tests proposition 12 by comparing the mean ages at last birth of women whose first sons were born at ages 15–19, 20–24, and 25–29. His findings are 38.5, 38.7, and 39.1.[54] The test of proposition 13 controls for birth order and therefore produces more complicated results. These are reproduced in their original form in Table 4. Note that the parity progression ratios reported in this table are "for women after age 30." I interpret this to mean that women in the upper left hand corner of the table bore their first child before age 20 (the son who survived) and then did not bear a second child until after age 30, while women in the next level of the same column bore their first child (again the son who survived) between ages 20 and 25 and then did not bear another child until after age 30.

The differences in age offered in support of proposition 12 are so small that they are very likely attributable to one of several uncontrolled variables. Only the data supporting proposition 13 deserve serious attention. The predicted differences are large and regular and would provide strong support for the revisionist position if they could be taken at face value. The

TABLE 3 Mean age at mothers' last birth by age at first birth and survival of children

	First child		First 2 children		First 3 children		First 4 children	
Age at first birth	Died	Lived	Both died	Both lived	All died	All lived	All died	All lived
19 and younger	37.8	37.3	38.2	37.7	38.5	38.1	38.7	38.6
20–24	38.6	38.2	38.9	38.5	39.4	38.9	39.8	39.4
25 and older	39.5	39.0	40.4	39.8	40.3	40.5	41.4	41.3

SOURCE: Zhao, "Deliberate fertility control," cited in note 4, Table 3, p. 745.

TABLE 4 Parity progression ratios for women after age 30, by age at birth of first surviving son

Age at birth of first surviving son	1st to 2nd birth	2nd to 3rd birth	3rd to 4th birth	4th to 5th birth	5th to 6th birth	6th to 7th birth
19 and younger	.797	.705	.546	.413	.289	.229
20–24	.874	.728	.563	.415	.305	.258
25–29	.889	.730	.578	.417	.315	.283
30 and older	.915	.797	.618	.458	.371	.331

SOURCE: Zhao, "Deliberate fertility control," cited in note 4, Table 6, p. 748.

problem is again that there are alternative explanations. Did the women included in Zhao's sample report to the census taker the children they gave away, sold, or otherwise disposed of at birth? Very possibly not. Like Mrs. King, they probably thought such children didn't "count." Thus it is all too likely that what the data really say is that the earlier a woman bore a son who survived, the more children she disposed of by adoption, sale, or infanticide.

Another possibility is created by Zhao's failure to control for age at marriage. This allows for the influence of what he calls "natural or unintentional factors." We know that the women at the top of each of the six columns shown in Table 4 married at an early age because they all bore a son at or before age 19. We do not know when the women lower down in the six columns married, but we can be certain that on average they married later than the women at the top of the columns. Thus it could well be that the likelihood of another birth rises as one ascends the columns because the marriages lower down were contracted when the women were older than was the case in the marriages higher up. The decline in frequency of intercourse found among marriages of long durations cannot be considered a deliberate form of birth control. It is entirely natural and largely unintentional.

A third possibility is that the women Zhao's criteria place high in the columns are less fecund than those placed low in the columns. This is likely because the data show that the women placed high in the columns have fewer children per year of marriage than the women placed low. Compare the women represented in the "19 and younger" and "25–29" classes in column 1. We know that the women in the first group married very young (no later than 18 and probably as early as 16 or 17), bore their first child (the boy who survived) before age 20, and then did not bear their second child until after age 30. We do not know when the women in the 25–29 group married, but can safely assume that it was later than the women in the 19 and younger group. Thus the fact that these women bore only one child before age 30 (the child who survived) means that on average they

bore fewer children per year of marriage than the women in the 19 and younger group.

In sum, then, tests of the 13 propositions I have extracted from the revisionists' argument fail to make their case. The data offered reject propositions 7 and 12, provide insubstantial support for propositions 5, 6, 8, 9, and 11, and fail to address alternative explanations in the case of propositions 1, 2, 3, 4, and 13. The only proposition for which the revisionists provide substantial evidence for which there is no obvious alternative explanation is proposition 10. My objection in this case is that the evidence comes entirely from the genealogies of the Manchu nobility. The proposition finds no support when tested among Han commoners.

I will report in later publications my own tests of the birth control hypothesis. There is space here for only one such test, but it is indicative of what is to come. I contend that most Chinese couples wanted as many sons as possible. The revisionists contend that far from trying to maximize the number of sons, they often took measures to limit fertility. To test these competing hypotheses I calculated from my 1980–81 interviews the average length of subsequent birth intervals among women with three or more surviving sons and compared this with the average birth interval of all women aged 30 and older. The comparison is biased against my hypothesis because by the time their third son reached age three the women in my sample were all beyond age 30. The results of my calculations show average birth intervals of 39.7 months and 39.2 months. The larger interval is for the women with three surviving sons, but the difference is only two weeks. These figures suggest that if the women I interviewed used birth control, the consequences were trivial.

A critical evaluation of the evidence offered in support of the revisionists' hypothesis would not be complete without notice of the evidence *not* offered. Birth control on the scale suggested would be like having an elephant in the living room. There would be evidence of its presence everywhere. Evidence exists that women wanted fewer children than they actually bore, but this attitude was regarded as selfish and subversive. Birth control was not a Chinese theme. It was not the cultural pattern demanded by the revisionists' argument. Medical texts did not label abortifacients as such; they did not describe them in detail; they did not compare them in terms of their effectiveness; proverbs did not recommend birth control as a provident strategy; they did not ridicule the man who improvidently raised five sons; the symbols decorating weddings did not suggest that the newlyweds should wait a while before starting a family; they did not hint at the advantages of an early end to childbearing; and when Chinese officials worried about the balance of population and resources, they did not advocate birth control as the solution. Instead, they tried to persuade tradesmen and artisans to return to farming and sought to discourage farmers from plant-

ing commercial crops. "Nothing is said in the imperial edicts or other official documents to suggest that the ruling elite might have advocated smaller families or sexual restraint as a means of slowing population growth."[55] Who, then, cleaned up after the elephant? No one. There was no elephant.

Notes

1 Jean Dickinson, *Observations on the Social Life of a North China Village* (Peiping: Yenching University, 1928), pp. 25–26.

2 Wang Feng, James Lee, and Cameron Campbell, "Marital fertility control among the Qing nobility: Implications for two types of preventive check," *Population Studies* 49 (1995): 399–400.

3 James Z. Lee and Cameron D. Campbell, *Fate and Fortune in Rural China: Social Organization and Population Behavior in Liaoning, 1774–1873* (Cambridge: Cambridge University Press, 1997), pp. 91–92.

4 Zhongwei Zhao, "Deliberate birth control under a high-fertility regime: Reproductive behavior in China before 1970," *Population and Development Review* 23 (1997): 758.

5 E. A. Wrigley, among others, argues that the "key change" in the demographic transition "was from a system of control through social institutions and custom to one in which the private choice of individual couples played a major part in governing the fertility rate." My point is that the Chinese were pretransitional but not governed by custom any more than post-transitional Americans. Their rationality was not what Wrigley calls "unconscious rationality." It was just rationality. See E. A. Wrigley, "Fertility strategy for the individual and the group," in *Historical Studies of Changing Fertility*, ed. Charles Tilly (Princeton: Princeton University Press, 1978), pp. 135–154 (quotation from p. 148).

6 See Arthur P. Wolf and Chieh-shan Huang, *Marriage and Adoption in China, 1845–1945* (Stanford: Stanford University Press, 1980), particularly Chapter 9.

7 See Arthur P. Wolf, "Chinese kinship and mourning dress," in *Family and Kinship in Chinese Society*, ed. Maurice Freedman (Stanford: Stanford University Press, 1970), pp. 189–207.

8 See Francesca Bray, *Technology and Gender: Fabrics of Power in Late Imperial China* (Berkeley: University of California Press, 1997) and Charlotte Furth, *A Flourishing Yin: Gender in China's Medical History, 960–1665* (Berkeley: University of California Press, 1999).

9 The case for the view that government intervention was the critical factor is made in Arthur P. Wolf, "The preeminent role of government intervention in China's family revolution," *Population and Development Review* 12 (1986): 101–116.

10 Zhao, "Deliberate birth control," cited in note 4, p. 761.

11 James Z. Lee and Wang Feng, *One Quarter of Humanity: Malthusian Mythology and Chinese Realities* (Cambridge, MA: Harvard University Press, 1999), pp. 20–21.

12 See Arthur P. Wolf, "Fertility in prerevolutionary rural China," *Population and Development Review* 10 (1984): 443–470.

13 Lee and Campbell, *Fate and Fortune*, cited in note 3, pp. 17 and 66. By Chinese reckoning a person is one *sui* at birth and adds one additional *sui* for each lunar New Year passed. Thus 16 *sui* is approximately (but only approximately) 15 years.

14 Ibid., p. 44.

15 Wang, Lee, and Campbell, "Marital fertility control," cited in note 2, p. 387.

16 Lee and Campbell, *Fate and Fortune*, cited in note 3, p. 90.

17 Zhao, "Deliberate fertility control," cited in note 4, p. 737.

18 Dickinson, *Observations on the Social Life*, cited in note 1, Table 7, p. 42.

19 C. M. Chiao, Warren S. Thompson, and D. T. Chen, *An Experiment in the Registration of Vital Statistics in China* (Oxford, OH: Scripps Foundation for Research in Population Problems, 1938), Table 32, p. 45.

20 Wolf, "Fertility in prerevolutionary rural China," cited in note 12, Table 10, p. 459.

21 The first two figures are from Arthur P. Wolf, *Sexual Attraction and Childhood Association: A Chinese Brief for Edward Westermarck* (Stanford: Stanford University Press, 1995), Tables 7.2 and 7.3, p. 120; the other figures are from unpublished data compiled by Arthur P. Wolf and Chuang Ying-chang.

22 Ts'ui-jung Liu, "The demography of two Chinese clans in Hsiao-shan, Chekiang, 1650–1850," in *Family and Population in East Asian History*, ed. Susan B. Hanley and Arthur P. Wolf (Stanford: Stanford University Press, 1985), Figure 2.2, p. 32; Ts'ui-jung Liu, "Chinese genealogies as a source for the study of historical demography," in *Studies and Essays in Commemoration of the Golden Jubilee of Academia Sinica* (Taipei, Taiwan, Republic of China: Academia Sinica, 1978), Table 5, p. 862; and Ts'ui-jung Liu, "A comparison of lineage populations in south China, ca. 1300–1900," in *Chinese Historical Microdemography*, ed. Stevan Harrell (Berkeley: University of California Press, 1995), Table 4.2, p. 99. My adjustment of the figures Liu reports assumes an infant and early childhood mortality rate of .333.

23 Ted A. Telford, "Fertility and population growth in the lineages of Tongcheng County, 1520–1661," in *Chinese Historical Microdemography*, ed. Steven Harrell (Berkeley: University of California Press, 1995), Table 3.1, p. 51.

24 Herbert D. Lamson, "Differential reproduction in China," *Quarterly Review of Biology* 10 (1935): 308.

25 Wolf, *Sexual Attraction and Childhood Association*, cited in note 21, Table 17.6, p. 290.

26 Stevan Harrell, "The rich get children: Segmentation, stratification, and population in three Chekiang lineages, 1550–1850," in *Family and Population in East Asian History*, ed. Susan B. Hanley and Arthur P. Wolf (Stanford: Stanford University Press, 1985), pp. 81–109.

27 Lee and Campbell, *Fate and Fortune*, cited in note 3, pp. 208–213.

28 Ibid., p. 92.

29 See Wolf and Huang, *Marriage and Adoption*, cited in note 6, pp. 230–250.

30 The Taiwan data are from a survey of 3,000 women living in or near Taipei City in the early 1930s. See Keien Ko, "Statistical studies on the menstruation of Formosans," *The Taiwan Igakkai Zasshi* (Journal of the Medical Association of Formosa) 32 (1933): 89–90. The Fujian and Sichuan data are from unpublished surveys of several thousand women in each province. The Fujian survey was conducted as part of a project directed by Chuang Ying-chang, Yang Guozheng, and Arthur P. Wolf; the Sichuan survey, as part of a project directed by Hill Gates working in cooperation with the Sichuan Women's Federation.

31 See James W. Wood, *Dynamics of Human Reproduction: Biology, Biometry, Demography* (New York: Aldine de Gruyter, 1994), pp. 405–410.

32 Wang Feng and Yang Quanhe, "Age at marriage and the first birth interval: The emerging change in sexual behavior among young couples in China," *Population and Development Review* 22 (1996): 302.

33 Wang, Lee, and Campbell, "Marital fertility control," cited in note 2, p. 390.

34 Lee and Campbell, *Fate and Fortune*, cited in note 3, p. 92.

35 Zhao, "Deliberate fertility control," cited in note 4, p. 735.

36 The Taiwan figure is for women in major marriages who were born in the years 1880–1900. Women who were divorced or widowed before age 45 are excluded. In the major form of marriage the bride was a nubile or nearly nubile young adult. In the alternative minor form of marriage she was an infant or small child. The distinction is relevant to birth intervals because the early association characteristic of minor marriage reduced fertility by approximately 40 percent.

37 Wang, Lee, and Campbell, "Marital fertility control," cited in note 2, p. 390.

38 William H. James, "The causes of decline in fecundability with age," *Social Biology* 26 (1979): 333.

39 Wang, Lee, and Campbell, "Marital fertility control," cited in note 2, p. 391.

40 Lee and Campbell, *Fate and Fortune*, cited in note 3, p. 94.

41 Zhao, "Deliberate fertility control," cited in note 4, p. 742.

42 Wang, Lee, and Campbell, "Marital fertility control," cited in note 2, pp. 396–397.

43 Louis Henry, "Some data on natural fertility," *Eugenics Quarterly* 8 (1961): 81.

44 Wang, Lee, and Campbell, "Marital fertility control," cited in note 2, p. 398.

45 Wang and Yang, "Age at marriage and the first birth interval," cited in note 32, p. 313.

46 Arthur P. Wolf, "Women, widowhood, and fertility in pre-modern China," in *Marriage and Remarriage in Populations of the Past*, ed. J. Dupâquier et al. (New York: Academic Press, 1981), pp. 139–147.

47 Wang, Lee, and Campbell, "Marital fertility control," cited in note 2, p. 397.

48 Zhao, "Deliberate fertility control," cited in note 4, Figures 2, 3, and 4, pp. 739, 741, and 743.

49 See, for example, Wolf and Huang, *Marriage and Adoption*, cited in note 6, Table 17.2, p. 232.

50 Zhao, "Deliberate fertility control," cited in note 4, Table 4, p. 746.

51 Ibid.

52 Wolf and Huang, *Marriage and Adoption*, cited in note 6, Table 15.6, p. 214.

53 The Taiwan data exclude women who reacted to the death of their own child by adopting (and presumably nursing) another infant.

54 Zhao, "Deliberate fertility control," cited in note 4, Table 5, p. 747.

55 Bray, *Technology and Gender*, cited in note 8, p. 269.

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Fertility Control in China's Past

ZHONGWEI ZHAO

IN AN ARTICLE published in this journal in 1997, drawing on data from China's 1982 One-per-Thousand-Population Fertility Survey, I argued that the high-fertility regime existing before China's nationwide family planning campaign was introduced could have involved deliberate fertility control. James Lee, Feng Wang, and Cameron Campbell reached a similar conclusion (Wang, Lee, and Campbell 1995; Lee and Campbell 1997; Lee and Wang 1999). Such claims challenge the widespread belief that historically the Chinese did not control their reproduction and wanted as many children, sons in particular, as possible.

Arthur Wolf (2001) in this journal challenged the studies undertaken by Lee and his colleagues and by myself. Unfortunately, Wolf's commentary misrepresents our work, uses our data in misleading ways, and produces contradictory arguments. This brief note aims to correct these mistakes, thereby refuting his major claims and conclusions.

Wolf started by questioning the quality of our data. China's 1982 One-per-Thousand-Population Fertility Survey was designed according to strict statistical procedures, and the data were collected by well-trained enumerators. The sample size is very large and the results have been systematically analyzed by leading demographers from around the world during the last two decades (Coale 1984). It is widely accepted that the 1982 survey is of very high quality. Wolf is skeptical about the survey data, arguing that recall problems make reports of numbers of children born up to 50 years earlier unreliable. His objection is based largely on his study of a small nonrandom sample (580 women) and on the observation that he himself under-recorded the number of births in his interviews (Wolf 2001: 135–136; 150). While acknowledging the possibility of under-registration in the 1982 survey data, I argued in 1997 that it is very unlikely that under-registration could have changed or explained the systematic patterns in fertility behavior identified in my article. Wolf cited under-registration as a major reason for the relatively low fertility levels and the complicated fertility patterns, but as will be demonstrated his interpretations are self-contradictory.

In challenging our interpretation of past reproductive behaviors, Wolf extracted 13 propositions from our findings and conclusions. He then applied a four-step test to each of them. First, he listed a single proposition or a group of propositions that he regarded as interlinked. He then provided limited data on the proposition. Third, he argued that factors other than deliberate fertility control could have caused the variation shown in the data. Finally, he concluded that there was no deliberate fertility control in the past.

This procedure presents a number of problems. First, it tends to distort our findings. Most of Wolf's propositions are extracted from our studies; while they relate to our findings, the formulations are very different from those in our work. Moreover, the examination of one or more highly simplified propositions at a time fails to give a complete picture of the complexity of people's fertility behavior as shown in our analyses. Second, Wolf implies that this procedure was also the way that our conclusions were drawn. For example, Wolf claims that "Zhao tests proposition 12 by comparing the mean ages at last birth of women whose first sons were born at ages 15–19, 20–24, and 25–29" (2001: 149). However, I did not proceed from propositions such as those set forth by Wolf to data analysis. My conclusions were reached through a consideration of all the findings reported in my article. A third, more serious problem arises from Wolf's procedure. That his tests (of each or a group of propositions) were conducted and his explanations were reached in isolation affects his ability to draw appropriate conclusions.

Wolf offers a number of explanations for China's complicated fertility patterns; most of them could explain one or at best a few of his propositions. None, in any event, could explain all the propositions. In contrast, the reasons we advanced can. Moreover, Wolf's suggestions are contradictory—a fact related to the way in which they have been reached.

For example, Wolf challenges the One-per-Thousand-Population Fertility Survey data by arguing that they may not be "entirely accurate." Women may fail to report all the children they had borne (Wolf 2001: 135). Wolf also states that women may fail to report children who were given away, sold, or otherwise disposed of at birth (Wolf 2001: 150). Elsewhere in his article Wolf suggests that the variation in fertility patterns as shown in my analysis and in the 1982 survey data could have resulted if many families gave away or sold their last-born son in infancy, because this would "inevitably shorten the next birth interval and thereby raise both maternal age at last birth and the parity progression ratio" (Wolf 2001: 148). But this explanation implies that these events were accurately recorded in the survey data. Wolf is in effect arguing that the quality of the 1982 survey data is high. He cannot have it both ways.

Similar contradictions can be found elsewhere in Wolf's discussion. For example, he claims that "far from limiting the number of sons reared,

Chinese families made every effort to maximize the number" (Wolf 2001: 134). But he also suggests that Chinese families "commonly surrendered third-, fourth-, and fifth-born sons" (Wolf 2001: 148). Practice of this kind is certainly not an indication that "Chinese couples wanted as many sons as possible" as Wolf insists (Wolf 2001: 151). On the contrary, it would suggest that these families did not want to maximize the number of their sons.

Wolf has been selective in using other people's results. This itself is not a problem so long as the user does not change the author's original findings and major conclusions. I presented three triangle-shaped diagrams in my 1997 article showing the mean age at mothers' last birth, parity progression ratios, and average length of birth interval, all by sex composition of preceding children, and I made the following observations. "[T]he sex composition of children already born exerted a noticeable effect on women's fertility behavior. A greater proportion of women who had only daughters among their preceding children went on to have another child; their birth interval was shorter; and their mean age at last birth was higher. These points, to a large extent, also apply to women who had only sons. In contrast, a smaller proportion of those who had both sons and daughters, especially those whose sons outnumbered their daughters, went on to have a further child; their birth interval was somewhat longer; and their mean age at last birth was lower" (Zhao 1997: 743–744). This is a general summary of the findings presented in the section to which the three diagrams belong. The actual fertility patterns are more complex because they are also affected by other factors. Wolf selects some figures from the three diagrams and lists them in his Table 1. This selection considerably simplifies and alters the complicated fertility patterns and major findings reported in my article. Wolf then uses the subset of the original data to test propositions he claims to have identified in my work (Wolf 2001: 146–147). In doing so, he attributes to me something that is not my major conclusion and does not replicate the way that my conclusion was drawn.

Wolf also incorrectly manipulates some results published in my article. In his Table 2, for example, he calculates from the data presented in my three diagrams mean age at last birth, mean length of last birth interval, and mean parity progression ratio for women who already had both sons and daughters (Wolf 2001: 147). Elementary statistics tells us that if one wants to calculate the mean, the weighted mean must be used under this particular circumstance. It is not clear how Wolf obtained his averages, especially the average parity progression ratio. But they are not weighted ones, because I did not publish data allowing such calculations in my article.

In addition, Wolf apparently misunderstands my results in a number of places. For example, he reproduces my Table 6 on parity progression ratios for women after age 30, by age at birth of first surviving son, as his Table 4. I stated that the conditional parity progression ratios in the table

were computed "according to the following procedure. First, the women were divided into four groups depending on the age when they had their first surviving son. Then the parity progression ratios were computed in each group. In contrast to the conventional procedure where all children are recorded irrespective of the age of their mothers, only the births a woman had after her 30th birthday are counted here. The first child born after age 30 is re-coded as parity one and so on, regardless of how many children a woman had before reaching that age" (Zhao 1997: 747). Wolf clearly misunderstands the table since he makes the following remark: "I interpret this to mean that women in the upper left hand corner of the table bore their first child before age 20 (the son who survived) and then did not bear a second child until after age 30, while women in the next level of the same column bore their first child (again the son who survived) between ages 20 and 25 and then did not bear another child until after age 30" (Wolf 2001: 149). It is apparently on the basis of this incorrect interpretation that Wolf imputes to us his proposition 13, which states that "An additional birth was more likely among women whose first surviving son was born early than among women whose first surviving son was born late" (Wolf 2001: 145). Although he claimed that this proposition had been extracted from our work, it bears no relation to my findings and conclusions.

There are further misreadings in Wolf's criticism. At the beginning of his article, Wolf labels me as one of the revisionists and then makes the following claim: "The revisionists and I agree...that marital fertility in China followed a natural fertility trajectory" (Wolf 2001: 134). I did compare the age-specific marital fertility patterns of Chinese women with those of so-called natural fertility populations (Zhao 1997: 757). But, I made it clear that the purpose of my comparison was to demonstrate that examining the shape of the trajectory of age-specific marital fertility may not be an effective way of identifying those individuals engaged in fertility-regulating behavior (Zhao 1997: 754–756). For the same reason, I was skeptical about the definition of natural fertility, let alone about using a natural fertility trajectory to measure China's marital fertility. However, Wolf represents me as a supporter of his claim.

Wolf also misapplies results reported by other scholars. For instance, he employs a set of fertility rates published by Liu Ts'ui-jung in 1995 to support his claim that China's historical fertility should be higher than suggested by Lee and his collaborators and by me (Wolf 2001: 137). Wolf fails, however, to mention that the figures he cites are based on the high estimate of fertility produced by Liu. More significantly, in computing this set of fertility rates, Liu included only those who had sons or whose sons were recorded in the genealogies; those without sons but at risk of having them were excluded. Therefore, these figures are not conventional fertility rates. It is wrong to compare them with those reported in our studies. Liu presented another set of

fertility figures in the same study that are closer to conventional fertility measures but indicate a lower fertility level (Liu 1992, 1995). If we take these into account, the actual fertility in the lineage population studied by Liu could be considerably lower than that suggested by Wolf.

Two of Wolf's major claims require further comment. First, Wolf claims that in the examination of China's moderate marital fertility, Lee and his collaborators and I stressed intentional control of reproduction and made no "attempt to formulate and test alternative explanations" (Wolf 2001: 145). This is incorrect. In searching for reasons why marital fertility was not very high in Chinese history, we have expended at least as much energy as Wolf. In preparing the article with which Wolf takes issue, I examined almost all of the explanations provided in his commentary—including such factors as decrease of marital sexual relations caused by child marriage, low coital frequency, prolonged and intense breastfeeding, poor health, low living standards, certain kinds of diseases, and periodic separation between spouses associated with seasonal migration. I stated that "[t]hese explanations are certainly important in our understanding of fertility patterns in Chinese history," although I also pointed out their limitations (Zhao 1997: 731). (I also examined the impact of infanticide and under-registration. But strictly speaking, these factors come into play after the birth of a child and should not be seen as means of affecting marital fertility.) The difference between Wolf and me, therefore, is not that I made no attempt to find alternative explanations for the moderate marital fertility while he did. What divides us is that facing complicated fertility patterns, Wolf has been reluctant to accept deliberate fertility control as an alternative explanation, while I, like Lee and his collaborators, have accepted this possibility and investigated it.

Second, the central theme of Wolf's commentary is that there is no evidence of deliberate fertility control in the past. He tries very hard to find better explanations for China's relatively low marital fertility but fails to advance any that have not been suggested previously. Most of Wolf's points are important to our understanding of past fertility patterns, but they are neither new nor sufficient in explaining the complicated fertility behavior observed in Chinese history.

Ironically, evidence uncovered by Wolf and his collaborators suggests that intentional fertility control was practiced in the historical Chinese population. For example, Wolf and his collaborators organized a study in the mid-1990s in Fujian province similar to the one Wolf conducted in the early 1980s. The study, which interviewed 50 elderly women in Lianjiang County, found that the mean number of children was 5.9 among 42 women with completed fertility. The mean age at last birth (computed for 40 women) was 38.3 (Zheng 2000: 71–72). These figures are very close to those derived from Wolf's early study and virtually identical with those I reported (Zhao 1997: 735). In discussing the question whether women had inten-

tionally controlled their fertility, one of the principal investigators, Zhenman Zheng, made the following remark: "It was a great pity we had not directly asked this question. But the indirect evidence suggested that the answer should be yes. For example, when women were asked about their intention of childbearing, the overwhelming majority said that they did not want to have more children" (Zheng 2000: 71).

Accordingly, the question seems not to be whether there is evidence of deliberate fertility control, but rather whether we are willing to acknowledge such evidence. Wolf is defending a position that he has long held. In the mid-1980s, in his debate with Ansley Coale about fertility levels in historical China, Wolf made the following claim. "Whatever the reason for *moderate fertility in China, it was not deliberate fertility control*" (Wolf 1985: 177). During the last 15 years, Wolf has slowly retreated from his original position that fertility was high in Chinese history, but he has yet to retract his claim of no deliberate fertility control in the past.

China has a long history. Population issues were discussed among scholars and politicians more than 2,000 years ago. Although traditional Chinese culture had many pronatalist components, evidence of "antinatalist" thinking and practice was widespread. In the Tang Dynasty (AD 618–907), Wang Fanzhi, a poet, expressed the view that having one son was enough. During the Song Dynasty (AD 960–1279), a number of scholars asserted that people wanted to have only two sons or even just one. The Ming-Qing period (AD 1368–1911) witnessed a further increase in the discussion of population issues. During the seventeenth century, some scholars and officials were concerned that population growth had accelerated and could overtake the growth of food output. Wolf states that "when Chinese officials worried about the balance of population and resources, they did not advocate birth control as the solution" (Wolf 2001: 151); however, some contemporary commentators did suggest that over the long term the number of children in each family needed to be kept at two. In the eighteenth century, Hong Liangji's assessment of population issues prefigured that of Malthus, earning him the title of "Chinese Malthus." In the mid-nineteenth century, Wang Shiduo advocated population control measures, including imposing a heavy tax on large families, enforcing late marriage, encouraging celibacy, and spreading the use of drugs to reduce pregnancies and births (Li 1994, 2000; Zhao 1997). China has a long history of using medical substances and other measures to induce abortion and to prevent pregnancy. Potions were used to cause abortion some 2,000 years ago. Medical substances and prescriptions that were believed to prevent or terminate pregnancy or cause sterilization were detailed in many medical writings published during last two millennia.

The extent to which the Chinese intentionally controlled their reproduction in the past is still less clear, but given the available evidence it

would be unwise to suppose that the Chinese made every effort to maximize the number of their children and never wanted to practice deliberate fertility control.

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Pretransitional Fertility in China

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ALMOST TWO DECADES ago, in this journal and elsewhere, Arthur Wolf and Ansley Coale engaged in a debate over the level of marital fertility in pre-revolutionary China (Coale 1984, 1985; Wolf 1984, 1985). Coale and his colleagues, George Barclay, James Trussell, and Michael Stoto, after re-analyzing survey data collected from 40,000 farm families in 119 widely dispersed localities in China in 1929–31, not only concluded that marital fertility was lower in China than in historical Europe, but concurred that such a low level "would be expected by demographers only in populations where some combination of contraception and abortion is practiced" (Barclay et al. 1976: 625). Wolf, relying largely on his own studies of early-twentieth-century populations from Taiwan and his 1980–81 survey of 580 elderly women from seven other provinces, challenged their results and their explanation.[1]

During the last decade, in this journal and elsewhere, we and others have expanded our understanding of Chinese population behavior.[2] While it is not yet possible to produce reliable estimates of population size and demographic rates for China as a whole before 1950, contemporary national surveys and historical micro-studies reveal consistent patterns of behavior over long periods of time for a variety of Chinese populations. In Lee and Wang (1999a, 1999b), we survey and summarize the demographic evidence from these historical micro-studies and compare the be-

[1]Wolf initially presented his criticism of Barclay et al. (1976) at a conference in 1978 and published it in an exchange with Coale in *Population and Development Review* in 1984 as well as in Hanley and Wolf (1985).

[2]Notably Stevan Harrell, William Lavely, Ts-ui-jung Liu, Ted Telford, Arthur Wolf, and Zhao Zhongwei among others. See Lee and Wang (1999b) for a complete bibliography.

havior patterns revealed with national trends for contemporary China.[3] We identify several distinctive aspects of Chinese demographic behavior—lopsided mortality and nuptiality, especially by gender, low marital fertility, and high rates of fictive kinship and adoption—that persist today, that differ from Western patterns, and that temper the classic Malthusian understanding of comparative demographic behavior in general and in China in particular.[4] We demonstrate how these behaviors interacted historically to form a consistent demographic system that was deeply embedded in the collective nature of Chinese social organization. We construct, in other words, a stylized model of a Chinese demographic system to contrast with the classic European demographic system first proposed by Malthus and elaborated by others, notably Hajnal (1965, 1982), Laslett (1983, 1988), Macfarlane (1978, 1986, 1987, 1997), Wrigley and Schofield (1981), and Wrigley et al. (1997).[5]

While Wolf accepts our general characterization of Chinese population behavior, he dismisses our evidence and disputes our explanation of low marital fertility (2001: 134).[6] As in his earlier exchange with Coale, he argues that the quality of our data is poor, that our estimates of fertility are low, that our results are unrepresentative of China as a whole, and that "the remaining difference is due to positive rather than preventive checks" (1984: 445).[7]

Wolf rejects the possibility that Chinese couples in the past deliberately sought to control their fertility and argues, "far from limiting the number of sons reared, Chinese families made every effort to maximize the number" (2001: 134). In so doing he largely repeats his 1984 assertions and arguments, while ignoring Coale's 1984 response.

Since Wolf's arguments are old, we had hesitated to respond. We do so now because his analyses and his critique of our analyses are incorrect, and because without such a refutation, his miscalculations, problematic evidence, and dubious interpretation serve other scholars in a larger debate

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[3]In Lee and Wang (1999b) we surveyed studies of some 500,000 individuals who lived before 1950. Continuing research during the last four years has added another 300,000 individuals and should continue to add many more, broadening our understanding of Chinese historical population processes.

[4]See Lavely and Wong (1998) and Zhao (1997) for two other comparisons using Chinese data, and Das Gupta (1995) for a comparison of the European and Indian experience.

[5]By "system" we mean the defining characteristics of Chinese demographic behavior during the last 300 years in contrast to the "European demographic system" identified by Flinn (1981).

[6]"The revisionists and I agree that in China marriage was early and nearly universal for females; we agree that as compared with premodern Europe, marital fertility was moderate; that birth intervals were longer in China than in Europe; that in China the interval from marriage to first birth was particularly long; that marital fertility in China followed a natural fertility trajectory; that it was higher among the wealthy than among the poor; that the Chinese used sex-selective infanticide to regulate family size; that they used both male and female adoption for the same purpose; that most Chinese recognized the relationship between breastfeeding and child spacing; that they used this knowledge to achieve fertility goals; and, most importantly, we agree that their attitude toward reproduction was eminently rational" (Wolf 2001: 134).

[7]Wolf argues that any instance of low marital fertility in China was an involuntary consequence of "poverty" and that low marital fertility is therefore "evidence of chronic malnutrition, untreated diseases, hard manual labor, and economically enforced conjugal separation" (Wolf 2001: 137).

who continue to define the pretransitional Chinese demographic regime in terms of the Malthusian positive check (Brenner and Isett 2002; Cao and Chen 2002; Huang 2002).[8] The stakes, as proclaimed by Wolf, are high since they have implications not only for China, but also for our understanding of larger comparative social and economic processes.

In this reply we focus on the three central issues of Wolf's refashioned critique: the levels of pretransitional marital fertility, the patterns of reproductive behavior behind them, and the use of poverty to account for low Chinese fertility within marriage. We clarify his misrepresentations of our data, findings, and interpretations. At the same time, we illustrate how he relies on sources that are sometimes selective and other times irrelevant and uses demographic methods that are occasionally inappropriate. Finally, we demonstrate how in accounting for low marital fertility in China, Wolf relies on an outdated understanding of the association between nutritional status and fertility.

Pretransitional Chinese marital fertility

Although Wolf (2001) agrees that the level of marital fertility in traditional China was moderate, he also seeks to demonstrate that our estimates of marital fertility for specific populations are low and to present alternative estimates of marital fertility for other Chinese populations that are somewhat higher. Wolf believes that these higher levels "do not demand birth control as an explanation" (2001: 136).[9] He also raises a number of objections to our data.

Wolf's first objection is that our evidence is limited and comes "almost exclusively from three sources" (2001: 135). This characterization is incorrect. It is true that our analyses of primary data are largely confined to two historical populations: 100,000 members of the Qing imperial lineage who lived in Beijing and Shenyang between 1600 and 1900, and 100,000 royal peasants who lived in what is now Liaoning Province between 1750 and 1900. Our descriptions of Chinese marital fertility, however, are derived from the entirety of the secondary published record in Chinese historical demography. In addition to our own fertility estimates for imperial Beijing and rural Liaoning, for example, we present estimates from 16 other historical and contemporary studies, including Wolf's own estimates of marital fertility in early-twentieth-century Haishan, Taiwan to document that

[8]We address elsewhere (Lee, Campbell, and Wang 2002; Wang and Lee 2002) the problems with these Malthusian interpretations of Chinese history.

[9]Wolf's (2001) apparent belief that the level of fertility can be used to reveal the presence or absence of fertility control is iconoclastic. It is precisely because the level of fertility is unreliable as an indicator of the presence of fertility control that Coale and Trussell (1974) developed procedures for detecting parity-specific control from the age pattern of fertility and that we identify other patterns of fertility behavior in Wolf's list of our 13 hypotheses (2001: 145).

pretransitional total marital fertility typically ranged from 5.3 to 6.3 births per woman (Lee and Wang 1999b: 85, 87).[10]

Wolf's second objection, that our data are flawed, ignores the measures we take to define and deal with their limitations.[11] For instance, he notes our estimate that the Liaoning household archives from Daoyi omit one-third of male births and two-thirds of female births (Lee and Campbell 1997: 13, 66), but he does not acknowledge our caveats about these data or our adjustments for this undercount. After presenting our final estimate of a total marital fertility rate of 6.3 for 3,000 married Han bannermen from Daoyi in Liaoning, he asserts without explanation, "it is likely that they considerably underestimate Chinese fertility. The true rate was at least 7.4 live births per woman, which was the figure for the 71 women included in Dickinson's study of Jianyang" (p. 136).[12]

Conversely, Wolf does not address potential problems with his own data. While he cites results from his and other analyses of mid-twentieth-century Taiwanese household registers, his own survey of 580 women in 1980–81, several genealogy-based studies, and Dickinson's and several other small-scale Republican-era studies, he discusses neither the selection of these samples nor the limitations of the sources.[13] He seems unaware of any possible survivor or cohort bias in his survey of elderly women,[14] and does not acknowledge that the genealogy-based studies he cites often exclude childless women and women who did not bear sons,[15] and are accordingly biased upward.[16]

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[10]These studies include Liu (1995) and Telford (1995) for a variety of other Chinese lineage populations during the Ming and Qing periods, estimates by Barclay et al. (1976) for 22 provinces of China in 1929–31, Wolf's (1985) estimates of age-specific marital fertility rates in Haishan, Taiwan in 1906–10, and estimates for China as a whole after 1949 by Coale and Chen (1987), Lavely (1986: 432–433), and Yao and Yin (1994).

[11]Wolf (2001) dismisses the genealogical archives of imperial lineages because even though these data can be "accepted as accurate," the population was unrepresentative of China. Conversely, he dismisses the household register archives of royal peasants in Liaoning because even though "they were representative of the larger population," the data are inaccurate (p. 136).

[12]This is but one example of a preference for assertion and illustration over deduction or induction as evidence. Wolf, for example, speculates that the registers were inaccurate because "it seems likely that some young men avoided registration by bribery or flight" (p. 136), ignoring our analyses of data quality and underregistration based on mortality patterns in the data and comparison with model life tables (Lee and Campbell 1997: 65–70, 223–237). He applies a similar logic in rejecting what he claims are our estimates for the lengths of the first birth interval, the intervals between births, and the age at last birth.

[13]Many of these samples are not only small, but potentially unrepresentative. While Wolf's own interviews of elderly women may suggest that they sought to have as many sons as possible, other retrospective surveys of different elderly women have come to opposite conclusions. Zheng's (2000) survey of Guanling village in Fujian Province is a good example. According to this 1994–95 study of 50 women aged over 60 years, 37 reported that they did not want more children than they had (pp. 70–71).

[14]Wolf's 1980–81 survey included only elderly women who had spent the majority of their reproductive years in the pronatalist period of 1949–69. Fertility estimates based on these women's experiences may be biased upward because those fortunate enough to survive to old age are likely to have been healthier and to have had more children than those who died at younger ages.

[15]Referring to the estimates of marital fertility from lineage genealogies, Liu (1995: 100) notes that they were "derived from age-specific fertility rates, which required the data from each conjugal family to include quite complete vital dates for each member, and those families with no sons were not included in the observation."

[16]Wolf compounds such biases when he multiplies the rates reported by Liu (1995) by 1.5 to account for underregistration (2001: 137). While an adjustment may be in order, the one he applies appears arbitrary; in any event, he provides no explanation for choosing it over other possible values.

Wolf, moreover, excludes several of his own findings that contradict his claims.[17] Thus while he insists that the Chinese total marital fertility rate was between 7 and 8 (2001: 137), he does not mention that his own estimates of total marital fertility in Haishan, Taiwan in 1906–10, 1911–15, 1916–20, 1921–25, and 1926–30 were 6.25, 6.38, 6.15, 6.69, and 7.08 respectively before peaking at 7.41 and 7.94 in 1931–35 and 1936–40 (1984: 455). Putting aside the bottom line that even these numbers are inflated and, once recalculated, hardly differ from Barclay et al. (1976) and our own, Wolf's use of early-twentieth-century fertility estimates from colonial Japanese Taiwan to challenge fertility estimates from eighteenth- and nineteenth-century northeast Qing China seems irrelevant.

Finally, Wolf's claim of high pretransitional fertility depends not only on his selective use of data, but also on his idiosyncratic calculation of total marital fertility. While demographers when comparing marital fertility typically use either age-specific rates[18] or a summary measure of total marital fertility for women aged 20–49, excluding those aged 15–19,[19] Wolf includes the latter age group and accordingly skews his comparisons.[20] To demonstrate the degree of this bias, we reexamine the age-specific rates on which he bases his claims. In Table 1, we compare two summary measures of fertility, the total fertility rate (TFR) and the total marital fertility rate (TMFR) including and excluding women aged 15–19.[21] Excluding females aged 15–19 would lower the TFR for Haishan, Taiwan between 1906 and 1945 by 9

[17]Similarly, when Wolf (2001: 137) lists 11 estimates of marital fertility for rural communities in Taiwan, two of which, 7.61 and 7.78, he says are from Wolf (1995: 120), he fails to mention that this source presents six estimates of the level of marital fertility for different population subgroups depending on ethnicity (Hakka or Hokkien) and type of marriage (major, where an adult daughter-in-law moves into her husband's home; minor, where an adopted daughter-in-law moves into her prospective husband's home at a young age; and uxorilocal, where an adult son-in-law moves into his wife's home). The 7.61 in Wolf (2001: 137) seems to correspond to Hokkien in major marriages. We could not match 7.78 to any of the estimates in Wolf (1995: 120), the highest of which was 7.69, for Hakka in major marriages. The total marital fertility rates for Hokkien and Hakka in minor marriages are much lower, 6.02 and 6.19, respectively (Wolf 1995: 120).

[18]Coale (1984: 475) in his response to Wolf (1984) plots age-specific marital fertility rates from Haishan, Taiwan 1906–45 and from Barclay et al. (1976) to show that they are almost identical with each other and appreciably lower than historical European marital fertility rates. We do the same in our comparison of age-specific marital fertility rates from a variety of pretransitional East Asian and West European populations (Lee and Wang 1999b: 87).

[19]Thus the calculations of total marital fertility rates by Flinn for pretransitional European populations (1981: 31), and by Wrigley et al. (1997: 427) for selected pretransitional English populations, include only married women aged 20–49; the same is true for the comparisons by Lee and Wang (1999b: 87) for historical Chinese populations and by Lavely (1986) for contemporary Chinese populations.

[20]Including married women aged 15–19 would raise the total marital fertility rates for pretransitional English populations to 9.5 for all married women and to 12.0 for fecund women (Wrigley et al. 1997: 355, 416). Similarly such a procedure would raise the TMFR of pretransitional German populations to between 10.6 and 11.3 over the interval 1750 to 1899 depending on the time period (Knodel 1988: 257).

[21]The total marital fertility rate that Wolf relies on is a summation of age-specific marital fertility rates. It reflects the number of births a woman would have if she married by the earliest age of the earliest age group and remained married until the latest age of the last age group. Thus a TMFR based on age-specific rates from ages 15 to 49 would reflect the number of children a woman would be expected to have if she married at age 15 and remained married until age 49. A TMFR based on rates from ages 20 to 49 would reflect the number of births expected for a woman who married at age 20. Comparisons of TMFRs can be misleading if they include ages at which few women are married, in particular the age group 15 to 19. Relatively few women are married in this age group, and the marital fertility of those who are married tends to be unusually high. Including this age group generates a misleading impression that every married woman would have the same number of births as those who married as young as 15 to 19. Most calculations and comparisons of the TMFR accordingly begin at age 20.

TABLE 1 Fertility rates recorded by Wolf, with and without age group 15–19 years

Period/location	15–19	20–24	25–29	30–34	35–39	40–44	Total	Omitting 15–19	Percent difference
Wolf's Haishan, Taiwan data, 1906–45									
Age-specific fertility									
1906–10	139	252	231	212	157	86	5.4	4.7	13
1911–15	123	250	229	206	173	84	5.3	4.7	12
1916–20	111	252	230	206	159	74	5.2	4.6	11
1921–25	128	267	251	207	159	76	5.4	4.8	12
1926–30	139	271	261	222	170	78	5.7	5.0	12
1931–35	130	255	267	240	175	82	5.7	5.1	11
1936–40	114	261	270	251	196	93	5.9	5.4	10
1941–45	88	222	236	208	163	89	5.0	4.6	9
Age-specific marital fertility									
1906–10	261	263	237	221	171	98	6.3	5.0	21
1911–15	271	271	243	209	186	95	6.4	5.0	21
1916–20	245	281	239	212	169	84	6.2	4.9	20
1921–25	296	306	266	222	160	78	6.6	5.2	22
1926–30	310	318	282	239	184	84	7.1	5.5	22
1931–35	336	311	291	259	188	96	7.4	5.7	23
1936–40	353	337	305	275	213	105	7.9	6.2	22
1941–45	321	316	283	243	182	100	7.2	5.6	22
Wolf's 1980–81 survey									
Age-specific fertility									
Beijing	142	312	312	254	165	27	6.1	5.4	12
Fujian	128	238	293	253	158	55	5.6	5.0	11
Zhejiang	119	294	326	205	173	49	5.8	5.2	10
Jiangsu	77	313	283	265	160	54	5.8	5.4	7
Shandong	67	264	269	240	196	80	5.6	5.2	6
Shanxi	163	261	268	226	161	54	5.7	4.9	14
Sichuan	69	251	302	273	167	118	5.9	5.6	6
All	106	275	298	246	169	65	5.8	5.3	9
Age-specific marital fertility									
Beijing	261	348	335	279	183	30	7.2	5.9	18
Fujian	346	279	344	294	205	78	7.7	6.0	22
Zhejiang	244	331	349	224	190	55	7.0	5.7	18
Jiangsu	285	347	294	277	175	60	7.2	5.8	20
Shandong	249	334	282	255	216	91	7.1	5.9	17
Shanxi	241	265	269	230	168	58	6.2	5.0	20
Sichuan	185	274	313	293	190	148	7.0	6.1	13
All	255	309	308	264	189	76	7.0	5.7	18

SOURCES: Hai-shan, Taiwan (Wolf 1984: 454, 455); Wolf 1980–81 survey (Wolf 1984: 458, 459).

to 13 percent and for the seven locales of Wolf's 1980–81 survey of elderly women by 6 to 14 percent. Excluding married females aged 15–19 reduces the TMFR even more, by over 20 percent for Wolf's Haishan data and by 18 percent for his 1980–81 survey data, lowering the TMFR from Wolf's 7.4 (Wolf 2001: 136) to somewhere between 4.9 and 6.2, which is consistent with our and other estimates of Chinese pretransitional marital fertility.[22]

Pretransitional Chinese reproductive behavior

The real debate, of course, is not about the level of fertility but about its determinants. Our own view is that the low level of marital fertility in historical China at least partially reflected deliberate control. We say partially because low marital fertility in China resulted from a number of behaviors, some of which reflected deliberate efforts to delay or forgo births, some of which had other purposes. While couples were capable of deliberately adjusting their fertility to their circumstances, they also engaged for other reasons in other behaviors that lowered fertility, such as low coital frequency and very prolonged breastfeeding. Thus, while we have emphasized deliberate control because its presence is the most distinctive feature of Chinese marital fertility (Lee and Wang 1999a, 1999b; Wang, Lee, and Campbell 1995), we also recognize the contribution of these other behaviors to China's low pretransitional marital fertility.

This approach to the evidence differs radically from Wolf's (2001) characterization of our account of low marital fertility.[23] We do not, as Wolf suggests, claim that the relatively low marital fertility of the Chinese was solely due to "birth control," that is, deliberate behavior specifically aimed at delaying or forgoing births (2001: 137). While we argue that for Chinese couples, unlike European couples, control over reproduction was within the "calculus of conscious choice" (Coale 1973), we do not claim that all couples exercised control at all times. Nor do we claim that the differences between pretransitional Chinese and European marital fertility were the product of parity-specific birth control.

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[22]The total marital fertility rates for genealogy populations from Anhui presented in Telford (1995: 50) are similarly inflated, as he too includes married women aged 15–19 in his calculations.

[23]Wolf not only misstates our argument; on several occasions he also misstates specific findings. For example, he (2001: 142) parses the text of our comparisons between Chinese and European birth intervals in Wang, Lee, and Campbell (1995) and Lee and Campbell (1997) to suggest that we claim birth intervals in China must have been 48 months. He suggests that this value must have been unrepresentative. In both cases, he ignores the estimates of birth intervals we provided, as well as our discussion and qualification. Thus we state that in the Qing imperial lineage "the average length of the closed birth interval was [initially] around 30 months, but later rose to almost 50 months" (Wang, Lee, and Campbell 1995: 389). Similarly, after the comparison of estimated birth intervals for Daoyi to those in England and France, we note that the "intervals calculated for Daoyi are undoubtedly exaggerated by the omission of unregistered births" (Lee and Campbell 1997: 94) and conclude only that "even the most liberal assumptions...about the proportion [of births] unregistered would not close the gap with Europe."

We do not, as Wolf suggests, "accept as a fact about Chinese behavior an elite 'medical' recommendation that 'coital frequency should be no greater than three times a month for young adults, less than twice a month for middle-aged adults, and once a month at most for the elderly'" (Wolf 2001: 144). We did not claim that all or even most Chinese obeyed such advice, at least not to the letter.[24] Rather, we sought to show that such advice was part of the context in which individuals made decisions. We accordingly noted that similar recommendations were "by the eighteenth century [a] long established consensus in the medical literature," and that as with modern medical advice against smoking, drinking, obesity, and lack of exercise "many Chinese took this advice to heart" (Wang, Lee, Campbell 1995: 398).

In asserting that our evidence of low coital frequency consists solely of qualitative evidence from medical texts, Wolf (2001: 144) ignores the quantitative evidence we cited in Wang, Lee, and Campbell (1995) and misrepresents our explanation for low coital frequency.[25] Surveys of at least one contemporary Asian population, Thailand, reveal that coital frequency for married couples is substantially lower than in the United States.[26] Such direct marked differences in sexual behavior between contemporary populations are hard to reconcile with Wolf's claim that lower coital frequency would require belief "that the Chinese were a peculiarly asexual people" (Wolf 2001: 144).[27]

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[24]In Lee and Wang (1999b: 91) we concluded that "the low fertility and long birth intervals of Chinese couples in the past were at least in part the result of their ability and even willingness to regulate coital frequency."

[25]This is true for our discussions of early starting of childbearing as well as long birth intervals. Whereas according to Wolf (2001: 139–140) we claim that intervals between marriage and first birth were long because married couples "deliberately delayed having children," we actually attributed long intervals at least partly to low coital frequency. In particular, our discussion of this phenomenon referred to more recent studies of long first birth intervals in Asia that suggested a role for the lack of conjugal passion that may have characterized arranged marriages, at least in their early stages (Rindfuss and Morgan 1983; Wang and Yang 1996). Thus we noted that late starting of childbearing was common among the two populations we have analyzed in detail; and we concluded in Lee and Campbell (1997: 93) that "long intervals between marriage and first birth were until recently common throughout China and much of Asia and are usually attributed to the relatively low coital frequencies assumed to have characterized arranged marriages."

[26]In Wang, Lee, and Campbell (1995: 398) we wrote:

> Fertility surveys reveal that even today when couples have the protection of contraception, Asian couples continue to follow a pattern of coital frequency considerably lower than elsewhere. In Thailand, for example, the mean coital frequency of all currently married women during the four weeks preceding the 1987 Demographic and Health Survey was 3.2. Newlyweds only had a monthly coital frequency of 6 which dropped to 4.2 after one year of marriage and 3.7 after four years of marriage. See N. Chayovan and J. Knodel, 'Coital activity among married Thai women: evidence from the 1987 Thailand Demographic and Health Survey,' Research Reports of the Population Studies Center, University of Michigan, no. 91-221, 1991. The comparable number in the United States in 1975 was 8.9 for all currently married women and 10.4 for women in their first five years of marriage. See J. Trussell and C. Westoff, 'Contraceptive practices and trends in coital frequency,' *Family Planning Perspectives* 12 (1980), pp. 246–249.

[27]Wolf's claim that "[t]he assumption that Chinese sexuality was easily inhibited is contradicted by universal prostitution and an illegitimacy rate that far exceeded the European rate" (p. 144) is based on shaky logic and evidence. While it is true that the illegitimacy ratio in Haishan, 1906–45 was 8 percent (Wolf 1981: 143), the ratio for Taiwan as a whole during this period was just one-third to one-half of Haishan's, rising from 2.3 percent in 1906 to over 4 percent from 1920 to 1940 (Barrett 1980: 298). By comparison, according to Laslett (1977), "late in the reign of the first Queen Elizabeth, the proportion of bastard births could reach 9% to 10% over whole decades in certain parishes in Lancashire and Cheshire" (p. 104), and was as high as 6 percent for England as a whole in the early nineteenth century (Wrigley et al. 1997: 224).

Wolf's discussion of the 13 hypotheses that he claims to identify in our work is also problematic (2001: 145–151).[28] For example, he treats purposive fertility behavior as evidence against the existence of control.[29] In particular, he interprets the low fertility of parents who have had sons relative to parents who have had daughters as evidence *against* fertility control *(Wolf 2001: 146–147). He reasons that the higher fertility of parents with* daughters reflects their anxiety over their failure to produce a son, and is indicative of a deliberate effort to increase fertility. We regard such purposive behavior as but one more form of control, since it is completely consistent with our view that Chinese couples sought to regulate their fertility, increasing it in some situations and decreasing it in others. Efforts to accelerate childbearing would distinguish Chinese couples from their European counterparts, for whom reproduction, if not "up to God," was outside the calculus of conscious choice.

The link between poverty and fertility

In arguing that poverty must have accounted for low marital fertility among the Chinese, Wolf (2001) embraces a view of the relationship between nutritional status and fertility that is archaic in light of results from studies carried out in developing countries in recent decades.[30] While starvation of the sort observed during famines may cause menstruation to cease and lower fertility, and poor nutrition may delay menarche, chronic malnutrition above the level of starvation appears to have only limited effects on fertility at later ages (Bongaarts 1980; Gray 1983; Menken, Trussell, and Watkins 1981). In light of such findings, the burden of proof is on Wolf to provide decisive empirical evidence of an association between malnutrition and fertility of the sort he relies on in his explanation.

Coale (1984), in his response to Wolf almost 20 years ago, pointed out that Wolf's invocation of malnutrition to account for low marital fertility was not supported by empirical evidence from developing countries.[31]

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[28]*See the detailed critique of these hypotheses by Zhao (2002).*

[29]Space and time constraints prevent us from dealing with the remainder of Wolf's discussion of the 13 hypotheses he claims to identify in our publications. While we encourage readers to consult Wang, Lee, and Campbell (1995), Lee and Campbell (1997), and Lee and Wang (1999a, 1999b), we suggest that this is an appropriate place for application of Occam's Razor. When one simple and straightforward explanation can plausibly account for 13 distinct phenomena, parsimony seems to dictate that it be favored over the unwieldy combination of separate explanations, conflations, refutations, and dismissals that Wolf (2001: 145–151) offers.

[30]"The revisionists are right in insisting that marital fertility was lower in China than in parts of Western Europe and very much lower than in such exemplary populations as the Hutterites. The reason, they say, was birth control. The reason, I say, was poverty" (Wolf 2001: 137).

[31]This lack of support is the case not only with the level of prerevolutionary Chinese fertility, but also with Wolf's repeated claim that any observed low Chinese fertility was a consequence of malnutrition or poverty. As Coale (1984: 477) replied, "There is a theme running through much of the discussion in Wolf's paper to the effect that fertility is negatively associated with poverty and positively associated with level of living.... As a general proposition, I think the assumption of a close association between level of living and fertility in noncontracepting populations is erroneous."

Summarizing results from studies that had recently been carried out in Bangladesh and Guatemala, Coale concluded, "if there is actual starvation, menstruation ceases and fertility is greatly reduced, but long-lasting malnutrition above the level of starvation seems to have very little effect on fertility" (p. 477). Wolf's (2001) continued invocation of malnutrition to account for low marital fertility is accordingly problematic. He has not cited any scientific evidence that contradicts Coale. Given that Coale's discussion of the relationship between nutrition and fertility is almost two decades old, we conclude our reply with a review of later findings.

While poor nutrition delays the onset of menstruation, even in poorly nourished populations the age at menarche was low enough that it was marginal in the context of comparisons of marital fertility between Europe and China, which should exclude women aged 15–19. Even the late ages of menarche that Wolf (2001: 140) claims for China in the past, 16 to 17 years, were lower than the average age of female marriage in the historical Chinese populations for which data are available—summarized in Lee and Wang (1999b: 67). In Liaoning, it was 18 Western years of age (Lee and Campbell 1997: 86). In the Qing imperial lineage, it was between 20 and 21 (Lee, Wang, and Ruan 2001: 358–361). For Chinese women born between 1900 and 1925, according to results from the nationally representative two-per-thousand survey, it was 19 (Wang and Tuma 1993). In Taiwan during this same period, it was if anything slightly higher (Barclay 1954: 211).[32] We accordingly doubt whether delayed menarche of the sort Wolf suggests would have had a substantial effect on the fertility of married couples.

Even for women who had already reached menarche, the effects of poor nutrition on their fertility are likely to have been mild at most. A series of studies carried out in developing countries since the late 1970s have concluded that chronic poor nutrition by itself does not induce amenorrhea, and therefore does not account for low fertility (Bongaarts 1980; John, Menken, and Chowdhury 1987; Menken, Trussell, and Watkins 1981). Only severe, acute malnutrition of the type observed during famines induces amenorrhea. More recently, a study in the Philippines found that once women who had been pregnant resumed menstruating, nutritional status did not increase subsequent time to conception (Popkin et al. 1993).

As for intrauterine mortality, malnutrition raises its likelihood substantially, but the net effect on fertility should have been mild. The baseline chances of fetal death are low enough that even doubling them has only a small proportional effect on the chances of carrying a pregnancy to term. According to a study carried out in Matlab, Bangladesh, women who weighed the least had twice the likelihood of intrauterine mortality of

[32] In Haishan, Taiwan between 1891 and 1921, median age at first marriage ranged between 18.2 and 19.6 for women in "major" marriages, 16.8 and 17.4 for women in "minor" marriages, and 17.4 and 19.6 for women in uxorilocal marriages (Wolf and Huang 1980: 135).

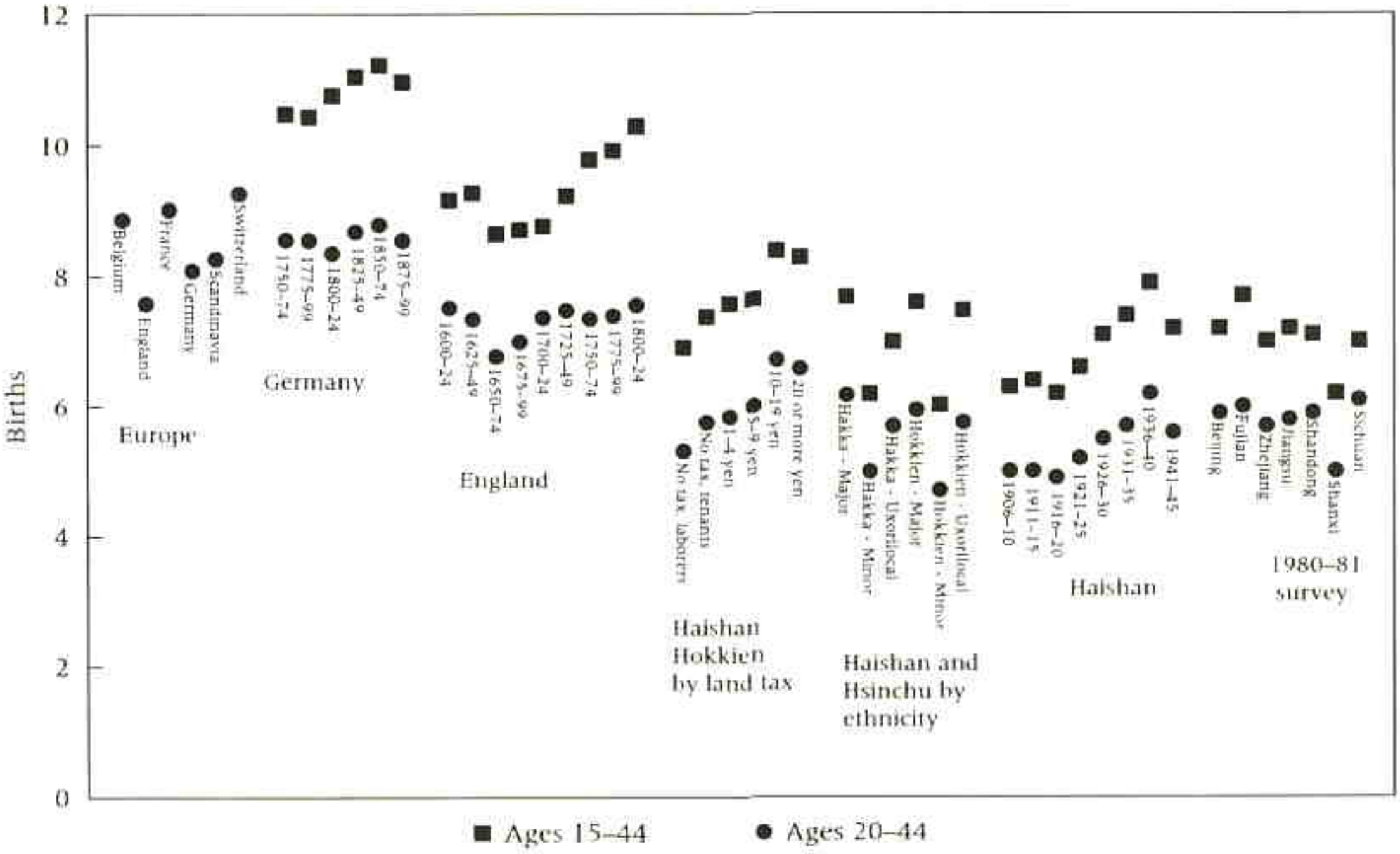
women whose weight was comparable to contemporary American women of the same height: 6 percent versus 3 percent (Pebley et al. 1985: 438). Even with a generous allowance for a much higher baseline risk, say 10 percent of observed pregnancies (Leridon 1977: 63), the implication is that moving from one tail of the distribution of nutritional statuses to the other would only reduce the chances of carrying a pregnancy to term from 90 percent to 80 percent. Moreover, a fetal death averts less than one birth, because women are eligible to conceive again soon afterward.

While there is scientific evidence that postpartum amenorrhea lasts longer for poorly nourished women than for well-nourished ones, it is unlikely that such an effect could have accounted for the gap between European and Chinese birth intervals that even Wolf (2001: 142) acknowledges. Among women in the Philippines, postpartum amenorrhea was only four months longer for poorly nourished women than for well-nourished women (Popkin et al. 1993). A study in Matlab yielded similar estimates of the difference between poorly and well-nourished women in the length of postpartum amenorrhea (Huffman et al. 1987). Neither of these effects—concerning chances of carrying a pregnancy to term and length of postpartum amenorrhea—would account for the differences of a year or more between European and Chinese birth intervals.

As for Wolf's claim that poor nutrition may have induced early menopause, there is no conclusive evidence that such an effect could have accounted for the low average age at last birth. While results from some studies suggest that nutrition may affect menopause—for example, some studies have shown that thinner women reach menopause earlier than heavier women—the strength and source of the relationship remain unclear (Rahman and Menken 1993: 67). Even if nutrition had an effect on the age at menopause, however, the effect probably would not have accounted for the relatively low age at last birth in historical China. Studies in both developed and developing countries almost all report an average age at menopause of around 50 years. The lowest average age reported in the studies surveyed by Rahman and Menken (1993: 66) was 43.6, for a population of malnourished Melanesians living in New Guinea. This is still several years higher than the mean age at last birth that Wolf accepts for China, 38 or 39 years.

Comparison of Wolf's own estimates of marital fertility with national historical data for England, Germany, and other West European countries confirms that poverty is unlikely to have accounted for differences between East and West. Figure 1 compares total marital fertility rates from Wolf's 1980–81 seven-province survey and his analyses of Haishan, Taiwan broken down by ethnicity and wealth, with various European total marital fertility rates calculated for both the 15–44 and 20–44 age groups. Although as Wolf notes there was a positive association between wealth, as measured in land tax

FIGURE 1 Total marital fertility rates in historical Chinese and European populations



SOURCES: Europe, Flinn (1981: 31); Germany, Knodel (1988: 257); England, Wrigley et al. (1997: 355); Haishan Hokkien by land tax, Wolf (1995: 290); Haishan and Hsinchu by ethnicity, Wolf (1995: 120); Haishan, Wolf (1984: 454–455); 1980–81 survey, Wolf (1984: 458–459).

assessments, and fertility, the marital fertility of even the best-off residents of Haishan, largely "rich peasants, landlords, and merchants" (Wolf 1995: 290), was still lower than any European population, even the English between 1650 and 1674. The gap was generally large: Chinese marital fertility was typically 30 percent lower than in the European countries.

We are skeptical that the other, nonphysiological mechanisms that Wolf invokes to account for low Chinese fertility were more important in China than in Europe. For example, Wolf suggests spousal separation as a result of migration to explain low marital fertility in China. We do not believe there is any a priori reason to expect that spousal separation was more common in historical China than in historical Europe, or that Chinese peasants worked harder than their European counterparts. Indeed, in two of the Chinese populations in which we have observed low marital fertility, spousal separation is highly unlikely to have played a role: the members of the Qing imperial lineage were confined to Beijing (Lee, Campbell, and Wang 1993), and the residents of the Liaoning state farms could only migrate legally within the state farm system (Lee and Campbell 1997).

Even if Wolf's views about the influence of nutrition on fertility were correct, it is by no means clear that the Chinese were as poor and

malnourished as he claims. While Wolf (2001) takes as a given the grinding poverty of the Chinese before 1949, there has in fact been vigorous debate over the comparative standards of living in Europe and China, especially before the nineteenth century (Fang 1996; Lee and Wang 1999b; Li 1998; Pomeranz 2000). Although this debate is by no means settled (Brenner and Isett 2002; Huang 2002; Pomeranz 2002), in light of the evidence presented by us and others it seems premature to invoke the extreme poverty of the Chinese to explain any distinct features of their demographic behavior. This, of course, is especially true when the Chinese under discussion are the imperial nobility.

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