APPROXIMATION THEORY

or

A MISS IS BETTER THAN A MILE

Professor A. Talbot

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Acknowledgements & reference

Fig. 1 H.W. Dickinson, "A short history of the steam engine". Cambridge Univ. Press, 1939, Fig. 13.

Fig. 2 ibid., Fig. 14.

Fig. 7 H.M. Cundy & A.P. Rollett, "Mathematical models". Clarendon Press, Oxford, 1961, Fig. 271.

Fig. 8 D.I.. Mendeleev, "Issledovanie vodnixh rastvorov po udelnomu vesu" (Specific gravity of aqueous solutions), St. Petersburg, 1887, p. 290

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APPROXIMATION THEORY, OR A MISS IS BETTER THAN A MILE

Whatever else an inaugural lecture is supposed to be - and a variety of descriptions have been offered here by previous victims of the system - I take it as an opportunity for a new holder of a professorial Chair to talk on something that interests him, preferably related to his own subject, and in such a way that he can be understood, largely at any rate, by his university colleagues, by students of all sorts, and by lay members of the public wishing to spend a comfortable hour relaxing in the lecture-room.

Now I profess to be a mathematician, and mathematicians are perculiar in having not merely the two modes of self-expression and communication - prose and poetry - enjoyed by Molière's Monsieur Jourdain and by others, but also a third, esoteric, mode - though as University teachers we do our best to spread the secrets as widely as we can. If I were to use this third mode - the language of mathematics - today, I fear I would quickly lose, figuratively if not literally, a part of my audience. Since I do not wish to do this a moment sooner that I can help, I will avoid this mode as much as I can, though I may occasionally have to use a technical term or two. Professor Lloyd, who faced the same problem in his inaugural lecture five years ago, chose to give his audience a wide-ranging discourse on Mathematics in general, from ancient times to the present. I could not possibly emulate him in such an undertaking and shall not attempt anything like it. Now in the course of my mathematical activity I have had the good fortune to be concerned among other things, with two subjects, namely electric network theory and approximation theory, which have two important features in common: they are both closely related to practical problems, and they have both given rise to a great deal of interesting and varied mathematics. Since this university is, as far as I know, almost if not quite the only one in this country in which approximation theory is offered as a distinct undergraduate topic in mathematics, I think it will be appropriate for me in this lecture to try to convey something of the nature and history of an aspect of that subject that particularly interests me, and I shall incidentally mention an interesting connection with network theory. If occasionally I am led to stray from my path by a beautiful face or other distraction - well, that will be my prerogative this evening.

According to popular belief, "a miss is as good as a mile", but this is more indefensible than most sayings of this kind. Not only does it distort, for the sake of a couple of anapaests, its intended meaning, that "a miss is no better than a mile", but this thesis itself is highly questionable, and is indeed belied by that other equally popular saying "Tis better to have loved and lost than never to have loved at all", which is surely far nearer the truth. So is the Danish proverb which, in translation, (for I dare not inflict my Danish on you), says "Almost shoots no man off his horse". Clearly this is irrefutably true, but it does not tell the whole story - for the objective might be not to shoot him off but to frighten him off. I mention this because in the problems studied in approximation theory the bull's eye is in general an unattainable objective and the important thing is to get as near to it as possible in the circumstances.

Before discussing what approximation theory is about, I would like to say a few words on what it is not. That much of mathematics is concerned with approximations is a familiar platitude. In fact according to Bertrand Russell "All exact science is dominated by the idea of approximation". The theoretical ratio of the circumference of any circle to its diameter is, in abstract, a precise universal constant. But any concrete arithmetical representation of it, as a fraction or decimal, can only be an approximation, and indeed a great deal of effort has been devoted for thirty centuries or more to obtaining increasingly accurate representations, for example the value 3 given in the Old Testament, the value 22/7 found by the Greeks, and so on until the present day when a computer can deliver a value correct to thousands of decimal places on demand. However, such approximations are not the concern of approximation theory. Or again if a problem in applied mathematics, for example involving a vibrating membrane, or torsion in a bar, or fluid motion, is to be solved, this will usually mean solving a differential equation for which no neat 'packaged' solution in the form of a closed formula exists. Then by sophisticated methods solution values can be obtained which are approximations to the true values. But these methods belong to numerical analysis, and not, in general, to approximation theory.

In order to convey what approximation theory <u>is</u> concerned with, I propose to take you back more than 200 years, to the early days of the industrial revolution. The scientific renaissance in the 16th and 17th centuries led, among other things, to an increase in the demand for minerals. It was not long before exhaustion of surface ore deposits necessitated deeper and deeper mining, which gave rise sooner or later to problems of drainage. Early steam engines were invented specifically for the purpose of operating pumps for raising water - mainly for mine drainage, though also for driving water-wheels and for domestic purposes.

The first steam engines in general use were those of Newcomen, dating from 1712. Newcomen came from Devon, and his engines found their first application in the Cornish tin mines. Their fame spread far and wide, and for sixty years they were in use, almost unchanged, not only all over Britain but throughout Europe.

Their action depended on the creation of a vacuum in a cylinder by the condensation of steam in it. However, they were very inefficient, and the first major improvement in their design was the invention of the separate condenser in 1765 by James Watt, then an obscure mathematical-instrument maker practising his trade at Glasgow University, who some years earlier had been given the task of repairing a broken-down Newcomen engine. Fig. 1 shows one of Watt's "single-acting pumping engines". As to its mode of operation I will say no more than that while condensation causes a vacuum in the cylinder, steam at atmospheric pressure applied above the piston forces it to the bottom, pulling down one end of the beam by means of an attached chain, and thereby raising the pump-rod attached to the other end. Valves operated by a third rod attached to the beam (the "plug-rod") now release the vacuum, and the piston is brought up once more by the weight of the pump-rod, thus completing one cycle of operation.

The next big step in development was the conversion of the reciprocating action, suitable for pump operation, into rotative action, needed for countless purposes in mills and factories. Watt, who in Professor French's memorable phrase was an "engineering animal" of the very highest order, invented many ingenious devices to that end, resulting in his double-acting rotative engine of about 1787, shown in Fig. 2, but the device which concerns us here was his so-called 'parallel motion', invented three years earlier.

As we have seen, the pumping engine was "single-acting", that is steam pressure acted on the piston only during the down stroke, the return motion being produced by the weight of the pump-rod. To make the engine double-acting and so more efficient, it was necessary

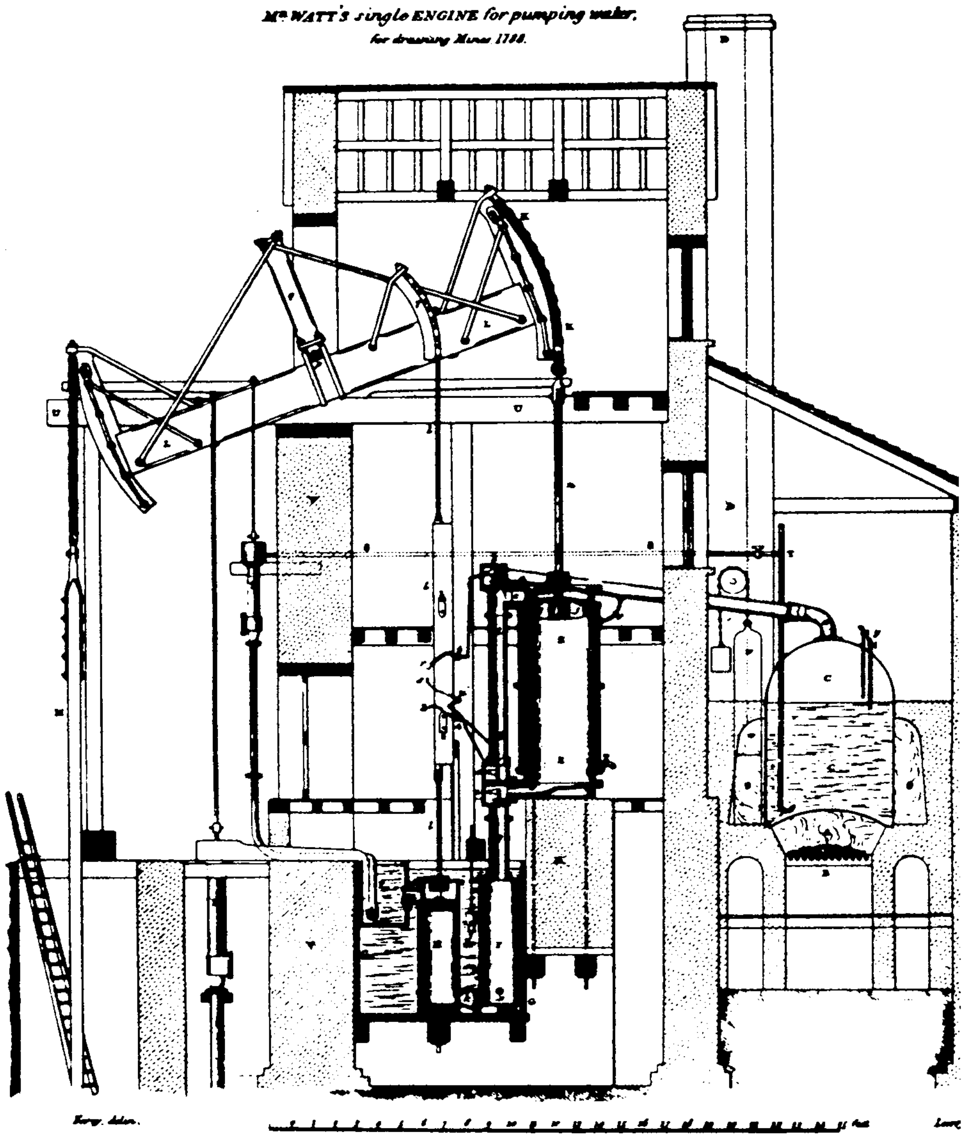


Fig. 1 Watt's single-acting pumping engine, 1788–1800.
From Farey's *Steam Engine*, 1827.

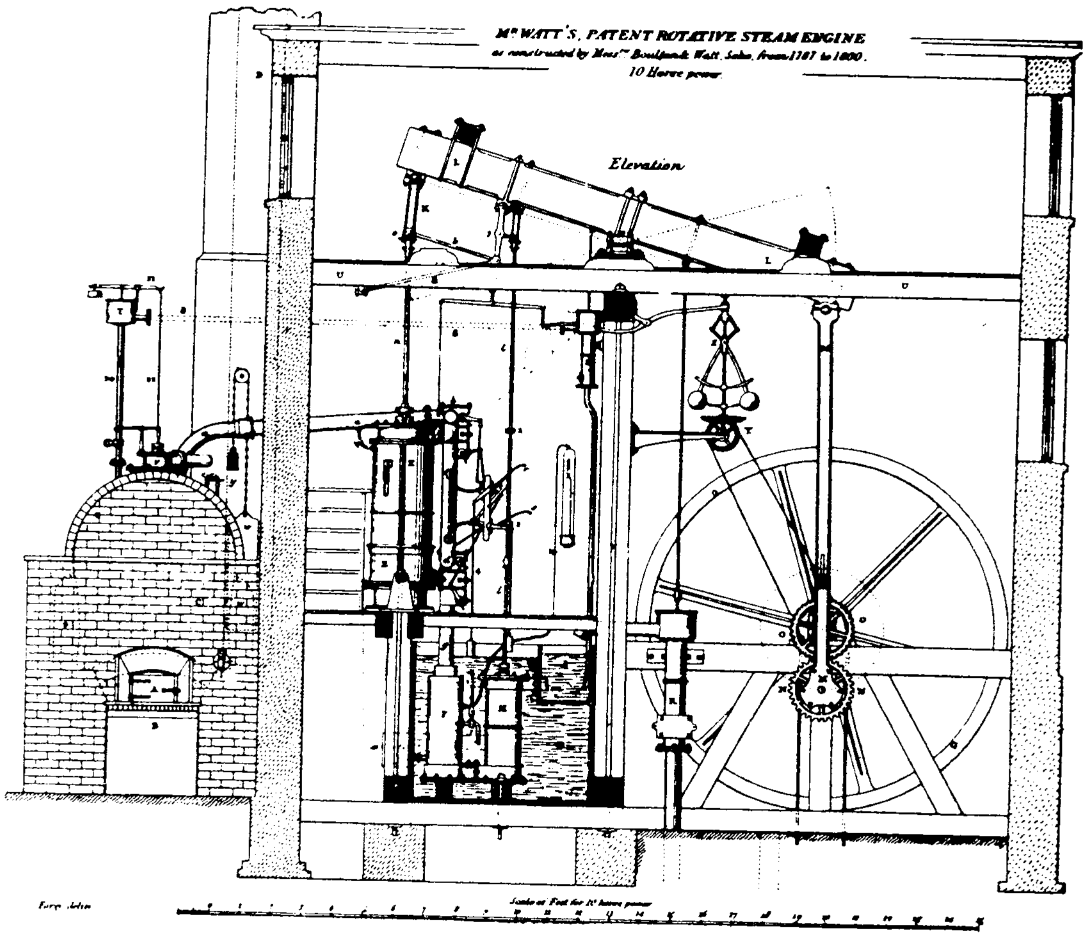


Fig. 2 Watt's double-acting rotative engine, 1787–1800.
From Farey's *Steam Engine*, 1827.

to replace the flexible chain, attaching the piston rod to the beam, by a rigid connection. However, a direct connection was not practical, since the piston and piston-rod needed to move in a straight line, while every point of the beam moved in a circular arc. Again, the use of smooth guiding surfaces was ruled out because, as Watt knew, surfaces of sufficient flatness could not be produced at that time, and in any case the friction that would be introduced was undesirable.

In a letter to his partner, Matthew Boulton, in 1784 Watt referred to his new idea in the following words: "I have started a new hare. I have got a glimpse of a method of causing a piston rod to move up and down perpendicularly by only fixing it to a piece of iron on the beam, without chains or perpendicular guides or untoward frictions, arch-heads or other pieces or clumsiness. I think it is a very probable thing to succeed, and one of the most ingenious simple pieces of mechanism I have contrived."

Watt's solution was a combination, ingenious indeed, of a 3-bar jointed linkage (or 4-bar according to one's point of view) and a pantograph, and with these not only the piston rod but also, as a bonus, the plug rod was given an approximately linear motion. The device, which can be seen below the upper end of the beam in Fig. 2, is shown diagrammatically in Fig. 3. There, EF is the beam, pivoted at D, ABCD constitutes the linkage, with A fixed, while DCEQB forms the pantograph, with CEQB a jointed parallegram, and P is that point of BC which is in line with Q and D. It is easy to see that however the various parts of the pantograph move, the paths of P and Q will always be similar, like those of Peter Pan and his shadow, and in particular if P moves in a straight line, or nearly so, then Q

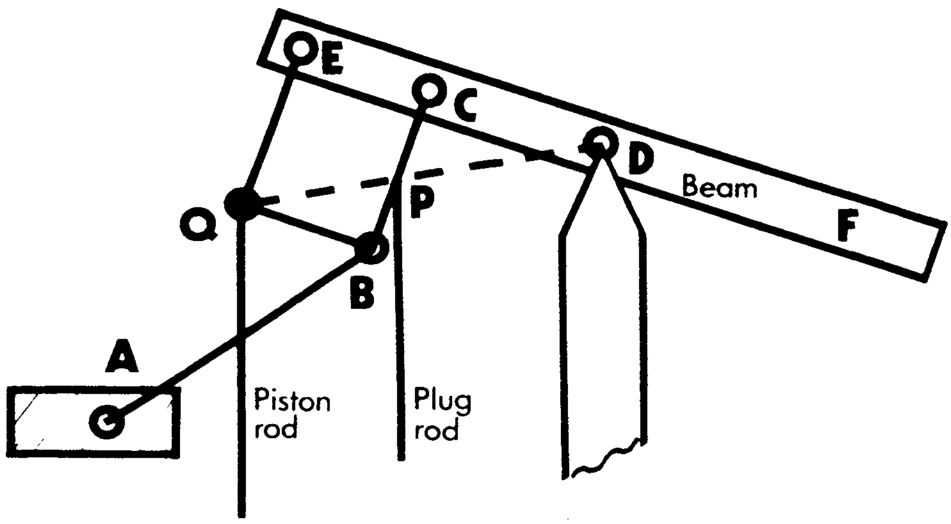


Fig.3

does likewise. Now as the beam oscillates to and fro about its pivot the total path of P is a narrow figure of 8, and Watt showed that if the various dimensions were such that the ratios BP : PC and DC : AB were equal, then the part of the total path corresponding to a beam oscillation of not more than 20° up or down would be very nearly linear. In this way he ensured that oscillatory motion of the beam corresponded to almost linear motion of the plug rod connected at P and of the piston rod connected at Q, as desired.

Years later Watt said "I am more proud of my parallel motion than of any other mechanical invention I have ever made". He would have been even prouder, I imagine, if he had known that in a later epoch and in a distant country his parallel motion would lead to the invention of a new kind of mathematical thinking, and the foundation of a new type of mathematical theory, which would not only make vital contributions to mathematical analysis, but would also play a major part in numerical analysis, and would have an intimate connection with computers.

To understand how this came about, we must travel to the University of St. Petersburg, where Pafnuty Lvovich Chebyshev was a Professor of Mathematics from 1847 to 1882, and enjoyed a European reputation for his work in many fields, including the theory of numbers, theory of probability, theory of integration, interpolation theory and numerical analysis. Now Chebyshev had from childhood been passionately interested in mechanisms and mechanical devices, whether for toys or for more important purposes, and to the end of his days he spent much of his time and money on inventing such devices and making working models. In his later years, for example, he designed and supervised the construction of a calculating machine with several novel features: it can be seen today in the Conservatoire des Arts et Mètiers in Paris.

1851 was a very important date in the history of 19th century science and technology, for it was the year of the Great International Exhibition in London. It was natural that Chebyshev should want to visit it, but in spite of a memorandum sent to the St. Petersburg authorities on his behalf by several senior colleagues, who pointed out the likely benefit to Russian technology, permission was not granted. However, in 1852, with official blessing, Chebyshev, at the age of 31, undertook an extraordinary grand tour of Europe, lasting from June to November. There never was such a tour as this before or since, and a long report which he submitted afterwards tells us in detail of his activities.

He devoted his afternoons to visiting factories, mills, railways, and inspecting machines and machinery of all kinds: Dutch windmills at Lille, water turbines, the hydraulic engines at Marly used for the foundations at Versailles, iron works and machine factories at Metz, machines and working models at the Conservatoire in Paris (many just purchased at the Exhibition in London), and so on. During the evenings on the other hand he did mathematical research, which resulted in at least two major papers, or else visited famous mathematicians, including Liouville, Cauchy and Hermite in Paris, Dirichlet in Berlin, and Sylvester and Cayley in London. (It is interesting to note, by the way, that he was able to visit the last two on Sundays also since in England, unlike France,all factories were closed then!) He was particularly interested in Watt's parallel motion, which was still much in evidence, not only on engines made by Watt's firm (which he specially sought out while in England), but also on several later types of beam engine. In fact, this mechanism is still in use at the present time, for example in the construction of indicators, in certain types of cranes, in ship-steering apparatus, and even in the suspensions of some high-performance cars.

It will be apparent that Watt's device must indeed have had great merit. But it was not perfect: the end of the piston rod did not move precisely in a straight line, but only approximately so, and although the deviations from linearity were very small (for example with a beam about 15 feet long, and a piston stroke of about 4 feet, the maximum lateral movement of the piston rod was less than a tenth of an inch), nevertheless the resulting pressures gave rise to frictional resistance which produced a certain amount of wear. Chebyshev, convinced that with the help of mathematics he could improve on Watt's device, by suitably proportioning the dimensions of its components, set to work on this task while still on his travels - to such good effect that soon after his return he was able to read a paper to the Imperial Academy of Sciences at St. Petersburg,which might well be regarded as the inaugural lecture in approximation theory. Like much of Chebyshev's published work, this paper was in French, and it bore the strange title "Théorie des mécanismes connus sous le nom de parallélogrammes" - strange, that is, not only because one would not expect to find pioneering mathematics hidden under such a banner; but also because, apart from the introductory paragraphs, the paper contained nothing whatever about mechanisms! After 30 pages of mathematics the author promises to apply his formulae to the design of "parallélogrammes", i.e. Watt linkages - but at that point his paper abruptly stops. In fact, although

Chebyshev remained extremely interested in linkages for the rest of his life, and wrote many papers on them, he never completed this first paper.

Since Watt's linkage illustrates rather vividly the nature of the problems met in approximation theory, and the principles used in solving them, I propose to examine it more clearly. Now referring to Fig. 3 it is easy to see that once the scale of the linkage is determined, by fixing the position of C on the beam, five adjustable parameters are available namely the horizontal and vertical distances of A from D, the lengths of the rods AB and BC, and the distance BP.

To obtain his solution Watt had argued that the moving point P should lie exactly on the intended vertical line of motion at three points, namely the top, middle and bottom points of the line. He thus produced a path for P of the form shown solid in Fig. 4 (a), and again in Fig. 4 (b) after being turned through a right angle, for convenience.

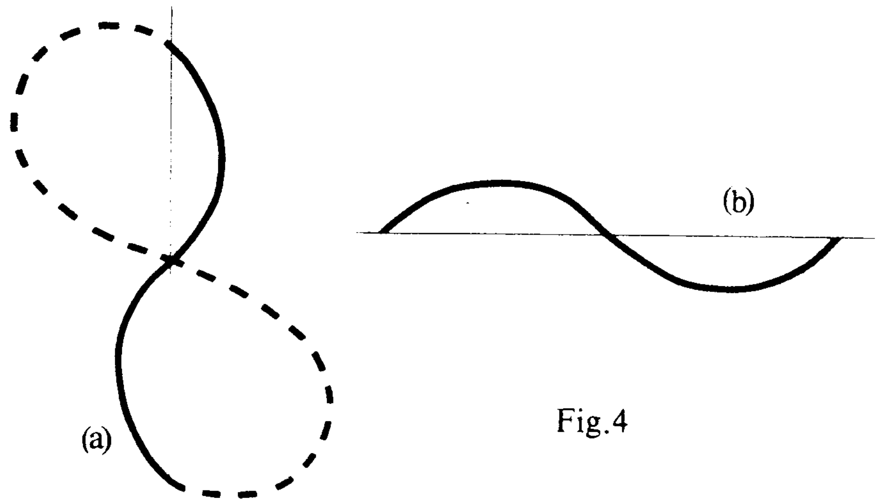


Fig.4

(The dotted portion of the curve in (a) represents positions of P unattainable with the limited beam oscillation. The width of the curve is of course highly exaggerated.) Now Chebyshev asserted in effect that with 5 parameters available it must be possible to produce a path that coincided with the intended line of motion not merely at 3 points as in Watt's curve, but at 5. But he went further than this: he stated -

and this was the major contribution of his 1853 paper - that the maximum deviation from the desired line between the ends of the stroke, or in other words the worst error, would be minimised if the points of coincidence were so chosen that the error actually reached its maximum value, positively or negatively (that is, on one side of the line or the other) at least 5+1=6 times. Thus for the least error the path should behave something like the curve in Fig. 5. However, this cardinal property, the like of which had not been formulated in mathematical literature before, was simply put forward by Chebyshev as a known fact. He never, either then or later, gave any indication of how he arrived at it. He knew instinctively that it was correct for the problem he was studying, and made no attempt to justify it in this paper. In fact he generally regarded the finer points of proof as of less importance than the discovery of practical methods for solving difficult problems. Whether this paper would have been accepted for publication in a modern journal I rather doubt: the author would probably have been told by a referee to avoid making unproved assertions, and anyway to tighten up on rigour generally. In judging him however we should remember that his paper was written at white heat, while he was still in France on his tour. In any case, he made handsome amends five years later in his second publication on what he called "la répresentation approximative des fonctions", or approximation theory as we would now call it. This was a purely theoretical paper of more than 100 pages, concerned with the general problem of minimising the maximum error committed over an interval by representing a given function on the interval by an approximating function of specified type involving a number of adjustable parameters. He showed how to calculate, for each particular type of approximating function, the correct number of points at which the maximum error must be attained in order to minimise it, and he then proceeded to solve the minimisation problem completely in particular cases when the approximating

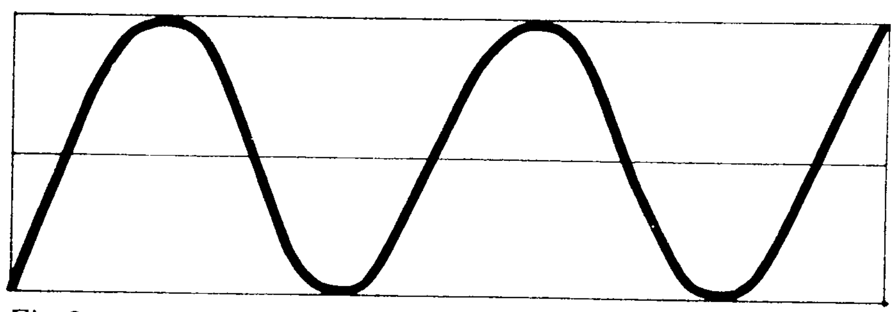


Fig.5

function is a polynomial, a rational function with a prescribed denominator, or a rational function with free numerator and denominator. These cases are in increasing order of difficulty, and Chebyshev's solution for the last case especially was a veritable tour de force, for which he made extensive and ingenious use of continued fractions. The problem was one of both algebra and calculus, but would have been quite insoluble by any orthodox approach. It was not until 1931 that a simpler solution to this problem was found, by another eminent Russian mathematician, Achieser, using conformal transformations in a complex plane. I have myself been interested in this problem for many years, and succeeded in obtaining a simpler and more elementary solution (the two adjectives are by no means synonymous!), by a method which can also be applied to a number of other problems.

If we return for a moment to the linkage problem, and imagine how an orthodox mathematician would have tackled it, we can be fairly sure that, armed with the Taylor expansion formula, he would have aimed to make the error as small as possible near the middle of the interval, by arranging for the error curve to have the highest possible order of contact there with the desired line (5th order in the case of Watt's curve), thus giving an error curve of the form shown in Fig. 6,

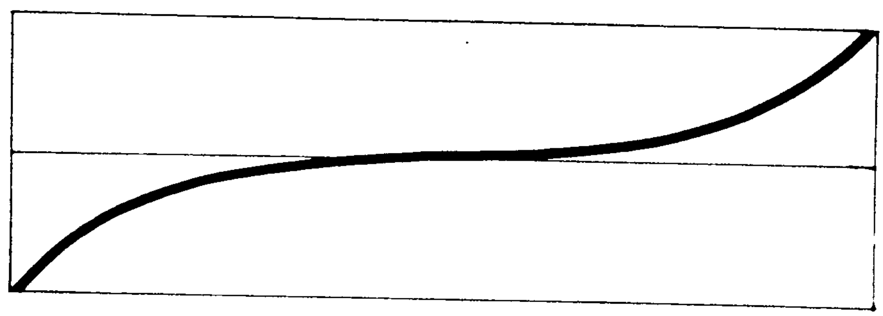


Fig.6

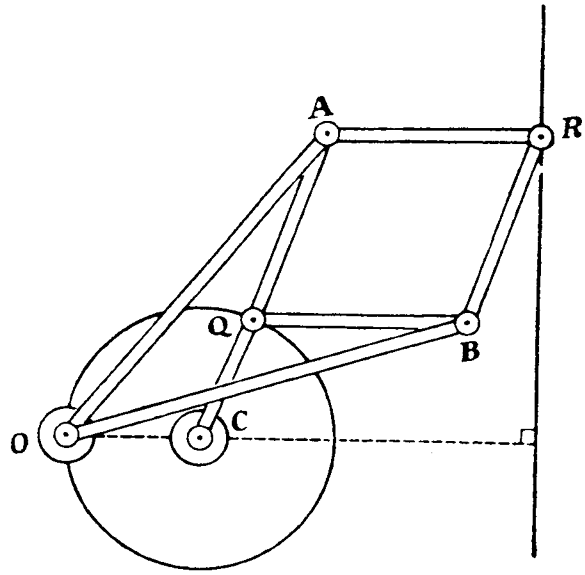
which is very good in the central region but gets progressively worse as one moves out to the ends. In comparison with this 'classical' solution one might loosely say that Chebyshev's curve minimises the maximum error by, as it were, spreading the error more or less evenly over the whole interval. The reward is considerable, for the maximum error is thereby reduced by no less a factor than 2^4 or 16. It will be observed incidentally that Watt's solution is intermediate between the two, and in fact Chebyshev claimed an improvement of only about 2 or 3 to 1 over Watt.

It is clear that by the invention of his 'parallel motion' Watt can fairly be considered as the progenitor of approximation theory (a claim I have never seen made by even the most fanatical of steam-engine enthusiasts!) It is interesting therefore that his double-acting engine (see Fig. ?) contained not just one but two quite separate devices each of which might have given rise to that claim. For besides his parallel motion linkage, Watt's engine contained a governor for regulating the speed, and although he had probably seen crude versions of this in machines operating in Cornwall, his own device was greatly superior to any of those. Naturally Chebyshev made a note of this too during his travels, and in due course - but very much later - he turned his thoughts to improving on it. The problem is one of dynamical synthesis rather than kinematical synthesis as in the case of the parallel motion. The operation of the governor is in principle very simple: any change of speed results in a change of centrifugal force acting on the heavy spheres, which accordingly move outwards or inwards and thus by means of a linkage operate a butterfly valve in the steam pipe which tends to counteract the original change in speed: a classical example (probably the first) of automatic control by negative feedback. Now of course the regulation is not perfect (it would be useless if it were!) and the controlled speed, instead of being constant, varies slightly with the angles of inclination of the rods. The design problem is therefore to arrange the geometry of the device in such a way that for an appreciable range of variation of the angles, the corresponding speeds differ as little as possible from a constant value. Chebyshev produced a solution in 1871. Formally the problem is similar to the first one: again there are 5 adjustable parameters to be found, and a curve to be made as flat as possible. But in detail this problem is much more complicated. He attacked it by first obtaining tentatively what I have called the 'classical' solution and modifying this, by a process developed in the paper of 1853, so as to get very near to the optimal solution in which the worst error, or departure from flatness, was minimised. However this solution has unfortunately a fatal flaw in practice, due to the very feature that makes it optimal, namely the property that the curve must oscillate in the way seen earlier. The result of this is that the same speed can occur for five different angles, and in practice this means an unstable system: for any particular speed, the governor would not know which configuration to take up, and it would exhibit schizophrenia of a very high order.

Chebyshev had the practical sense to recognize this, and to see what had to be done. It will be noticed that the classical curve does not have this objectionable property: it is monotonic, that is to say it continually increases (or decreases), and never takes the same value twice. Thus Chebyshev set himself the problem of finding the optimal design subject to the constraint that the curve of speed should be monotonic. As far as I know this was the first example of constrained approximation. Such problems are always much more difficult to solve. Chebyshev of course was not defeated by this problem, and produced a most elegant solution.

Let us now take stock. As I have already indicated, approximation theory is concerned with finding near misses, or nearest possible misses, in problems where a bull's eye is unattainable. But are there problems where a bull's eye can be scored in a non-trivial way? The answer - which would certainly have suprised Chebyshev in 1860 when he was still engrossed in his work on Watt's parallel motion - is yes! And it was another linkage that demonstrated this. In 1864 a French army engineer named Peaucellier, in a brief letter to a mathematical journal, mentioned Watt's linkage and referred to the problem of producing exact rather than approximate linear motion by means of linkages, but he gave no hint that he might have found a solution. In 1870 a young pupil of Chebyshev named Lipkin submitted a paper to the Academy of Sciences describing a 7-bar linkage giving exact linear motion which he had discovered two years before, at the age of 17, and this paper was published in 1871. In 1873 Peaucellier published an identical solution and claimed priority for it, saying he had referred to it in his 1864 letter. This gave rise to an unpleasant quarrel between Chebyshev on the one hand and the friends of Peaucellier on the other. The verdict seems to have gone in favour of the latter, for the discovery is now always attributed to him, while Lipkin is rarely mentioned. Whatever the truth was, there is no doubt that the discovery was a sensational event in the world of applied mechanics, although, with its 7 bars and 6 joints (see Fig. 7) it was in practice less accurate than Chebyshev's 3-bar linkage, because of the large number of errors due to production tolerances. An exact straight-line linkage with fewer bars would clearly be preferable, but Chebyshev, having studied linkages for more than 20 years, expressed the opinion that no such linkage could exist. But he had no proof of this, and was relying only on intuition, That he was quite wrong was shown in 1874 by Hart, an English engineer whose 'crossed parallelogram' straight-line linkage has only 5 bars - which he proved in fact to be the smal-

Fig. 7



lest possible number. The straight-line properties of both Hart's and Peaucellier's linkages depend on the fact that the inverse of a circle with respect to a point on its circumference is a straight line, and could easily have been proved by any Sixth-former of those days. Whether this would still be the case today, now that Euclid has been relegated to the scrap-heap, I am not sure.

Until the end of the 19th century, work on the theory of approximations initiated by Chebyshev was confined to some of his students, who, using the framework established by the master, tackled and solved difficult problems involving approximation by polynomials or rational functions. It is astonishing however that neither Chebyshev nor his students ever considered the basic question of how accurate the approximation could become if the number of parameters was increased indefinitely. Could the maximum error be reduced to as near zero as one wished, or was there a lower bound below which it could never fall however many parameters one used? The question was answered in 1885 in an epoch-making way by the great German mathematician Weiersrass, who showed that by sufficiently increasing the degree, and hence the number of coefficients, of a polynomial approximation to a given continous function, the maximum error over a given interval can be made as small as desired. This result has had a profound effect on the theory of functions of a real variable, and is one of the major pillars of mathematical analysis.

Weierstrass and Chebyshev were acquainted with each others' work, and indeed were linked in a most interesting way - by a brilliant and beautiful Russian woman named Sophie Kovalevsky. In 1868, at the age of 18, she tried to gain admittance to the faculty of Mathematics at St. Petersburg, but even though Chebyshev supported her application, the official anti-feminist prejudices of the time preveiled, and she was refused. Thereupon, determined to enter a university, she contracted an initially nominal marriage with a young lawyer who had become a science student, and armed with her certificate of respectability, went to Germany. After a year at Heidelberg, where she attended the lectures of Kirchhoff and Helmholtz, among others, she moved to Berlin. There however she was not admitted to lectures, but contrived to become a private student of Weierstrass-at that time the most eminent analyst in Europe - and four years later returned to Russia as a doctor of philosophy. After interruptions for high society, the struggle for the emancipation of women, literary activity and the birth of a daughter, she resumed her mathematical studies in 1880, and in 1884 was appointed Professor of Mathematics in Stockholm - the first woman university professor in the world. Incidentally, she enjoys another interesting distinction. In E. T. Bell's book "Men of Mathematics" there is one and only one reference to Chebyshev, namely that he had called on Weierstrass when he was out, and had left a message saying that Sonja (that is, Sophie) had gone social in St. Petersburg. Such is fame!

The point of this digression is that as a disciple of Weierstrass, through her frequent contacts with Chebyshev she served to highlight the difference in approach between the two men, and thus between their respective mathematical schools. German mathematics was oriented towards the general theory of functions, without regard to practical utility, while the Russians, led by Chebyshev, always kept their feet firmly on the ground, and concentrated on research likely to result in useful applications. In the development of mathematics, there have always been both kinds of mathematician, sometimes represented in the same person as for example in the case of Euler, Gauss, and Poincaré, and indeed both kinds are essential for progress. In this country one might be tempted to refer to them respectively as "pure" and "applied" mathematicians. But I have never liked this artificial distinction, and indeed the epithets are not really applicable to the two men in question. Weierstrass was by no means unfamiliar with the mathematical physics of his day. In fact his proof of the theorem on approximation already quoted was achieved through an ingenious application of an integral occuring in the theory of heat. And when in 1880 Sophie Kovalevsky, after several years' separation from mathematics, appealed to Weierstrass for a problem she could tackle, he suggested an investigation of the propagation of light in a crystalline medium. On the other hand Chebyshev always had an active interest in subjects so relatively 'useless' (from a practical point of view) as the theory of numbers and certain problems involving the integration of irrationals. But in any case Chebyshev, though mainly interested in practical applications, was still not an applied mathematician in the classical British sense. Nevertheless, like Weierstrass he was well informed on problems of applied mathematics, and was particularly interested in astronomy. Thus when a talented 26 year old student named Lyapounov asked Chebyshev in 1882 for a problem, he was given the important but extraordinarily difficult one of determining the possible equilibrium forms, other than the ellipsoidal form already known, of a rotating mass of fluid subject to Newtonian attraction between its particles, and of investigating the stability of these forms. Within a year he produced a thesis containing a partial solution, which, if he had published it, would have astonished the mathematical world, for two years later Poincaré, in ignorance of Lyapounov's work, published a less satisfactory solution which nevertheless gave him instant fame, election to the French Academy of Sciences at the early age of 33, and the Gold Medal of the Royal Society. Eventually, Lyapounov, after many years of continuous work on what he always called "Chebyshev's problem", completely solved it, and therewith discovered a method for the investigation of stability which is of the utmost importance at the present time in the study of all kinds of linear or non-linear systems, including systems for automatic control.

Mathematics has been called the Queen of the sciences, but it is equally their servant, and one of the most facinating features of its relationship to science and technology is that each can provide an energising and fertilising stimulus for the other. For example, we have seen how a simple engineering device served, after a lapse of nearly 70 years, to promote the birth of a new branch of mathematics. In the opposite direction is the path from Riemannian geometry, which burst upon the world in 1884, to Einstein's general theory of relativity of 1917. I propose now to mention two more examples of this interrelationship, one in each direction, and both drawn from approximation theory.

About 1887 the great chemist Mendeleev, probably best knowh for his periodic classification of the chemical elements, was investigating the way in which the specific gravity s of various kinds of aqueous solution varied with the percentage concentration p of the solute. Among the solutes he considered were sulphuric acid and ethyl alcohol. In each of these cases he gathered data from many reliable sources, and for each value of p studied he took the mean of the values of s available, which differed from each other by at most a few parts in ten thousand. From these values of s he calculated the values of the derivative ds/dp, and on plotting these he found in both cases that the derivative was approximately a piecewise linear function of p, that is to say, its graph consisted of a number of consecutive straight lines with different slopes - which meant that s was a piecewise quadratic function of p. Moreover, the transition points between the pieces corresponded closely to various molecular associations of sulphuric acid or alcohol with water which were already known or suspected. A detailed analysis of the figures for s gave sets of quadratic formulae which fitted them extremely well, but when the corresponding derivatives were plotted, the linear pieces did not meet at the transition points.

Now in the case of sulphuric acid the discontinuities at these points were much too large to be explained away by experimental error: they clearly represented a genuine chemical phenomenon. But in the case of alcohol (shown in Fig. 8) they were much smaller and Mendeleev was too good a scientist to overlook the possibility that they might be accounted for by small errors (i.e. errors of at most 2 or 3 parts in 10,000.) He was thus led to consider the following mathematical problem: if a quadratic function of p is constrained to have a prescribed maximum numerical value in a certain range of values of p, what maximum value may the derivative have?

In fact Mendeleev solved this problem unaided, and concluded that the discontinuities in the case of alcohol might indeed have been produced by experimental errors. But meanwhile his mathematical colleaue at St. Petersburg, A. A. Markov, had heard of Mendeleev's problem. Markov, a former student of Chebyshev's and himself an eminent mathematician, known today especially for his work in probability theory, typically set himself the more general and much

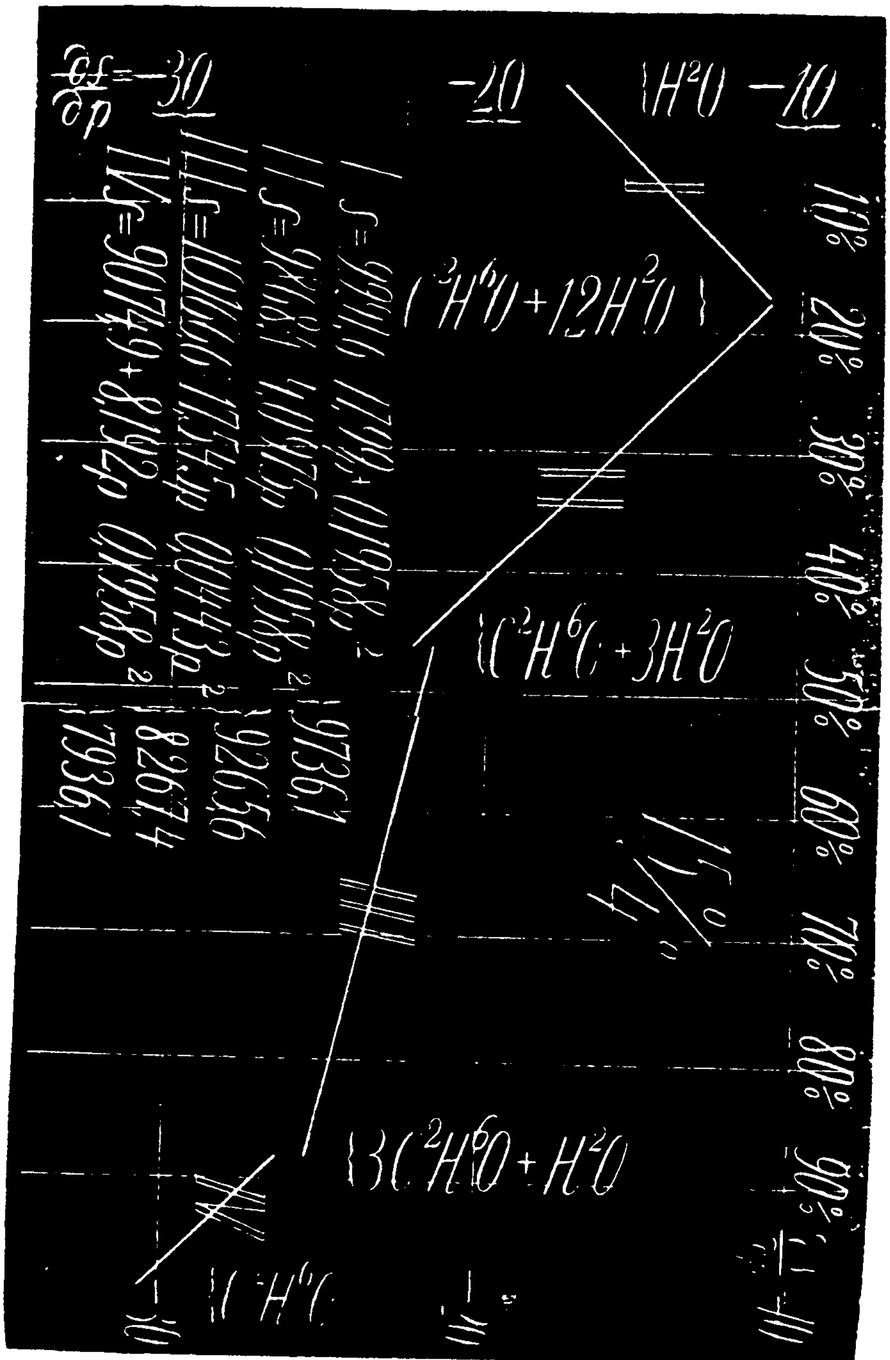
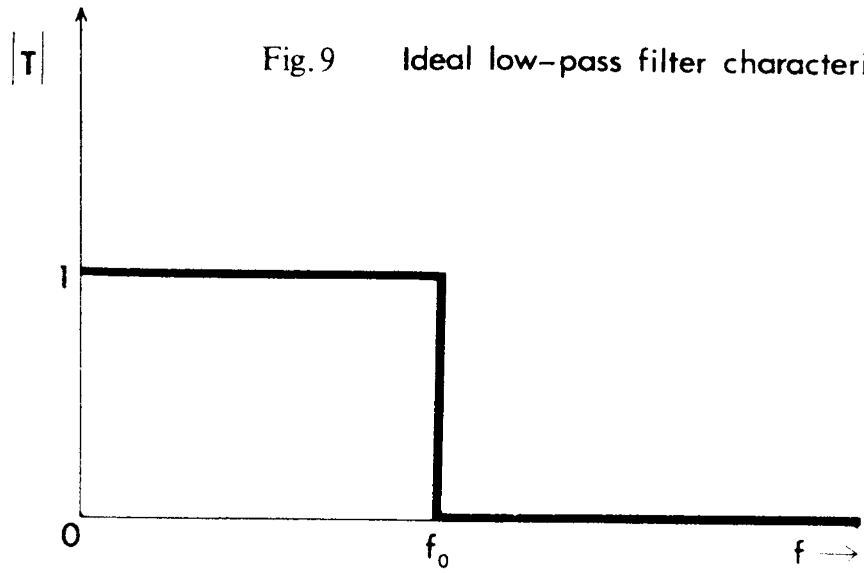


Fig. 8

more difficult problem in which the quadratic was replaced by a polynomial of degree n; and he solved it brilliantly, thereby starting a new and important chapter in approximation theory. His main result was that if a polynomial of degree n is scaled so that its maximum numerical value does not exceed 1 for values of the variable between -1 and +1, then the numerical value of the derivative cannot exceed n^2.

My second example concerns the design of electric filter networks, on which I must first make a few introductory remarks. A filter network may be simple or complicated, and contain a small or a large number of components (which may be of several different types), but we need only note that it has an input and an output current or voltage whose ratio, which we may call the transfer ratio, depends on the oscillation frequency of the input. The filter design problem is to put together components of the right types and right values so that the graph of the transfer ratio when plotted as a function of the frequency, has a prescribed shape. For example, an ideal low-pass filter transfer function would have the form shown in Fig. 9.

Fig. 9 Ideal low-pass filter characteristic



All input frequencies below the cut-off frequency f_o would be passed through the filter without loss, but for frequencies above f_o the output would be zero. However, a mathematical analysis shows that such a filter cannot possibly be constructed - for if it existed it would have the remarkable property of enabling messages to be received before they were sent. Thus here we have a case where the bull's eye cannot possibly be reached, and the designer must therefore aim for as near a miss as possible with a prescribed number of components. The art of filter design dates from the early 1920's, and had reached a very sophisticated and effective stage by 1930 - but the idea of designing for optimal approximation, in the sense of Chebyshev, had not yet emerged. However, in 1931 the electrical design world was startled by the appea.ance in Berlin of a book, "Siebschaltungen", by Wilhelm Cauer, in which the complete design of filters with the best possible performance for a given size was described. Formulae involving elliptic functions were profusely displayed , but no proofs were given, and it is said that for some time after the publication, the best mathematical brains in the Bell Telephone Laboratories in the United States were assigned the task of finding out how Cauer had arrived at his results. In fact, Cauer had been a mathematical physicist before turning electrical engineer, and had become acquinted with Chebyshev approximation. He saw that this was exactly what was needed for his design problem: for example, for a low-pass filter, since the ideal curve could not be attained, one must aim at a curve of the so-called "equiripple" type shown in Fig. 10, in which the deviation from the

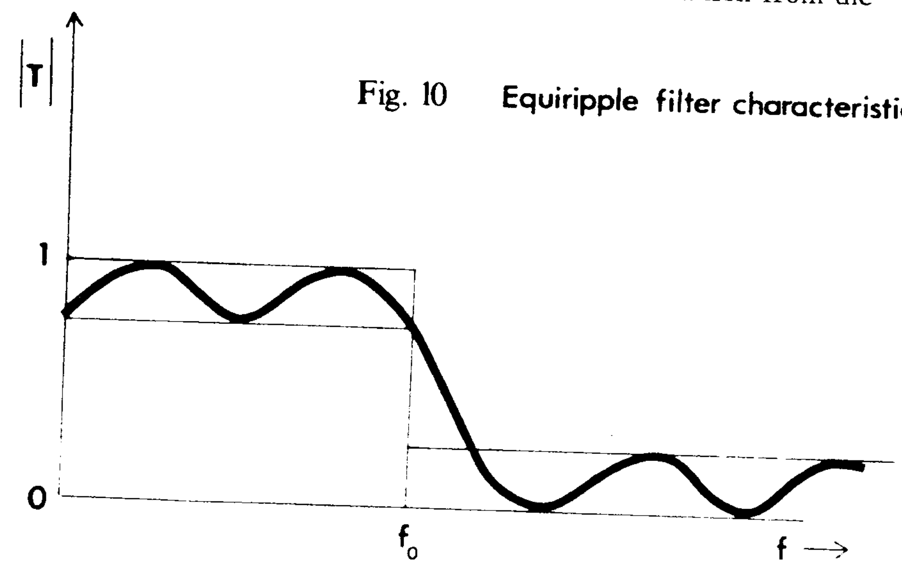


Fig. 10 Equiripple filter characteristic

ideal curve oscillates in both the pass-band and the stop-band in a similar way to that in Chebyshev's linkage, However, the problem of achieving this behaviour was a difficult one, and here Cauer was helped by a paper of 1877 by Zolotarev, another of Chebyshev's students. Cauer found that by adapting Zolotarev's results, he could solve his design problem. I may add that it was through this work of Cauer that, as a mathematician engaged many years ago in electric network design I first became acquainted with and fascinated by the whole subject of approximation theory.

In this last example I have leapt from the 19th century well into the 20th. Let me go back however to 1899. This was an important year in the history of the subject, for it marked the simultaneous publication in both Russian and French of Volume 1 of Chebyshev's collected works, the first fruit of which was the inclusion in a text-book on real-variable theory in 1905 by the famous French mathematician Emile Borel, of a simple yet completely rigorous treatment of Chebyshev's method. Chebyshev had left two important questions unanswered: whether, in a given problem, there really was a best approximation (one that could not be improved upon) and, if so, whether there were several equally good best approximations; in other words the questions of existence and uniqueness of best approximation. That it is not merely pedantic to bother with the existence question is shown by the fact that for some types of approximating function, and in certain cases, there may not be a best approximation. Borel used what I might call Weierstrassian analysis to solve the existence problem. But his main contribution was to emphasise the importance of the error oscillation property, according to which the error must not only attain its maximum value a sufficient number of times (as indicated by Chebyshev), but must take positive and negative values alternately in doing so. Chebyshev was certainly aware of this property, but unaccountably he never mentioned or used it. In Borel's hands it became the key to a full understanding of the situation, and he showed that it completely characterised the optimum error in the case of approximation by polynomials. The uniqueness of best approximation is then a simple consequence of this alternation property.

Borel may be said to have wedded the ideas of Chebyshev and Weierstrass, and it was this union that produced the approximation theory of today. His book was widely read and very influential, and was undoubtedly responsible for the spread of the subject in Western Europe and beyond, though it was nearly 40 years before it took root on this side of the Channel-but that is another story!
In the hands of mathematicians of Europe and the United States the

subject developed rapidly, and great names like Bernstein, Dunham Jackson, Polya, Walsh, Achieser and many more graced the pages of papers devoted to an ever increasing number of its branches. Moreover, with the appearance of functional analysis in the world of mathematics, from about 1920, approximation theorists received a powerful new tool which at the same time opened ever wider fields for their activities.

Nearly all this work was theoretical. It is true, some Russian mathematicians, especially Remez, concentrated on the practical aspect, that is to say, the approximate representation of given functions by polynominals or other standard types of function, as efficiently as possible. Remez in 1935 developed algorithms, guaranteed to lead to the best possible solution, but unfortunately the amount of computational work involved was so great as to discourage the use of his method.

The situation changed dramatically after the War, with the rise of the digital computer. The calculations became practicable, and the algorithms once more worthy of study and development. But now a most interesting new phenomenon appeared, for it was seen that not only did approximation theory need the computer - but the computer needed approximation theory! To understand this, we should recall that when a mathematician solves problems numerically, he must have available a wide range of mathematical tables, giving values of functions of many kinds: trigonometric functions, exponential functions, Bessel functions, and so on, which are liable to be needed at any stage of the calculation. If a computer is to solve these problems, it too needs to be able to call on the values of such functions - and moreover to have them available to a very high degree of accuracy, for otherwise the numerical capabilities of the computer will be wasted. Now the obvious procedure would be to put all the values that might be required into the computer's store. But alas if this were done there would be no room for anything else - and anyway, computer storage space is a very expensive commodity. The alternative is to give the computer the means - i.e. a programme - for calculating any function that may be needed, and it is here that approximation theory makes its contribution, for it provides the most efficient possible programmes for the calculations, for which all that need to be stored are the values of a small number of parameters, for example the coefficients of a polynomial approximation. Using these, the computer can calculate the value of the corresponding function for any value of the variable, and this moreover to an accuracy guaranteed in advance.

Thus there has developed a sort of symbiotic relationship between approximation theory and computers which has lasted for about 20 years and has undoubtedly contributed greatly to the explosive growth of the subject since the War. As P. J. Davis has said, writing of the impact of computers: "The computer research effort has been a great spur to the theoretical aspects of approximation theory. In blunt language, an awful lot of money has been spent on approximation theory in the name of computation'." To which I can only add "amen!".

In Chebyshev's first paper on the subject he introduced a special polynomial - the best approximation to zero, in an interval, among all polynomials with fixed leading term, and this polynomial, which now bears his name, is an indispensable tool in much of numerical analysis as well as in approximation theory itself. So important has it become that some years ago an international committee was set up to decide on a standard spelling of the name Chebyshev from 9 different forms then in use! These polynomials have been powerfully used, in particular for the numerical solution of differential equations, and it is appropriate for me to mention that one of the foremost workers in this field has been my colleague at Lancaster, Professor C.W. Clenshaw.

In this account of approximation theory I have, I realise, allowed my enthusiasm for Chebyshev approximation to obscure the fact that there are other forms of best approximation, in which the root-mean-square or some other average value of error is minimised, rather than the worst error. However, there is no doubt in my mind that Chebyshev approximation is the 'best of the best', and I make no apology for having concentrated on it.

This subject which, as I have tried to show, has inspired and been inspired by technology, science, pure mathematical analysis, numerical analysis, and the most abstract flights into function spaces, has clearly earned itself a permanent place not only in mathematics, but also in the university mathematics curriculum, and I am confident that Lancaster's example in this respect will be followed by many other university departments of mathematics.