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Essen and the National Physical Laboratory's atomic clock

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Abstract
To commemorate the fiftieth anniversary of the development of the first atomic frequency standard, we present some notes about the work of Louis Essen at the National Physical Laboratory. In addition, we publish below some personal recollections of Essen on his work, which have previously been available only on the Internet (http://www.btinternet.com/~time.lord/TheAtomicClock.htm).

Louis Essen joined the National Physical Laboratory (NPL) in 1928, having graduated with first class honours from University College, Nottingham. He worked with D W Dye on 'tuning fork' clocks, eventually focusing on the use of quartz oscillators and designing the quartz ring resonator. The impetus given to radio frequency techniques during the war years enabled Essen to become an expert in microwave synthesis and resonators, leading to his work in determining the speed of light [1]. Thus he was well placed to make a large contribution to the development of atomic time-keeping when the opportunity arose.

Although Essen has stated that spectroscopy was far from his field of work, it is obvious, on reading his publications, that he was adequately versed in those parts of the field that were relevant to accurate spectroscopy of alkali metals. He makes frequent references to the seminal work of Ramsey [2] in his theoretical introductions to his reports.

Modern caesium primary standards have an accuracy, at 1 in 10^{15}, some five orders of magnitude greater than what was achieved by Essen. Many more systematic frequency biases have been uncovered than were known in Essen's days. Nevertheless, Essen's publications show that he was aware of all those that were relevant to his apparatus, at the level of stability achievable then. The techniques he applied for quantifying these biases are largely unchanged today: having determined the stability with reference to quartz oscillators of comparable short-term performance, he quantified the Zeeman shift using transitions that are linearly dependent on magnetic field and investigated the effect of non-uniform field; he explored the effect of cavity phase difference in the Ramsey cavity by the reversal technique; he quantified the effect of microwave power variation; he developed microwave apparatus [3] with controlled spectral impurities to investigate their contribution to the bias; he quantified the Doppler effect and the effect of background gases; and he set an upper limit on the bias due to the Millman effect. Essen's publications show that, by quantifying all the known biases, he had indeed constructed what has come to be known as a primary frequency standard. Of course, the second being defined at that time in astronomical terms, the apparatus could not officially be known as such.

The decades leading up to the atomic frequency standard had seen timescale development, based on a combination of quartz oscillators and astronomical observation, reach maturity. There was little prospect of further significant improvement of stability. NPL operated 100 kHz quartz oscillators based on Essen's design and combined these with time signals from astronomical observatories. Two of the quartz oscillators, designated Q_{13} and Q_{26}, were among the most stable standards of the era.

In August 1957, just over two years after Essen had first operated his frequency standard, he published his measurements of the quartz ring oscillators and of astronomical time against the caesium second [4]. Essen had demonstrated a standard deviation for measuring a quartz oscillator of 1 in 10^{10}. A period of some days was required to check the biases of the caesium standard, and the instability of the quartz oscillators in this period limited the accuracy of relating the quartz-derived time interval to the caesium second to 2 in 10^{10}.

Over two months in 1956 Essen had quantified the stability of the quartz-based measurement of time interval at NPL; the peak-to-peak departure from a linear drift model, with a frequency of Q_{31} and Q_{26}, when measured against the caesium second, was found to be within ± 2 in 10^{10}. The data indicated that a large part of this spread was accounted for by the instability of the quartz devices. He noted that each measurement, being the mean of twenty readings, had taken some ten minutes. The best precision of astronomical time

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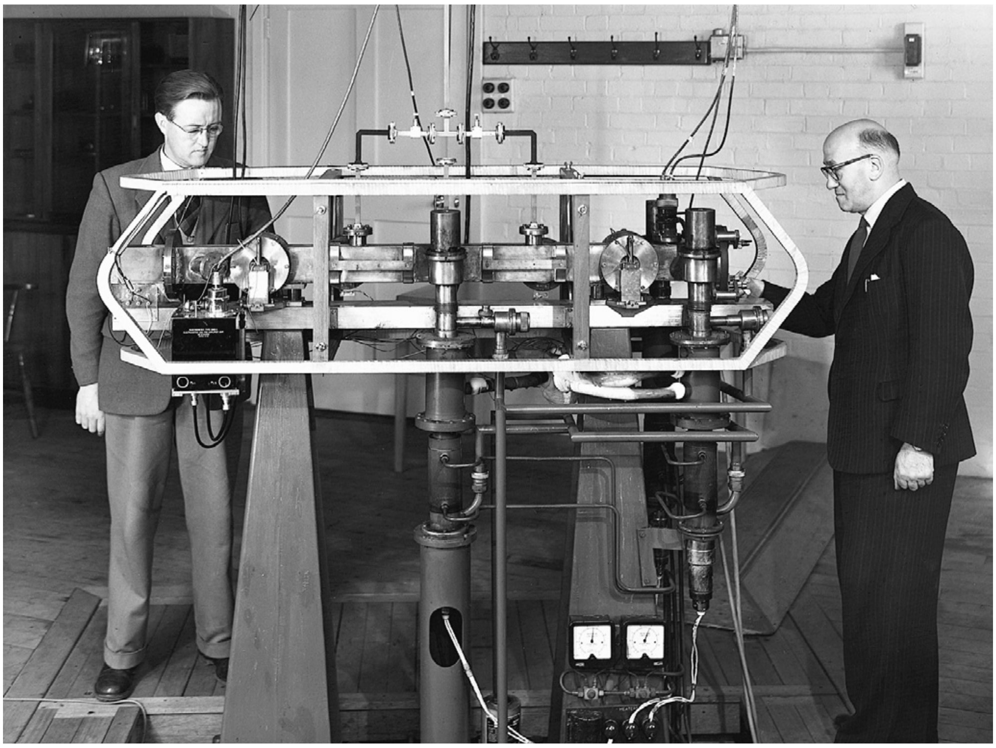

Figure 1. Essen (right) and Parry with their atomic frequency standard. (Crown copyright 1960. Reproduced by permission of the Controller of HMSO and the Queen's Printer for Scotland.)

that could be achieved in a single night (using a photographic zenith tube) was some 200 times poorer; by extending the astronomical observation over a month the accuracy could be improved to the extent of 1 in 10^9.

From June 1955 to January 1957 the caesium second was compared with the second of universal time (UT), as realized by the Royal Greenwich Observatory and the US Naval Observatory (USNO). The results demonstrated the superior stability of Essen's caesium frequency standard. The two astronomical realizations of UT displayed a stability of their monthly average at the part in 10^9 level. The data enabled Essen to measure the caesium second against UT2, averaged over the period from June 1955 to June 1956, with an uncertainty of ± 2 in 10^{10}. Clearly evident in the data was the drift in the rate of UT compared with the caesium second, of a few parts in 10^9 per year. The NPL measurement of time interval, based on caesium-disciplined quartz, was shown to be an order of magnitude more stable than the measurement of time interval based on astronomical observation.

Some months after Essen's first operation of his caesium standard, ephemeris time (ET) was adopted for the definition of the second. The reasoning behind the redefinition was the greater potential stability of the second of ET compared with UT. Dr W Markowitz of the USNO had been working on a means of relating the second of ET to the realization of UT at the USNO, and he and Essen collaborated to determine a value for the caesium second in terms of ET. The result, based on caesium measurements taken only on Essen's apparatus, was announced in 1958 [5]. The frequency of the caesium resonance was determined to be 9192 631 770 Hz with an uncertainty of ±20 Hz.

The 1957 paper [4] contains some references to previous measurements of the caesium frequency, both published [6] and unpublished. The measurements by Sherwood and Lyons agreed within their uncertainty with those by Essen, but that uncertainty, at between one and two orders of magnitude greater than in Essen's apparatus, was not an advance on what could be achieved by the combination of astronomical time and quartz oscillators. It is for this reason that 1955 marks the dawn of atomic time.

Louis Essen recorded some recollections of his life and scientific career in 1996, one year before he died [7]. They include an account of his contribution to the development of the atomic clock and his efforts to have the 'atomic' second accepted. How better to complete this review than in the words of Essen himself? (These recollections are taken from [7].)

Louis Essen's account

'Quartz clocks had revealed an annual periodic variation in the time of rotation of the earth and hence in the value of the astronomical second of time. The stability of the clocks enabled them to smooth out these variations as well as other small irregularities in the earth's motion and in the errors of observation but they could not be regarded as standards. They were adjusted to keep time with the earth and their virtue was that once adjusted they operated very stably from year to year providing a time scale which could be read with great precision. Little was to be gained by improving them still further. It was the fundamental standard itself which was no longer adequate for modern practical application in the fields of air navigation and communication.

The development of microwave techniques during the war provided spectroscopists with a powerful new tool and enabled them to study the response of atoms to electromagnetic waves covering a whole new band of frequencies. Atoms of the alkali metals were of particular interest because they have a single electron in the outermost orbit and, therefore, give the simplest spectrum. The results were brilliantly interpreted and led to the assumption that the outer electron and nucleus were spinning either in the same or in the opposite direction and that the two conditions represented states having slightly different energies. Transitions between them were accompanied by the emission or absorption of a (quantum of) radiation in the microwave region of the spectrum. The significance of this from our point of view is that the frequency can be measured in terms of the quartz standards and it becomes possible to define the second as the time occupied by a certain number of cycles of an atomic spectral line. It was suggested by Rabi that a spectral line of an isotope of caesium might be suitable. Its frequency was near 10^{10} Hz in a band used for radar.

Early work in US

I was naturally interested in these developments although spectroscopy was so far out of my field that I did not expect to take an active part, that is, until I visited the USA in 1950 and saw the work at MIT and Columbia University. Zacharias at the MIT was quite enthusiastic and although he was not interested in clock making himself he was confident that his technique could be developed to form the basis of a time standard. The microwave spectral lines are so feeble that they cannot be detected directly, but use is made of the magnetic properties of the atoms arising from their spins. They behave like tiny magnets which are deflected in a non-uniform magnetic field; and if they are subjected to another field oscillating at the frequency of the line they are induced to change to the other energy state, the polarity of the magnet is reversed and they are deflected in the opposite direction. He knew of our expertise in the microwave work and thought that we would not find the atomic beam technique too difficult. A peculiar feature of these spectral lines is that their sharpness depends on how quickly the transition takes place—the slower the better. For a useful clock the time would need to be much longer than that used in the MIT experiments so that the atoms would have to travel a longer distance in a highly evacuated enclosure. I was not sure that a sufficiently high vacuum could be obtained but he pointed out that we had in the UK two of the world's best manufacturers of vacuum equipment.

At Columbia University, Kusch expressed similar views although his colleague Townes favoured the use of ammonia, which had a much stronger spectral line, using a new technique. This became the maser which was studied in many laboratories as a standard without much success. It did, however, lead to the laser with its revolutionary applications. From my experience I decided that the atomic beam using caesium was the best option. The main problem arose from the high accuracy required. In spite of its shortcomings the astronomical second was the most precise of all our standards and with the help of quartz clocks was made instantly available with an accuracy to one part in a hundred million or a thousandth of a second in the length of a day. It was reasonable to aim for at least a ten-fold improvement and this, in my view, ruled out the use of the ammonia line. On my return to the UK I suggested that the atomic clock should be added to our programme of work. The Director, Sir Edward Bullard, was sympathetic to the idea but could not provide the two or three extra people I thought would be required as the NPL was then concentrating on the construction of a computer and any spare hands were directed to that project. The delay was disappointing but it enabled me to improve the velocity of light measurement and also to work with Froome on the refractive index of air. The precise frequency at which these measurements were made was immaterial so I chose a value close to that of the caesium spectral line ensuring that the electronic equipment was ready if and when it became possible to start on the atomic clock. One item which proved to be most important was an oscillator which could be varied smoothly in the region of the caesium frequency. The spectral line was known to be very narrow and in order to find it the searching oscillator would need to have even greater spectral purity. We had often used microwave oscillators stabilised by cavity resonators in the manner described by Pound and realised what a remarkably useful device it was. By using the large cavity resonator used for the velocity of light measurement and adjusting the circuit conditions with exceptional care, I was able to reduce the bandwidth of the oscillations to a few cycles.

In a demonstration given at the Institution of Electrical Engineers two oscillators had been shown to beat together with a clean audible note. It seemed well suited to searching for the spectral line and measuring its exact frequency. It was realised that when this was known it would be necessary to synthesise the frequency from the quartz standard and the spectral line; and provision was made for this step. I knew that it was impossible to keep the signal pure by synthesising directly from our 100 kHz standard and that an intermediate stage must be introduced. An oscillator that appeared to be suitable was made by Marconi's Wireless Telegraph Co. Its nominal frequency was 5 MHz but they agreed to adjust one to the required frequency when I let them know what this was. It turned out to be 5.0069 MHz, which when multiplied by 1836 gave the caesium frequency. Provision was made for altering the quartz frequency a small amount by adjusting the electronic circuit.

A further visit to the USA in 1953 strengthened my interest in building an atomic clock. There had been a change in attitude at the MIT where Zacharias was now keenly interested in time keeping. He had made arrangements with a firm to make a commercial model and was designing a model of enormous potential accuracy to build in his laboratory. The National Bureau of Standards in Washington was working on another model to the design of Kusch who was acting as their consultant.

NPL enters the race

Although it now seemed probable that an atomic clock would soon be in operation in the USA it was important to make an independent clock at another national standardising laboratory. Some of the technical problems had now been solved and I proposed to start work on a clock with our existing staff and some assistance from another scientist J V L Parry who had expressed an interest. His half-time help soon became full time and he was ideally suited to the work not being afraid to tackle new techniques and having a flare for experiment. We both had a good relationship with the workshop, recognising the important part they played in our success. Our rough sketches of the beam chamber were discussed with A Gridley, the head of the instrument workshop and he converted them into working drawings for the mechanics. In the remarkably short time of a few months we were able to begin assembling the equipment in the laboratory, where a space had been prepared. The papers published on atomic beam spectroscopy had all stressed the need to avoid ground vibrations. I knew something about this having had the same problems with quartz clocks. A large slab of concrete had been let into the ground and supported on springs adjusted to give the block a natural period of vibration of a few cycles per second. As our design developed we realised that the deflections of the atoms could be made so large that ground vibration could be ignored. The springs were never unclamped although I expect the concrete slab is still there.

Inside the clock

The atomic clock was made possible through the brilliant theoretical and experimental work of a number of scientists, several of whom received Nobel prizes, but the clock itself is very simple, as can be seen from the sketch in figure 2.

Atoms leave the oven through a narrow slit and pass between the pole pieces of a powerful magnet which is shaped to give a non-uniform field. They follow various paths according to their initial direction, velocity and energy state. Only two paths are shown. A few of the atoms are selected by the slit half way along the path and continue through the pole pieces of a second magnet, which is the same as the first and increases the deflections in the same direction and away from the centre line. A weak radio field is applied in the space between the magnets and, when its frequency and strength are exactly right, the atoms jump to the other state, those initially in the low energy state absorbing energy from the field and, strangely enough, those in the high energy state being induced to emit energy, so that they are all reversed. Their deflections in the second magnet are also reversed and they are deflected back to the centre line where they strike the detector. This is a hot tungsten wire which imparts a charge to the atoms which boil off as charged particles and are attracted to electrodes and, after enormous amplification, measured as an electric current. Atoms which are not in the two states concerned are not deflected at all and give a steady signal which is useful for lining up the apparatus. The beam strength increases by ten per cent when transition occurs. The components are all contained in a metal pipe about 150 cm long evacuated as completely as possible. Fortunately this was at a time when enormous improvements were being made in vacuum practice and we were able to make immediate use of them, two stage diffusion pumps, liquid air traps, neoprene O-rings for making seals and sophisticated devices for measuring the pressure.

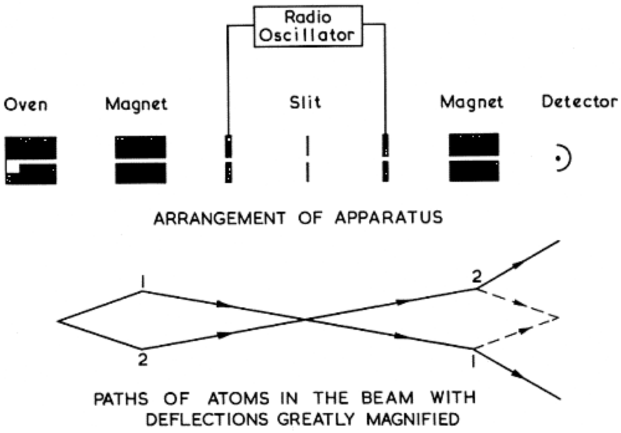

Figure 2. Schematic drawing of the design of Louis Essen's frequency standard (reproduced by kind permission of Ray Essen).

Birth of atomic time

In less than two years the equipment was installed and the beam detected. The radio frequency oscillator was switched on and its frequency varied through the region where the line was expected. We were incredibly lucky to find the right conditions after searching for a few days and there was the resonance exactly as sharp as predicted. We invited the Director to come and witness the death of the astronomical second and the birth of atomic time. And it was indeed the birth because much to our surprise it was another year before any clocks were working in the USA. It was obvious from this very first moment of operation that we could set the quartz clocks with a far greater accuracy than could be obtained by astronomical means.

Our first task was to make every possible test to check to what extent the frequency could be varied by external conditions such as pressure, temperature, strength of the electric and magnetic fields, and so on. This could only be done by establishing a provisional atomic time scale, making use of the stability of our quartz clocks. They were set at intervals of a week by means of the atomic clock operating under standard conditions. These conditions were then varied and the effect measured by the quartz clocks. The test showed that with a very simple control of the conditions the atomic clock was enormously more accurate than astronomical time as well as having the advantages of being far simpler to use and being immediately available. It did not give the time of day, of course, but this is not required accurately. It is the length of a time interval and its inverse, frequency that it needed ever more accurately for modern developments in navigation, computers and communication.

A few months after the atomic clock had been in operation The Astronomer Royal invited me to describe it at the meeting of the International Astronomical Union to be held in Dublin. One of the main subjects for discussion was the adoption of a new unit of time. Astronomers knew that the unit based on the rotation of the earth was no longer adequate and they were recommending a unit, the second of the ephemeris time, based on the revolution of the earth round the sun. Unfortunately although this unit might be expected to be more constant than the mean solar second, it is much more difficult to measure, and the observations would have to be averaged over years to give the required accuracy. This rendered it useless as a unit of measurement which must be available immediately. I pointed out that whatever advantages this unit

Figure 3. Dr W Markowitz with the dual-rate moon camera which he designed in 1951 for the purpose of timing the occultation of stars by the moon and hence relating UT2 to ET (photograph reproduced by kind permission of the US Naval Observatory, James M Gilliss Library Archives).

might have for the astronomer it was useless for the physicist and engineer, and suggested that since an atomic unit would be needed in the future it would be wise to defer a decision until agreement could be obtained on the definition of such a unit. There was no support for this suggestion and the second of ephemeris time was adopted and was later confirmed by the International Committee of Weights and Measures, showing how even scientific bodies can make ridiculous decisions. One useful outcome of the Dublin meeting was that with the help of Markowitz—I was not an official delegate myself—a resolution was passed to the effect that when the relationship between ephemeris time and atomic time was established the atomic clock could be used to make astronomical time available. This meant that we had international approval to introduce atomic time when the comparisons were completed without further international meetings. A detailed programme was arranged with Markowitz. The time interval between certain time signals was measured at the NPL in terms of the atomic clock and at the US naval observatory in terms of the ephemeris second. The comparisons took longer than anticipated because of the relative inaccuracy of the astronomical measurements but after three years it was decided that further averaging was not likely to improve the result. The value was, therefore, announced and was eventually accepted internationally as the unit of time.

In parenthesis it might be explained here that before changing a unit of measurement it is essential to establish the relationship between them so that measurements made in the past would still be valid within the accuracy of the old unit. The second of time presented special problems because it was known that the mean solar second varied. That is why the atomic second was linked with the second of ephemeris time which was believed by astronomers to equal the average value of the mean solar second over some hundreds of years.

No results were announced from the USA until our clock had been working for a year. It transpired that at the Bureau of Standards there had been difficulties with the microwave source and the work had been delayed by a move from Washington to Boulder, and the ambitious scheme of Zacharias had turned out to be too ambitious. The commercial development was going well but had presented technical problems not easily solved. The work moved from the National Company, to Varians and then to Hewlett Packard, the scientist concerned going with it and was supported all the time by government agencies. The clock finally put on the market some years later was a wonderfully compact and accurate instrument.

Although some clocks in the USA were in operation before the completion of our comparisons, Markowitz kept to our agreed programme and the published value was based entirely on the NPL clock. A sub-committee of the International Committee of Weights and Measures was set up to discus atomic time and it is interesting to follow its gradual and reluctant acceptance by astronomers. The meeting in 1957 refused to accept the term atomic clock insisting that it was simply a frequency standard for the second: the second meeting in 1961 accepted that it was a standard of time interval but continued to stall by recommending that further work should be done: the third meeting in 1963 recommended the adoption of an atomic unit of time the value being that obtained at the NPL. No formal steps were taken to implement this recommendation and the International Scientific Radio Union, in which I was the chairman of the relevant section, had to stress the urgency of putting the resolution into effect. It was formally adopted as the unit of time in 1968 with only one abstention, the representative of the Greenwich Observatory, I regret to say. It had in fact been used since 1955 although the astronomical second was still the official unit. The UK standard frequency transmissions were controlled by the NPL and we had the confidence to express and publish their values in terms of the atomic unit. Atomic time had thus been available throughout the world and we found that it was used to correct international time signal, thus disposing of the argument that it could not be used to measure time. There was one problem which had to be solved before atomic time could be universally adopted. Some users such as those at sea with rather simple equipment still navigated by the stars and required astronomical time even if it was not uniform. A suggestion from the US Time Service was that astronomical time should be used for sea navigation and domestic purposes, and atomic time for air navigation and scientific work. My experiences with time signals and standard frequency transmissions convinced me that this would cause endless confusion as well as involving duplication of equipment and I argued strongly that a method of combining all the information in one set of transmissions must be found. The main difficulty was that, although the two time scales could be synchronised to start with, they would gradually drift apart because of the variations in the rate of rotation of the earth.

An excellent compromise was achieved when the astronomers agreed that time signals could be allowed to drift by as much as 0.5 s from astronomical time. Atomic seconds could be transmitted continuously and if, after a year say, they were found to be approaching this difference the markers denoting the hour signal could be moved along by 1 s, without disturbing the continuity of the atomic seconds. A record kept of such step adjustments or leap seconds can be used to give long intervals of atomic time which may be used to measure the periodicities of the bodies of the solar system. The transition to atomic time was made easier by the fact that at first only the UK and the USA were involved, and we agreed to co-ordinate and synchronise all our time signals. As other stations joined in they synchronised their signals with the existing ones and a worldwide system of atomic time was established without any more formalities.

Atomic clocks were improved and several models at national laboratories including the NPL and the Hewlett Packard commercial model were accurate to 1 part in 10^{12}, 10 000 times more accurate than astronomical time, and time signals instead of needing corrections published a year in arrears were immediately available with an accuracy of one microsecond. But scientists as well as the lay public found it difficult to disassociate time keeping from Observatories and seemed unaware of the revolution that had occurred. The Observatories acquired the commercial model and transmitted the signals as before. My contribution was acknowledged by the award of the Tompion Gold medal from the Worshipful Company of Clockmakers and the Popov Gold Medal from the USSR Academy of Sciences who were splendid hosts when I went to receive it in Moscow.

During the discussion on the adoption of an atomic unit the US delegate strongly advocated the use of the hydrogen atom instead of caesium. The choice finally fell to caesium because it was shown that the clock using hydrogen was more influenced by the conditions of operation. However the hydrogen maser, the brainchild of N F Ramsey at Harvard University, is a wonderful piece of equipment. Hydrogen atoms in the higher energy state are selected by their deflection in a magnetic field and directed into a spherical quartz bottle, the internal walls of which were coated with Teflon. As with the caesium atoms a narrow spectral line is obtained only if the transition from one state to the other occurs slowly. Normally an impact with another atom or with the wall of the vessel would cause a transition, but the Teflon coating enabled thousands of wall collisions to occur before this happened. The bottle was enclosed in a cavity resonator tuned to the spectral line frequency. The energy was thereby amplified sufficiently to maintain the circuit in oscillation. It was thus similar to the ammonia maser but far superior in frequency stability.'

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