

## Historical Note

DR. MED. H.C. WILHELM HILLEBRAND

**Editorial note.** *The recent death of Dr Herman Knaus, co-author of the rhythm method of fertility control, recalls an incident which brought to my knowledge an interesting sidelight on the supplementation of the Knaus-Ogino arithmetic methods by daily measurements of basal body temperature.*

*In 1963, the World Health Organisation, for the first time in its history, convened a Scientific Group on the biology of human reproduction and later in that year I had the privilege of presenting the report of the group to the Organisation's Advisory Committee on Medical Research. During the lively discussion which followed, Professor H. Hamperl of Bonn University, a member of the Committee, asked me whether I knew the story of the village priest who was one of the first to use the BBT method for the detection of ovulation. I had to admit ignorance, and subsequently Professor Hamperl most kindly sent me a reprint of a paper by Professor K. G. Ober which had appeared a few years earlier. This paper gives a most interesting account of a little-known slice of biosocial history and is of additional interest in emphasizing that the biphasic nature of the BBT curve in women was known, and even its relation to ovulation suspected, 30 years or more before its application to fertility control. And, incidentally, it is intriguing to find a parish priest giving an exposition of biological variation to medical men. The text which follows is a slightly edited translation of the greater part of Professor Ober's paper, published in *Geburtsh. Frauenheilk.* (1920) 20, 188, and I am greatly indebted to the author for permission to make this use of his material.*

A.S.P.

DR. MED. H.C. WILHELM HILLEBRAND

K. G. OBER

Wilhelm Hillebrand, parish priest of Schevenhutte, died in Aix-la-Chapelle on 19th July 1959. On 8th July the Medical Faculty of the University of Cologne had conferred an honorary degree on him.

Who was W. Hillebrand, and which problems ultimately affected his life? We shall try to answer this question, as far as possible, by quoting his own words.

His father was a physician. He was born on 27th January 1892 in Titz, Kreis Julich, the second of eleven children. He took Holy Orders in Cologne in 1915. He was a chaplain in Eupen until 1922, then in Aachen-Burscheid until 1929. In that year he became parish priest of Rott, near Aix-la-Chapelle. His uprightness

and integrity led him into difficulties with the National-Socialist authorities and he was forced to leave his parish. In 1938 he took over the parish of Lohn, which saw heavy fighting during World War II. He had to leave the parish for a time, but returned after the war. In the last years of his life he wished to devote his time in a small parish to those duties which were particularly close to his heart. He thus became the incumbent of Schevenhutte in 1953.

As a priest, he was soon faced with the problem of unwanted conception and the prevention of pregnancy which is, as is well known, opposed in its usual forms by the Catholic Church.

In 1926 he wrote to his brother: 'Married people, who confess to having infringed matrimony within the Catholic meaning of the term, regularly express their bitter remorse. They then solemnly promise, as a condition of absolution, that they will mend their ways, and will not sin in future. At the next confession the whole process is repeated!'

He told us once, how welcome were to him, under those circumstances, the results of the researches of Ogino and Knaus. Already in the 1930s he started giving advice according to Knaus' recommendations. He had both successes and failures. In a letter to H.K., dated 20th April 1958, we read:

'I have observed three pregnancies, in short succession, in couples who had received careful instructions from me in your teachings and instructions of natural contraception. These pregnancies occurred in days which fell into the infertile period, according to your teachings, but which fit exactly into Ogino's period, when according to him, conception is rare, viz. 20-24 days prior to menstruation. These cases staggered me, and led me to give thought as to how one might devise a simple, yet safe, way of determining ovulation dates in both these and, eventually, also all other women. I then remembered having read some time ago in van de Velde's *Perfect Marriage* a chapter entitled 'Rhythmic phenomena in the female organism' in which he stated that the hormone of the corpus luteum exerts, amongst others, a characteristic influence on the body temperature of women. This caused me to determine, in as many women as possible, their monthly temperature cycles with the aid of a clinical thermometer. During the relatively short period from August to December 1935, twenty-one women, married and single, were investigated. They had recorded seventy-six monthly cycles. I soon found in one woman that there was a deviation from your norm, that ovulation occurs 15 days before menstruation. In her case ovulation occurred 19 days before menstruation. This upset me greatly. I expressed doubts, and reproached her with not having kept sufficiently accurate measurements. However, the impression made by her as a person and the way she put her case were overwhelming. A further observation finally convinced me. I am as certain now as I ever was, that you have discovered the purely physiological and normal ovulation period, but I recognized then that deviations must occur, both at the beginning and at the end of your norm, and that therefore one must not adhere blindly to it.

Thus was born the method for verifying the ovulation period by means of taking the awaking temperature, which forms part of a fool-proof natural contraceptive method.'

Many of these graphs from 1935 still exist. What was the significance of that experience at that time? Van de Velde had already described in 1904 the biphasic temperature curve and had interpreted it as representing a cyclic phenomenon in the female organism. In 1926 he expressed the opinion that the low-point in the temperature curve, about the middle of the cycle, was related to ovulation, but that premenstrual hyperthermia is related to the functional span of the corpus luteum. Van de Velde's well-known book has had a large readership. Many gynaecologists have read it, some have reviewed it in medical journals. However, at that time, the profession was not inclined to draw conclusions from this work. As far as we can ascertain at present, in addition to Hillebrand, only Harvey and Crockett in 1932 and T. T. Zuck in Cleveland in 1935 expressed any interest in the relation of awaking temperature to fertility in woman. It was years before the phenomenon of the biphasic temperature curve was again taken up in the German literature by Vollmann, in 1940. Large-scale trials of this method were, however, only started abroad during World War II. For the last 12 years it has gained increasing recognition in Germany. One ought not to forget this historical background. Today there hardly exists a physician dealing with gynaecologic-endocrinological problems who does not measure the awaking temperature, even when he has the most elaborately equipped laboratory at his disposal.

Nor should it be forgotten that W. Hillebrand submitted his first graphs and the results of the advice which was based on them to various doctors at an early date. Initially, he was rebuffed everywhere. His priesthood put him in an awkward position. Theologians rejected his efforts; physicians were not interested.

At a time when academic medicine rejected or at best took up a sceptical attitude towards the doctrine of periodic female fertility, W. Hillebrand believed in it; he attempted to make use of it in marriage guidance but, above all, he was the first to recognize the significance of the awaking temperature in marriage guidance. When Döring reported in 1950, for the first time in Germany, on 526 cycles in sixty-five fertile women, W. Hillebrand provided him with part of his material. Over the years there accumulated at Schevenhütte many letters from married couples thanking him for having shown them an acceptable way out of a dire situation.

Two questions occupied his mind during his last years. They were the development of a practicable instruction for married couples, and a consideration of his conception of pre-ovulation by those researchers who investigated the length of the luteal phase.

On 6th June 1957 he wrote to H.K.:

'I am concentrating my efforts on the problem of how married couples may be most easily and safely introduced to and inspired for natural birth control.

This cannot be achieved by means of popular or scientific writings, although these have their use, but only if an expert takes the trouble to instruct these people, and that not generally but individually, until they are capable of dealing with their own particular situation by themselves. If this includes a compilation of a menstrual calendar it will be even more necessary for the fertile days to be checked by means of the awaking temperature. For instance, your article "The fertile and infertile days of woman and their correct significance" would not be of any practical help to a wife. She will blunder along on her own for some time, and then she will tire of the whole business or, what is even worse, her contraceptive attempts will come to grief.'

An extract from a letter (5th January 1948) to the present writer reads: 'Both the Knaus-Ogino method and taking the awaking temperature present advantages as well as disadvantages. It thus seems obvious that the two should supplement each other. This seems so evident to my mind that I can only look with amazement at the fanatical temperature takers who will eventually, I am certain, change their minds. I was the first to pioneer the checking of fertile days by measuring the awaking temperature and I have been chastened by many failures due to the pre-ovulatory period so that I, if anyone, would have had grounds for changing my views. Yet, this thought never occurred to me even remotely. Nor will it do so in future. My basic concept is as follows: Knaus determined the physiological, and therefore normal, ovulatory period. Ogino has discovered as well the ranges of anomalous ovulation periods. The method of temperature measurements, on the other hand, provides us with a relatively simple method of determining which women have normal and anomalous ovulatory periods, and to what extent.'

One of his last letters ended as follows:

'It has always been my life's task to break a lance for a combination of temperature measurement and the Knaus-Ogino method. Above all, this means that for the last 25 years I endeavoured to construct and try out the necessary charts for recording the combined method of natural birth control. I think I have reached my goal. My instructions are practicable. I should like to submit them for testing by a suitable group.'

It was not to be. His papers are in the theological seminary at Aix-la-Chapelle, and include the draft, ready for the press, of instructions for married couples, which he drew up in the last weeks before his illness. They would be of great help to those who wish to continue his work. The laborious, individual advice which he dispensed will show no tangible results in the foreseeable future without benevolent men of his stamp. In the long run, only further experience will be able to show whether, with such an important human problem, the path which W. Hillebrand took during the last 20 years of his life is practicable.

A few days before his death he told us:

'I was always fully aware that I was in step with Catholic moral theology and

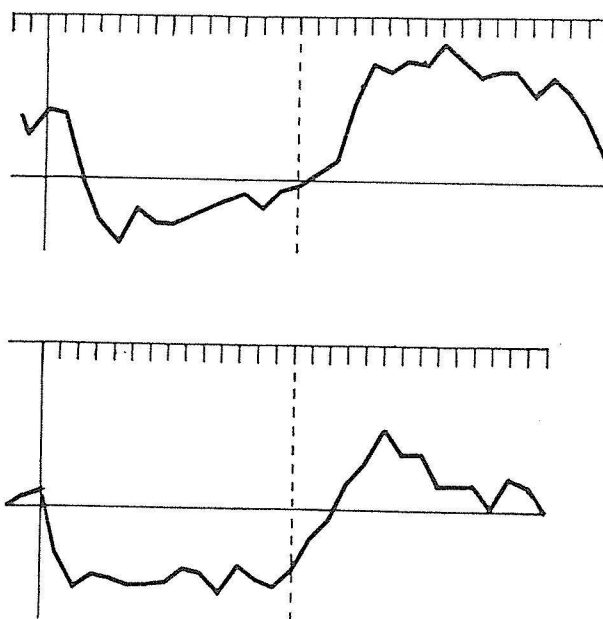
that I tried to improve Catholic marriage morals, in the sense advocated by Pope Pius XII.'

He had satisfactory contacts during his last years with Knaus whose picture stood on his writing table. Knaus was responsible for putting Hillebrand into contact with the present writer. I was to help him with the scientific background for his second problem, pre-ovulation. The necessary clinical investigations could not take place, however, since circumstances (pregnancy of one woman, illness of another woman's husband) made these observations impossible at the time. What were his aims? On 4th December 1956 he wrote to H. Dietel:

'I believe I shall render you, as well as Professor Knaus, a welcome service, as well as a kindness, by reporting to you in detail an observation which I made as early as 1935, and whose cause was already apparent to me at the time. You must, however, be kind enough to make allowances for certain peculiarities regarding the manner of recording these experiences. As to the cause, we are dealing with that peculiarity of a minority of women which I should like to call pre-ovulation, in contrast to the so-called early ovulation. Whereas 'early ovulation' indicates ovulation which seen in relation to the shortest usual cycle occurs early; pre-ovulation deals with the lifespan and function of the corpus luteum. As this span is sometimes shortened for pathological or other reasons, thus leading to early menstruation, one cannot disregard the possibility that for similar though different reasons, it is lengthened, thus delaying menstruation. One might, therefore, speak of early and late menstruation instead of, or in preference to, pre- and post-ovulation. According to Professor Knaus, the interval between ovulation and menstruation is, without exception, 14 days under ideal physiological conditions. It has been long known and admitted that, occasionally, it is less than 14 days; it is much less well known that this interval may be increased, instances have been recorded, in the absence of pregnancy and without it being necessary to adduce the phantom and awkward hypothesis of an ovulation induced by sexual stimulation or by some other means.'

Several tables and graphs accompany these lines. They come from mother and daughter. The former conceived in 1935, 4 days earlier than would have been consistent with the hypothesis of a constant luteal phase. After birth, the awaking temperature was measured from 1936 onwards. W.H. obtained similar graphs from the daughter 21 years later. The illustration shows two mean value curves, drawn by W.H. The upper curve shows the average of five successive cycles of the mother for 1936, the lower the mean values for four successive cycles of the daughter for 1957.

Over the years Hillebrand observed six women who, in successive cycles, consistently showed premenstrual hyperthermia of more than 15 days duration and in whom, above all, the intermenstrual temperature low-point occurred more than 17 days before the beginning of the menses. This phenomenon has not hitherto been recorded as representing a typical cycle variety in a woman,



especially as one recurring in mother and daughter. In order to rescue this observation from oblivion we hereby present it for the benefit of physicians.

In the spring of 1959 Hillebrand was taken gravely ill. Four weeks before his death we learned that the end was near. Eleven days before his death the Albertus Magnus University of Cologne conferred an honorary doctorate on him. 'All medicine is love', said Theophrastus von Hohenheim. Wilhelm Hillebrand had the gift of grace. It redounds to the honour of the University of Cologne that it has recognized this fact.

**Editorial footnote.** *The mention of Professor Döring's name prompted me to write to him to ask whether he knew of Professor Ober's paper. In reply, Döring said that he had seen Ober's paper before it was published and agreed entirely with the contents. He went on:*

*'I knew Reverend Hillebrand personally. After my first publication on the temperature-method in Geburtsh. Frauenheilk. (1949) 9, 757, I received a letter from him with an invitation to visit him. He showed me a lot of temperature charts and letters on this subject. I was very impressed by him and I am still today. In fact he was the first man in the world who used the changes in basal-body-temperature, published by van de Velde in 1904, practically since 1935, in order to advise women about the safe period. I saw his correspondence with Knaus who did not recognize at that time what the temperature method meant generally and especially for his theory on the periodicity of fertility. I remember quite well a conference in Sankelmark in 1956 on problems of interruption of pregnancy and birth control. Knaus mentioned then the letters of Reverend Hillebrand and said literally "What a fool must I have been not to have*

recognized the importance of these curves" [in German: *Was muss ich damals für ein Brett vor dem Kopf gehabt haben . . .*]. The problem was, that he had plenty of time, and I, as an Associate-professor at a big University Clinic in Obstetrics and Gynaecology had very little.'

This emphatic comment, for which I am much indebted to Professor Döring, appropriately concludes this account of the remarkable contribution to human biology made by Wilhelm Hillebrand, parish priest.

A.S.P.