

### The decline of fertility once again: a critical note on John Knodel's book and standardized demographic indexes

Hohorst, Gerd

Veröffentlichungsversion / Published Version

Zeitschriftenartikel / journal article

**Zur Verfügung gestellt in Kooperation mit / provided in cooperation with:**

GESIS - Leibniz-Institut für Sozialwissenschaften

#### Empfohlene Zitierung / Suggested Citation:

Hohorst, G. (1982). The decline of fertility once again: a critical note on John Knodel's book and standardized demographic indexes. *Historical Social Research*, 7(2), 50-62. <https://doi.org/10.12759/hsr.7.1982.2.50-62>

#### Nutzungsbedingungen:

Dieser Text wird unter einer CC BY Lizenz (Namensnennung) zur Verfügung gestellt. Nähere Auskünfte zu den CC-Lizenzen finden Sie hier:

<https://creativecommons.org/licenses/by/4.0/deed.de>

#### Terms of use:

This document is made available under a CC BY Licence (Attribution). For more information see:

<https://creativecommons.org/licenses/by/4.0>

THE DECLINE OF FERTILITY ONCE AGAIN: A CRITICAL NOTE ON  
JOHN KNODEL'S BOOK AND STANDARDIZED DEMOGRAPHIC INDEXES

Gerd Hohorst<sup>+</sup>

There is again at present discussion in the Federal Republic of Germany about the fall in the birth rate. The most important contributions to this debate, which constitute 'prognoses' of the development of the population till the year 2000 or beyond do however contain assumptions which are at least implicitly based on the premise that the continual decline in fertility since 1965/66 (probably falsely called 'Pillenknick') was the start of a recent secular process, of a second demographic transition or even merely of a further phase of that phenomenon. The justification for such a supposition does however depend on whether a state of affairs is in existence which led at the end of the nineteenth and start of the twentieth century in Germany to the known decline in fertility. In answer to this ever topical historical-demographic debate, John Knodel recently produced an important contribution.(1)

Before turning to a critical discussion of Knodel's results, a short recollection of the phenomenon in question should be useful. The main characteristics of the process of demographic transition, often described in terms of a theory, are based on empirical facts. The starting point in the literature is the observation that industrialized societies, during a specific phase of their development, experienced a definitive declining in fertility after a preceding historical process of durable declining mortality. The 'critical' period, characterized by an already low and further sinking mortality and a simultaneous high fertility (the phase of demographic transition in a narrow sense(2), has been interpreted as part of a secular process(3) which led to the so-called industrial 'Bevölkerungsweise', after having broken the predominance of the former 'pre-industrial' reproductive behaviour. Finally with the arrival of low fertility and mortality this process is said to have generated an extremely slow population growth, including a tendency towards stagnation. Some dispositions to causal explanation of that transition have remained unconvincing.

---

<sup>+</sup>Address all communications to: Gerd Hohorst, Universität Bielefeld, Fakultät für Geschichtswissenschaft, Postfach 8640, 4800 Bielefeld 1, Fed. of Germ.

They have settled between an extremely economistic position(4), on the one hand, and a narrow demographic one, on the other. The latter position denies any causal relationship between the development of fertility and that of economic variables.(5) The key question, however, is whether or not the decline of fertility has been due to increasingly practiced birth control. The contrary hypothesis supposes that declining fertility is primarily a result of sinking standards of aspired family size together with growing opportunities of realizing family plans. Relevant to the latter issue is the historical experience of a declining and stabilizing child and infant mortality over time. My own supposition is that the first mentioned hypothesis is absurd, at least with respect to Germany, because birth control was already familiar to pre-industrial times. From this point of view it is not necessary to measure fertility development by means of demographic indexes which contain as a measure of reference 'natural' fertilities that are defined by the absence of birth control. Thus Knodel's standardization, which uses Hutterite fertilities (standing for the 'natural' fertilities of a population), would be absolutely unnecessary if there were not that second effect of his standardization, namely, the consideration of variations in the age structure of the childbearing women over time and/or by regions. Because of this argument standardization would be highly desirable as long as no unintentional 'artificial' effects are introduced. But this is indeed the case with Knodel's standardization: this will be the main point of my critique. However, I should first like to begin with Knodel's findings.

In addition to analysing the social and economic factors affecting fertility he rightly places two groups of problems in the centre of his investigations, that is the question of the correct dating of the period of decline in fertility as well as the traditional hypothesis which claims a link between this secular decline in fertility and the reduction in infant mortality, (the latter one must add, becomes more stable).

I should like to criticize the essential points of his summary which has the following answers to the two above questions:

1. "On a national level, between unification in 1871 and the early 1930s, overall fertility declined by 60 percent, material fertility by 65 percent, and illegitimate fertility by 54 percent" (page 246).

And to the quoted hypothesis:

2. "In Germany, unlike the usual description incorporated in the demographic transition model, a decline in infant mortality did not precede the state of the fertility transition" (page 265).

Before I attempt to elucidate critically the various stages in his analysis, let my own judgement at this early stage be placed against Knodel's:

In answer to 1.: This assertion is at best misleading, because until 1900 (in my view) there was at a national level no noteworthy reduction in fertility which could not be suitably interpreted as regular fluctuations in the time series variables. Even Knodel's own figures

(page 39, table 2.1, fig. 2.1) support my point of view; in addition I would however like to prove in what follows that the figures show in all probability a bias towards a too early decision concerning the dating of changes in trend. I further believe that the hypothesis stated is not refuted by Knodel's test.

I answer to 2.: This assertion is supported by a regional cross-section analysis which of course contains the demographic indices as a dependent variable. The correlation analysis (page 148 ff.; 174 table 4.10) which is supposed to be the final verification of the result, is an unsuitable test of the hypothesis because of inter alia the time bias in the indices. Consequently Knodel's assertion is just as unfounded as would be the counter assertion criticized by him without a valid test method.

I shall attempt to substantiate my claim by retracing step by step the analysis which led to these results. Since the analytical approach presupposes the exact dating, Knodel first tries a time series analysis for the 71 German regions as well as for the aggregates: in order to obtain such dates. Afterwards the cross-section analysis by region is established as the test of the quoted hypothesis. Because infant mortality (especially the mortality of babies) can certainly not be considered as real causes but only as indicators of quite definite socio-economic circumstances, Knodel spends a lot of time on a far-ranging discussion of social factors. However, in this matter he does in fact reach some rather unconvincing conclusions - perhaps because, amongst other things, the calculation of his demographic indices reveals flaws in his method which affect both the dating of the start of the decline in fertility and the above mentioned regional cross-section analysis. I will in what follows limit my criticism to three points:

1. The question of the time series analysis by Knodel and the dating issue;
  2. The problem of standardization when employing Hutterite fertility, or to be more precise: the importance of the quantitative independent (eigen) development of the denominators in Knodel's demographic indices and
  3. Knodel's evaluation of the cross-section analysis as a test of the hypothesis.
1. The subject of the analysis is first of all the development of fertility over time. As regards the question of the dating of the final and irreversible decline in fertility, it may be unimportant whether one examines the fertility of the whole population, i. e. whether one considers the real natality of the reproductive years, for which the general fertility rates, the gross reproduction rate or even the net reproduction rate would be the suitable standard, or the fertility of specific groups. The layout of data ultimately determines the evaluation. First it must be critically noted that one cannot talk of a time series analysis in an actual sense, since the processes which in time form the fertility variables are not examined - these would be at least year values of the variables and therefore not the averages of several years. In addition the cohort analysis evaluation is used in its false but nevertheless customary five year period form. Now in the simple dating of the trend changes in the variables it is certainly not a causal analysis and even the exact description contains according to Knodel's evaluation a degree of freedom which places his attempt at dating almost in the realm of the arbitrary.

For Knodel's result is ultimately the result of his decision to allow the irreversible decline in fertility to begin at that point in time when a definite lower value, that is a reduction of 10 % of the value of 1871, is exceeded. The threshold values are not extracted from the analysis but spring from an arbitrary assessment which also requires that all variable values after the dating are below that before the dating. A classical time series analysis would have produced a more reliable basis. What is required so far is an exact description not a causal analysis. At all events it is required of one, who quite rightly does not wish to have anything to do with the so-called theory of demographic transition.

2. A sensible standardization of the variables could be part of the exact description of the progress of fertility within a given period of time. Knodel (6) believes that it is sensible, instead of merely tracing the course of actual fertility, to measure and to use as the basis of the dating the relative changes in 'natural' fertility which would result from the complete absence of birth control. Like Coale he uses as 'natural' the fertility of married Hutterite women aged between 20 and 49 in age intervals of 5 year periods, as researched by Louis Henry. (7) Even at this point it must be critically noted that it is not a question of 'natural' fertility (in the above-described sense) of the population under investigation, but of a time-, place- and culturally unknown fertility as a standardization. One would require a lot of space to completely develop the criticism implicit in this statement. For Knodel's purpose of exact dating first of all it is only important that the calculation creates no artificial eigen (i.e. independent) values which lead to temporal biases. I believe that this is indeed the case. However, for the purpose of demonstration, Knodel's definitions are here reproduced in shortened form. Knodel defines overall fertility

$$If = \frac{B}{W_i F_i},$$

where B is the total number of annual births,  $W_i$  the number of women in the respective age interval and  $F_i$  the Hutterite fertility (8) relevant to the corresponding age interval. Marital and illegitimate fertility are defined analogically. This procedure is discredited by a preliminary test which shows that for the development of marital fertility it is irrelevant which of the possible fertility models one uses for standardization, although the absolute level of fertility depends on it. Between 1875 and 1925 there is in any case a reduction of 57 % (9). This argument is also theoretically correct if the distribution of women into age groups has remained constant, or if all the fertility models used in the structure are basically identical, when the differences between the age specific fertilities collectively give the same sum of the differences in relation to its unweighted arithmetic mean. If however one of the conditions is not fulfilled - and that is an empirical question - then standardization itself leads to time period eigen-developments of the denominators in the fertility indices which have already led to false conclusions

in the matter of the dating. I must now go a little further back in order to prove this exactly. Knodel's 'If' falls on the one hand with the total number of annual births (B). Here we are dealing with an empirical fact. 'If' also falls however if the number of possible births rises; it rises because the standard fertilities are in fact presented in age specific terms (these are always those of the Hutterites between 1921 and 1930), when the empirical - age structure is displaced favouring the fertile female ages. Contrary to criticism of the conventional measures of fertility, Knodel's effect is on this point (10) greatly to be desired, for with an age structure which remains constant any standardization would be absolutely superfluous. The denominator of the index 'If' rises out of all proportion if the age structure alters in favour of the groups with high fertility and at the same time the differences between the group specific fertilities are collectively greater in the denominator than in the numerator. Or expressed in another way: in a given empirical score of the age groups the weighted arithmetic mean of the age specific fertilities varies with the differences, while the unweighted arithmetic mean remains constant. And so it is not a question of the absolute quantity of fertilities but only of its structure and so also of the number of groups. A hypothetical example will serve to illustrate this; hypothetical, because of the here fictional nature of the age structure and the fertility structure of the numerator of 'If' which Knodel for obvious reasons has replaced by its result, the number of births. (11) The process of proof becomes at this juncture rather complicated through a trick of in fact substituting the bare number of births (B) by a complete fertility structure which was formed similarly to that of the denominator. This happens primarily in order to show the isolated effects: if only the (fictitious) age specific fertilities of the numerator are so altered that, from a mathematical analogue with that of the married Hutterite women, they finally contain after alteration to each group the unweighted arithmetic mean (the quotients from the sum of the differences and the unweighted arithmetic mean of fertilities being then equal), then the numerator falls by about 5 % - and with it also the quotient formed according to Knodel's 'If'. What is shown by this, if one returns to the example, is that the denominator in 'If' reveals an eigen variation as opposed to the numerator simply because of the biased structure of the standardization. This possibility of eigen variation comes much more plainly to the fore if the comparison combines a change of the age structure in the course of time with the above-described structural effect of the fertilities. (12) 'If' falls by about 7 % between the two hypothetical points in time with the different age structures because of the effects already outlined. The examples were chosen in such a way that they permitted an eigen movement of the denominator. It may be however that empirically no great eigen movement of the denominator of 'If' attained significance. But in order to make clear the empirical relevance of my argument, let us first bring a few thoughtprovoking facts into the discussion. During the period which interests us here (1871-1900) the number (13) of women rose by 36,9 %, the number of women aged between 15 and 20 by 40,1 % and those aged between 20 and 45 by 35,8 %. As regards the dating

problem, it is especially noteworthy in view of what has been said that this growth structure inverts itself in all conceivable respects in the critical period 1890 to 1900: the total number of women then grew by 13,6 %, the number of those in the mentioned age groups by 10,1 % and 16,7 % respectively. In addition in the years 1899 and 1900 the marriage rate again reached the level of 1876 (14), which after this time was not surpassed until 1930. As illegitimate fertility in the period in question did not fall especially strongly, one would actually expect with the known difference to marital fertility a retarded decline of overall fertility (If) and more than ever of marital fertility (Im) - unless, as Knodel concludes, definite birth control had in fact gained ground specifically in the 1890s. Or: specifically at this time the above-described eigen movement of the denominator of the indices 'If' and 'Im' was extremely pronounced as a consequence of the biased fertility structure of the married Hutterite women together with a clear displacement in the age group structure of German women. In the second case the start of the decline in fertility in Germany would be placed too early by Knodel. Before any decision can be taken as to which of the two alternatives developed is correct, some further facts should be discussed. The overall fertility rate even in the year 1900 had a value of 158, only 3,1 % lower than of 1871 (163) and 5,4 % less than the maximum of the period in the year 1880/81 (167). (15) And not until 1902 did the number of live births hesitatingly begin to fall despite further increases in the number of women in the fertile age groups. Both of these facts indicate that the decline of fertility in Germany did not start before 1900/01.

But in order to corroborate my assertion of the eigen development of the denominator, I would like to discuss a further example which centres around overall fertility and the empirical growth of the fertile ages of women. However, it formulates the numerator of 'If' analogously to the denominator, yet explicitly chooses another age specific fertility structure. In the construction of such a structure, which is in fact impossible because of the data layout of the empirical findings, it is at this point merely a matter of showing what freedom of growth the denominator had in the period 1871 to 1900 independent of the numerator (I refer again to Knodel's indices), if we on the one hand recognize as empirical conditions of the framework the overall fertility rate and on the other hand the quantitative development of the fertile female ages. Within these limits - and that is the decisive point here - as the example shows (16), substantial variations of age specific fertility structure (17), and/or of the age structure of the relevant ages of women are possible. The age specific fertility structure chosen for the numerator has been constructed, as befits the logic of my line of argument, quite consciously as a counter example to that of the married Hutterite women. So I have not bothered to question whether it is empirically plausible in detail as a structure but only paid heed to its result remaining compatible with the overall fertility rate: However, it revealed the greatest possible dissimilarity with that of the Hutterite women. I should like to stress

yet again that I am attempting, within the given empirical setting, to refute Knodel's result using one case which from the data is possibly contrary. The result is, as was to be expected from what has been discussed above, that Knodel's 'If' only describes exactly the chronological course of total fertility when the fertility structure of the numerator in the empirically indisputable change of age structure between 1871 and 1900 is mathematically similar to that of the denominator and that the quotient from the sum of differences of the age specific fertilities and their unweighted arithmetic mean is equal in numerator and denominator. (18) On the other hand I have found that despite a constantly held age specific fertility structure the numerators in 'If' between 1871 and 1900 can fall by 4,5 %, by 1,8 % from 1871 to 1890 and by 2,7 % from 1890 to 1900, if the age structure in this period altered to a large extent but not implausibly in empirical terms. This change in Knodel's index would be diagnosable despite fertility remaining constant, only because of the structurally determined eigen development of the denominator as opposed to the numerator in his index. Since Knodel has considered, along with a series of other authors, that a standardization with the fertilities of married Hutterite women between 1921 and 1930 is necessary, it should, in view of the actual changing age structure of the fertile ages, have been a matter of some urgency to demonstrate that his standard structure revealed that mathematical similarity with the empirical fertility structure in Germany during the time in question. I had set myself the task of proving that a large part of that 10 % reduction of indices for the dating of the start of the decline in fertility as stipulated by Knodel could have been the result of an eigen movement of the denominator at the time in question. Or put another way: if one subtracts that 4,5 % artificial reduction of the index from 10 %, but retains the 10 % criterion itself, the start of the 'genuine' decline in fertility would have to be dated later, because a reduction of 10 % would then actually not be attained until a later point in time.

Besides the purely formal demographic arguments, several points could in fact be discussed. With the standardization used it is noticeable that it contains a fertility structure which was valid for married women if one simply ignores the still unattained level of Hutterite fertility. Knodel himself had to complete the standard for the group of 15 - 19 year old women, since the Hutterites showed no fertilities for these ages. Even if it were questionable whether 1.000 women of these age groups within the 5 years they remained in this age interval really had 300 births as opposed to 'only' 550 of the next age interval, the value estimated by Knodel is certainly in agreement with the structure, as a lower value would have raised the sum of the differences and with it the possibility of an eigen movement of the denominator; the very necessity for completion impressively shows however that with the Hutterite fertilities it is a question of a standardization which is completely alien to the German population between 1870 and 1940. Meanwhile the difficult question is whether a substantial



rise of the marriage rate in the relevant period does not in itself suffice to impart a dynamic to the mixture of legitimate and illegitimate births in the numerator of 'If', which because of its incalculability makes for the extremely variable interval between a standardization reflecting the 'natural' fertility and a constant actual and lower fertility; an interval which would have to remain constant under the mentioned condition. For this reason with 'If' a part of the development of the denominator could also be compensated by a growth of the numerator, dependent on the fact that the overall fertility was parried for a time by the rise in the proportion married, that is when birth control was not consolidated. For marital fertility this effect is certainly not possible. Since, according to Knodel's analysis (Table 2.1, page 39), only marital fertility (I<sub>g</sub>) between 1871 and 1900 in fact sank by more than 10 %, it would have been important to represent the time development of numerator and denominator of the indices separately, so that it is clear whether the sinking of the index at that time results predominately from a falling numerator or from a rising denominator.

In summary I would like to place my unease with Kondel's dating into a conceptual framework with a plea from the existing literature: Pressat mentions (Page 195) that the results of analysis which are based on age specific fertilities, are to be treated with the utmost caution in Malthusian populations. However, in the case of the German population at the end of the nineteenth century we were dealing with a population with birth control, for this was practised much earlier here. If one looks for the start of a secular decline in fertility in such a population, then one must also pay regard to the relationship of planning and the realization of planning of desired family size and the feasibility of the relevant birth control method, as well as to the possible methods of calculation. I believe that the start of the decline in fertility in the whole of Germany can not be dated before 1900, because a high and strongly variable infant and child mortality impeded any effective family planning.

Perhaps the so-called 'Pillenknick' of a later period, similar to the fall and levelling-out in infant and child mortality from 1900/02, has been the reflex reaction to the suddenly afforded opportunity of carrying out more exact family planning. Or put another way: I believe that even such an apparently simple question like the exact dating of the development of fertility can not fail to take into consideration factors which, as intervening variables, link together family planning and the possibility of its execution without being in the meantime causal factors themselves. This is also the deeper reason for my view that in another respect 'problem-free' standards like the Hutterite fertilities are not applicable in the analysis of a society with family planning and birth control, especially because they contain a 'natural' generative behaviour completely alien to that society as though a paradigm.

3. Despite the remarks made on the dating problems for the whole of Germany, it remains clear that there have been quite substantial regional differences regarding the start of the decline in fertility.

Were there however also similar differences in the levels and variations in infant and child mortality? The criticism of Knodel's cross-section analysis as a test of the hypothesis formulated by following closely Wappäus can be short, as the arguments already fully developed here only receive another base of reference.

The connection between infant mortality and fertility is actually a time series phenomenon. It may only be seen at a regional level and processed with a cross-section analysis, because the developments in the regions do not occur simultaneously and so at a given point in time there is a predominance of differences, which basically were differences in development: simultaneousness of the non-simultaneous! Of course there are regional differences sui generis. They are however unsuitable for confirming the so-called connection. For fertility and infant mortality are linked in the relevant direction here through a reaction of fertility to developments in infant mortality in the course of time. (20) Once this is acknowledged as valid, it is also immediately clear why the eigen development of the fertility rates (the standardization) brings a bias into the cross-section variables. With the given standardization (the Hutterite fertilities) the significance of the denominator depends only on the age structure of the decisive women's ages and moreover the age structure revealed far greater differences in the regional than in the time series comparison.

The inevitable conclusion is that the test with the standardized fertility rates is completely unsuitable for examining the quoted hypothesis. The regional differences of the index values stem from two basically different sources, from 'genuine' fertility differences on the one hand and those which on the basis of differences in the age structures affect the respective value of the indices as artificial eigen differences of the denominator. Even if the hypothesis were validated one could not expect the corresponding (high) correlation in this evaluation.

It would be wrong to consider my criticism of some points to be a fundamental criticism of Knodel's important work. Quite the opposite is true. Knodel's book uses, as far as I can see, not only all available material on the undisputed important question of the decline of fertility in Germany, but is to a high degree sociologically inspired in both theory and method. The author's remarks too on the question of dating do not rely completely on analysis of the index but bring other indices into the discussion. Altogether the treatise ultimately deals with the points raised here in a far more diverse way than it appears on a superficial reading. Of course Knodel does not provide in his summary any suitably differentiated representation of his results. Instead he offers definitions like the one quoted at the outset which in their simplicity are over-stretched against the background of the overall analysis and lead to misinterpretations. A more general conclusion drawn from the special effects of standardized indexes would be rather trivial: before rushing at standardization work demographers should ask the cardinal question whether or not standardization will be necessary.

It is definitely not necessary if one is only interested in natural population increase including the general fertilities of the population under investigation. It will never be necessary when current work on family reconstitution does generate the empirical fertility structures, because in my view population history of societies has nothing to do with 'natural' fertility.

#### FOOTNOTES

- 1 See John Knodel, *The Decline of Fertility in Germany, 1871 - 1939*, Princeton 1974.
- 2 Wolfgang Köllmann, *Gesellschaftliche Grundlagen der Bevölkerungsbewegung*, in: Franz-Yaver Kaufmann (ed.), *Bevölkerungsbewegung zwischen Quantität und Qualität*, Stuttgart 1975, page 20-29,25.
- 3 See Gerhard Mackenroth, *Bevölkerungslehre. Theorie, Soziologie und Statistik der Bevölkerung*, Berlin 1953.
- 4 See eg. H. J. Habakkuk, *Population Growth and Economic Development since 1750*, Leicester 1971, page 58. Habakkuk's Argument does differentiate in a suitable way but nevertheless it is not convincing with respect to this question.
- 5 See Köllmann, loc. cit., passim.
- 6 Ibid. Page 33 ff.
- 7 Henry's research has shown that these fertility structures were valid for the years 1921 - 1930. See Knodel, page 33.
- 8 As no relevant Hutterite fertilities exist for the age group 15 - 19, Knodel estimates this to be 0,3. The other fertilities are:

Age	Fertility	Age	Fertility
20-24	0.550	33-39	0.406
25-29	0.502	40-44	0.222
30-34	0.447	45-49	0.061

Fertility here and in the following is defined as births per women.

- 9 Ibid., page 35.
- 10 See Roland Pressat, *Demographic Analysis*, London 1972, passim especially page 188 ff., 352 ff.
- 11 Here we are dealing with firstly a demonstration of the effect of an age structure which stays the same but which nevertheless reveals a higher score in the second age group. Weighted and un-

weighted arithmetic means of the fertility structure are therefore not equal. Two quotients are then compared which were both formed in a similar way to Kondel's 'If'. The difference is simply that the numerator just contains the absolute number of births and the other sign the number of births which would result if one calculated it from a fertility structure. Although both empirically would be have to be equal, they are not in this case, as we are here discussing a purely theoretical effect with a corresponding example.

The difference of the two 'If' of 5 % results in this case from a higher numerator value obtained when using in the numerator the mentioned fertility structure according to age groups. And there is only a difference if at least one age group has a higher score than the others, as a difference only exists then between the weighted and unweighted mean. Since this is not an empirical argument, it was processed with the following fictitious data:

1. The 'structure'

<u>age</u>	<u>fertility</u>
15-19	0.112
20-24	0.205
25-29	0.187
30-34	0.167
35-39	0.152
40-44	0.083

2. The 'age distribution'

<u>number of women</u>
100
200
100
100
100
100

The final -unimportant - group has been omitted. The unweighted arithmetical mean of the fertility amounts to 0.151; using the total number of women would give us the absolute number of births. But we are using here a fictitious age distribution and not the empirically given total number. So much for the comparison between structured and unstructured fertility with an unequal age structure. A further effect is discernible if, with an identical unequal age structure, we compare mathematically dissimilar fertility structures in numerator and denominator. The overall effect ist intensified when all age groups with high fertility show a higher score compared with those with a lower fertility.

12 The mathematical similarity mentioned in note 7 now plays a decisive role in the eigen movement of the denominator as opposed to the numerator in 'If', when the age structure between the compared points in time is substantially altered. Then and only then does the influence of the sum of the differences in the fertility structure on the difference between the weighted and unweighted arithmetic mean fully come into play. Whilst with similar fertility structures in the above mentioned sense no influence of the alteration in the age structure on the difference between weighted and unweighted means is discernible in the time scale, it is fully apparent with dissimilar fertility structures.

In the above effects we are dealing with possible eigen developments of the denominators as opposed to the numerators in Kondel's fertility indices. Whether they actually come into being is an

empirical question, especially as regards the actual change in the age structure of the fertile female ages and/or of the mathematical similarity of the actual age specific fertility structure of the same, which Knodel has used for standardization. Whether these effects are also empirically possible and at the same time plausible will be discussed in the final example. (compare note 16).

- 13 For the figures see Gerd Hohorst, Jürgen Kocka, Gerhard A. Ritter, Sozialgeschichtliches Arbeitsbuch II, München 1978<sup>2</sup>, page 23.
- 14 Ibid., page 29.
- 15 Ibid., page 32.
- 16 In this example it is matter of showing that an empirically plausible change in the age structure between 1871 and 1900 together with an equally inconceivable age specific fertility structure of the numerator in Knodel's index 'If' leads to an eigen development of the numerator, which - here 4,5 % - can be of considerable importance. We are dealing with one of the conceivable and at the same time empirically plausible cases, which contradict Knodel's assertion.

The following development of the age distribution of women between 15 and 45 years was compiled with reference to early available data, as the empirical age distributions were not available to me.

Year	Number of women Score of age groups aged from...to...years.						
	15-19	20-24	25-29	30-34	35-39	40-44	Total
1871	1893	1689	1589	1489	1379	1309	9338
1890	2410	2166	2080	1646	1646	1126	11075
1900	2635	3000	2900	1859	1200	1174	12766

The total and the score of the first age group are empirical figures, the score of the other groups was estimated.

The following structure was estimated as an age specific fertility structure of the numerator which should, in the spirit of the example of those of the Hutterite women, be as dissimilar as possible, but should not however contain the unweighted arithmetic mean in all groups:

Age group	Fertility	Age group	Fertility
15-19	0.155	30-34	0.155
20-24	0.170	35-39	0.155
25-29	0.170	40-44	0.155

Since the structure for all 3 estimated years, 1871, 1890 and 1900 had to be the same, it was so chosen that its unweighted

arithmetic mean, 0.160, corresponded to the mean of the general fertility rates for the years 1871/72, 1890 and 1900/01 (see Hohorst *inter alia* page 32). As expected neither a constant age structure nor a fertility structure mathematically similar to that of the Hutterites (with a varying age structure) produced the eigen movement of the denominator. It was only produced from a combination of the age distribution varied by time of the fertile women's age groups with the represented fertility structure of the numerator which was fictitious and dissimilar to those of the Hutterites. The result was unequivocal:

Year	'If'	Weighted arithmetic mean of the numerator	Weighted arithmetic mean of the denominator
1871	0.392	0.160	0.408
1890	0.387	0.161	0.416
1900	0.378	0.162	0.429

It is clear that the decline of 'If' is mainly brought about by the over-proportional growth of the denominator.

This should show what it was intended to show - that an empirical age distribution could exist which so deviates from another later one that the denominator increases more strongly than the numerator, if there is a constant age specific fertility structure which produced the empirically verified absolute number of births.

Knodel sees the possible effects which were described by Coale. But he omitted to clarify their empirical effectiveness during the period he was investigating, as he also presented the development of the numerator and denominator of his indices separately.

- 17 I would like to point out that these are only inserted for the numerator while the calculation of the denominator follows Knodel's method.
- 18 Of course even with a time constant age structure of fertile women's ages an exact description would be quite independent from the standardization used - from the denominator in 'If'.
- 19 In general rationality of family planning is a major finding of the new literature on proto-industrial populations. See Peter Kriedte, Hans Medick, Jürgen Schlumbohm, *Industrialisierung vor der Industrialisierung*, Göttingen 1977, *passim*. A more special proof of my assertion will be the well-known relationship between birth and rhye prices. With regard to the Prussian case see Gerd Hohorst, *Regionale Entwicklungsunterschiede im Industrialisierungsprozeß Preußens - ein auf Ungleichgewichten basierendes Entwicklungsmodell*, in: Sidney Pollard (ed.), *Region und Industrialisierung*, Göttingen 1980, page 215-38, 224 ff.
- 20 See eg. Gerd Hohorst, *Wirtschaftswachstum und Bevölkerungsentwicklung in Preußen 1816 bis 1914*, New York 1977, *passim*.