T. P. KIRKMAN, MATHEMATICIAN

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1. It is now widely believed that Kirkman was an eccentric amateur mathematician who invented a problem about schoolgirls. Yet some of his contemporaries considered him to be a powerful and original thinker, perhaps one of the leading mathematicians of his time. His place as one of Macfarlane's *Ten British Mathematicians of the Nineteenth Century* [**12**] bears witness to this. However, unlike many of Macfarlane's Ten, there is no collected edition of his works, and he does not appear in the more recent annals of mathematical hagiography [**1**]. Indeed, his cloudy reputation today merely reflects the obscurity of his life and work, for he spent 52 years as Rector of a small country parish, and published much of his work in obscure journals, using obscure notation and terminology. His greatest work was never published.

This article is an attempt to record some of his mathematical achievements. One fact beyond dispute is that he was no amateur. His published work spans nearly half a century, and includes over sixty substantial mathematical papers as well as uncounted minor publications. He solved the problem of Steiner triple systems six years before Steiner proposed it. He constructed finite projective planes. He wrote extensively on the theory of groups, working in parallel with, but independently of, Jordan and Mathieu. He proposed and, in his own estimation, solved the isomorphism problem for polyhedra. At the age of 80 he provided essential data for systematic tables of knots. It is remarkable how little of this can be gleaned from the standard works of reference. The best sources are the oldest: an obituary notice by W.W.K. (presumably his eldest son) in the Manchester *Memoirs* [**10**], and the essay by Macfarlane referred to above. In recent years Dr S. Mills [**17**] has rediscovered many interesting details about his life, and these have helped to unravel the complex nature of the man.

The following account is necessarily incomplete. If that be thought strange, one might reply that to understand the life's work of one man is the life's work of another. In the case of Kirkman's work (on polyhedra, in particular) the other man is not the author of this article.

2. Thomas Penyngton Kirkman was born on 31 March 1806 at Bolton in Lancashire. He was baptised 'Thos. Pennington', the alternative spelling of the middle name appearing to be a later modification. His father, John Kirkman, a cotton dealer of modest wealth, was determined that his son should follow him into the family business. He was sent to Bolton Grammar School, where 'the boy showed a decided taste for study and was by far the best scholar in the school' [**12**]. Nevertheless, he was forced to leave school at the age of fourteen, and for the next nine long years he worked in his father's office. The trials and tribulations of those years can only be imagined, but therein may lie the explanation of the sharper aspects of his character. Eventually, at the age of 23, he broke with his father and

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entered Trinity College, Dublin. He earned his keep by private tutoring, and studied the subjects prescribed for the B.A. degree—mathematics, philosophy, classics, and science. There are no records of his life in Dublin, which is strange, for even then he must have been an unusual student.

After graduating in 1833, he returned to England and entered the Church, holding curacies at Bury in Lancashire and Lymm in Cheshire. Then, in 1839, came the turning point of his life. The words of his son **[10]** cannot be bettered:

> ... he was enticed by fair words, by the then rector of Winwick, to bury himself for life as rector of the newly-formed Parish of Southworth with Croft, where he remained for 52 years. Here, by perseverance and his gift of teaching, he formed, out of the roughest material, a parish choir of boys and girls who could sing, at sight, any four-part song set before them. Here also, with an expenditure of mental labour that only the finest of physical constitutions could have sustained, he devoted, practically, the whole of his time (for the parochial work was small) to the study of pure mathematics, the higher criticism of the Old Testament, and questions of first principles.

In June 1841 he married Eliza Wright of Runcorn, Cheshire, and the first of their seven children, William Wright Kirkman, was born a year later. The income from the parish was not great, and at first it had to be supplemented by taking pupils. Later Eliza inherited some property and they were financially secure, but by no means wealthy. The new Rector was approaching forty, a member of a respected profession, and the head of a growing family. Many a man, in such circumstances, would seek little more than comfort and contentment for the rest of his days; but the Reverend Kirkman was not a comfortable man. There is no evidence that he had taken any special interest in mathematics before this time, or that he had any intention of devoting himself to mathematical research. Nevertheless, he had the time and he had the ability; all that was needed was the catalyst.

3. The Prize Question in the *Lady's and Gentleman's Diary* for 1844 was set by the editor, W. S. B. Woolhouse:

> Determine the number of combinations that can be made of n symbols, p symbols in each; with this limitation, that no combination of q symbols which may appear in any one of them shall be repeated in any other.

The *Diary* for 1845 contained several attempted solutions. Mr. Septimus Tebay of Preston asserted that the required number was $\binom{n}{q}$ divided by $\binom{p}{q}$, but the editor remarked that Tebay had assumed that each q-subset appears exactly once in the arrangement, and this may be impossible. The Question was held over for a year, and then it was replaced by the special case $p = 3$, $q = 2$. The editor once again drew attention to the difficulties, pointing out that when $n = 10$, for example, it is not possible to find a system of triples in which each pair occurs exactly once. In the *Diary* for 1847, Tebay, Woolhouse, and others, made some cogent remarks about the problem, without coming close to a general solution.

On 15 December 1846 Kirkman presented a paper on the Prize Question to the Literary and Philosophical Society of Manchester. Soon afterwards the paper **[46.1]** was published in the *Cambridge and Dublin Mathematical Journal.* He addressed himself to the problem in the following form: how many triples can be formed with x symbols, in such a way that no pair of symbols occurs more than once in a triple? Easy counting arguments show that a system S_x in which each pair occurs exactly once is possible only if x is congruent to 1 or 3 modulo 6. The main result of the paper is the converse—a system S_x does exist for all such values of x.

Kirkman's proof, which seems to be correct in principle, but lacking in detailed justification, is based upon two propositions. Proposition A is concerned with the construction of a system S_{2x+1} when S_x is given. He begins by constructing a system D_{2m}, consisting of an arrangement of the $\binom{2m}{2}$ pairs of $2m$ symbols in $2m-1$ columns, such that each symbol occurs just once in each column. (Nowadays this would be referred to as a parallelism, or an edge-colouring of the complete graph K_{2m}.) Next, he shows how a system S_x and a system D_{x+1} can be combined to give an S_{2x+1}. Finally, by deleting two symbols and some triples from S_{2x+1} he obtains a partial system S'_{2x-1}, in which certain pairs do not occur. In Proposition B he assumes that S'_{x+1} is given, and constructs S_{2x+1} and S'_{2x-1}. The conjunction of the two propositions provides a recursive method for the construction of S_x whenever x is congruent to 1 or 3 modulo 6, according to the scheme

$$S_3 \Rightarrow S_7 \Rightarrow S'_5 \Rightarrow S_9 \Rightarrow S'_7 \Rightarrow S_{13} \ldots .$$

In the remainder of the paper the existence of partial systems S'_x for other values of x is investigated.

The later history of this problem is not without interest. In 1853 the famous geometer Jakob Steiner published a short note **[22]** in which the question of the existence of the systems S_x is mentioned. Consequently, the name 'Steiner triple system' is often used to denote such an arrangement. A few years later Steiner's question was answered by Reiss **[21]**, an achievement which prompted an outburst of sarcasm from Kirkman **[87.1]**:

> ... how did the *Cambridge and Dublin Mathematical Journal*, Vol. II, p. 191, contrive to steal so much from a later paper in *Crelle's Journal*, Vol. LVI, p. 326, on exactly the same problem in combinations?

One of the incidental results of Kirkman's first paper was his construction of the systems D_{2m}. In the case $2m = 8$ his general rule leads to a system which has little symmetry, but he noticed that there is also a more symmetrical D_8:

1	2	3	4	5	6	7
ab	*ac*	*ad*	*ae*	*af*	*ag*	*ah*
cd	*bd*	*bc*	*bf*	*be*	*bh*	*bg*
ef	*eg*	*eh*	*cg*	*ch*	*ce*	*cf*
gh	*fh*	*fg*	*dh*	*dg*	*df*	*de* .

The seven triples of an S_7 using the symbols 1, 2, ..., 7, together with the 28 triples 1*ab*, 2*ac*, ..., 7*de*, comprise, according to Proposition A, a system S_{15}. Unlike the S_{15} derived from the asymmetrical D_8, this one has an additional property which Kirkman himself did not observe until after the original paper had been published, and which was to become the most famous (though far from the most important) of his discoveries. The 35 triples can be resolved into seven sets of five, in such a way that each one of the fifteen symbols occurs just once in each set of five. In the *Lady's* *and Gentleman's Diary* for 1850 Kirkman challenged the gentlefolk to discover this arrangement for themselves (Query VI, page 48):

> Fifteen young ladies in a school walk out three abreast for seven days in succession: it is required to arrange them daily, so that no two shall walk twice abreast.

It is unfortunate that such a trifle should overshadow the many more significant contributions which its author was to make to mathematics. Nevertheless, it is his most lasting memorial.

Cayley [**2**] was the first to publish a solution, and a few months later Kirkman revealed how he had discovered the problem [**50.1**]. His solution also appeared in the *Diary* for 1851. The general form of the question continued to interest mathematicians for many years. In order that a system S_x may be resolvable into $\frac{1}{2}(x-1)$ sets of $\frac{1}{3}x$ triples, it is clearly necessary that x be congruent to 3 modulo 6. The sufficiency of this condition was finally established by Ray-Chaudhuri and Wilson [**20**] in the late 1960s; their paper contains a list of over forty articles on special cases of the problem, written between 1850 and 1912.

4. In the years immediately following the publication of his work on triple systems, Kirkman wrote papers on a variety of topics. From the sources quoted in them it can be concluded that he was, at that time, reading widely in the mathematical journals and corresponding with several other mathematicians. Clearly, he intended to be taken seriously.

His second major paper [**48.2**] deals with hypercomplex numbers, or 'pluquaternions', as he called them. The ideas can be paraphrased in the following way. A pluquaternion is an expression of the form

$$x = x_0 + x_1 e_1 + \ldots + x_{2n-1} e_{2n-1},$$

where x_i $(0 \leqslant i \leqslant 2n-1)$ is real, and e_i $(1 \leqslant i \leqslant 2n-1)$ is an 'imaginary unit' satisfying $e_i^2 = -1$. The property $e_i e_j + e_j e_i = 0$ emerges as a consequence of the assumptions. In order to define a multiplication for pluquaternions, it would be sufficient to impose relations of the form $e_i e_j = \pm\, e_k$ for each pair of subscripts i and j. Equivalently,

$$e_i e_j e_k = e_j e_k e_i = e_k e_i e_j = \mp 1,$$

$$e_j e_i e_k = e_i e_k e_j = e_k e_j e_i = \pm 1.$$

The subscripts in these relations form a triple system S_{2n-1}, since each pair occurs just once in some relation. Each triple has two possible cyclic orderings, and one must be associated with the sign $+1$, the other with -1, as indicated above. If the multiplication is to be well defined, the signs must be chosen consistently. Kirkman refers to a letter from Cayley which gave him a 'pregnant hint' about these matters, and he begins by investigating the case of seven imaginary units. He finds eight possible sets of consistent defining relations, one of which is

$$\begin{array}{lll} e_1 e_2 e_3 = -1, & e_1 e_4 e_5 = -1, & e_1 e_6 e_7 = -1, \\ e_2 e_5 e_7 = -1, & e_2 e_4 e_6 = +1, & e_3 e_4 e_7 = -1, \\ e_3 e_5 e_6 = -1. & & \end{array}$$

These are the Cayley numbers, discussed by Graves and Cayley some years earlier. Kirkman shows that a similar construction is impossible if there are more than seven imaginary units, and goes on to investigate the possibility of a multiplication taking values in a space of dimension larger than that of the factors. All this, of course, has been studied intensively by later writers. In the second part of the paper he investigates the identities involving sums of squares which are consequent upon such multiplications of pluquaternions. A subsequent paper [**50.3**] discusses in detail the pluquaternions with 15, 23, and generally $8m-1$, imaginary units.

Three papers on geometry also appeared at this time. Two of them [**49.1**, **51.1**] deal with general questions of algebraic geometry, and the other one [**49.2**] is concerned with Pascal lines. If a, b, c, d, e, f are points on a conic, their Pascal line is the line which goes through (ab, de), (cd, fa) and (bc, ef). By permuting the points in all possible ways, 60 different Pascal lines are obtained. In 1832 Steiner had shown that these Pascal lines concur in threes, passing through 20 points, one such point on each line. Kirkman proves that there are 60 additional points of concurrence, such that each one lies on three Pascal lines, and each Pascal line contains three of the points. He gives a detailed account of the whole configuration: concurrences, coincidences, and polarities abound, and he revels in the discussion of them.

Kirkman tells us that his theorems on the Pascal configuration were first published in the *Manchester Courier*. Whatever might have been their impact on the people of Manchester, it seems fairly certain that this work was most influential in gaining Kirkman the respect of his fellow mathematicians. The sixty Kirkman points became part of the folk-lore of nineteenth-century geometry, and they are discussed at some length in the standard texts, such as Salmon's *Conics*.

Yet another aspect of his mathematical work in these years stemmed from his experience in tutoring mathematics. He believed that children dislike mathematics, not because of the intrinsic difficulty of the subject, but because they find it hard to remember formulae. (That is a matter of opinion.) His answer to the 'unpronouncableness of formulae' was Mnemonics. He wrote a paper on this subject in the Manchester *Memoirs* [**48.1**], and also a book, entitled *First Mnemonical Lessons* [**52.1**]. This is the book which De Morgan declared to be 'the most curious crochet I ever saw', and De Morgan had seen many. Written in the form of a dialogue between the author and his nephews and nieces, it consists of explanations of elementary parts of mathematics, which are mainly clear and concise, each followed by the enunciation of a mnemonic related to it. Uncle Penyngton advises his unfortunate niece Jane [**52.1**, p. 2]:

> ... I recommend you strongly, *before you begin* to try to remember these properties, to learn perfectly by heart the following mnemonical aids ... and to teach them to your ear and to your tongue, each of which has a memory of its own, by saying them again and again with a sing-song repetition, marking well the accented syllables.
>
> [1] Line cutting páralls. makes álter. ints. équal,
> and éxt. equal ínt. op.
> And cutter cuts paralls., if equal altér. ints.,
> or ext. equal int. op.

The book was not a success.

5. In addition to writing papers on pluquaternions, Pascal lines, and mnemonics, Kirkman continued with his studies of combinatorial problems. These investigations

were directed towards various aspects of the 1844 Prize Question, a subject which, in the nineteenth century, came to be known as 'Tactic', and is now the Theory of Designs. It will be convenient to use the modern terminology here. A t-design, with parameters (v, k, λ), is a family of k-subsets (often called blocks) of a v-set, with the property that each t-subset occurs just λ times in a block. Sometimes one speaks of a t-(v, k, λ) design.

The original Prize Question was concerned with q-$(n, p, 1)$ designs, except that the condition that each q-subset should occur exactly once was not imposed—an omission which led to some confusion. In a "Note on an Unanswered Prize Question" [**50.2**], Kirkman referred to the lack of progress on the general form of the problem, and solved some very interesting special cases. Two of his theorems are:

> THEOR. I. If r be any prime number, r^{m+1} symbols can be formed into $(r^{m+1}-1):(r-1)$ columns of r-plets, or combinations of r together, each column containing all the symbols, and so that every duad shall be once and once only employed.

> THEOR. II. If r be any prime number, (r^2+r+1) symbols can be formed into (r^2+r+1) $(r+1)$-plets, so that every duad shall be once and once only employed.

Theorem I asserts the existence of a 2-design with parameters $(r^{m+1}, r, 1)$, which can be resolved into a number of 1-designs with the same parameters. Nowadays this construction would probably be given in a geometrical context: the 'r-plets' correspond to lines in the $(m+1)$-dimensional affine space over the field with r elements, and the 'columns' are sets of parallel lines. Kirkman's proof is purely arithmetical; in fact he proves a complicated generalisation of Theorem I. The deduction of Theorem II from Theorem I goes as follows:

> In theorem I let $m = 1$; we can make $r+1$ columns each of r r-plets; and by prefixing a different new symbol in every column, we have $(r+1)$ columns of r $(r+1)$-plets each. The $r+1$ new symbols are combined with all the r^2 old ones, and make one $(r+1)$-plet more; hence we have (r^2+r+1) $(r+1)$-plets, made with r^2+r+1 symbols, theorem II is demonstrated.

This too could be interpreted geometrically, in terms of adding a 'line at infinity' to an affine plane. For this reason, the resulting 2-design with parameters $(r^2+r+1, r+1, 1)$ is known as a projective plane, of order r. In the rest of the paper there is more discussion of resolvable designs and the related questions of the 'schoolgirls' type. For example, it is demonstrated that the set of all 84 triples of nine symbols can be split into seven disjoint 2-(9, 3, 1) designs, each of which can be further resolved into four 1-(9, 3, 1) designs.

A couple of years later came the announcement of many new 'Theorems in the Doctrine of Combinations' [**52.2**], and soon afterwards proofs of them were published [**53.1**]. In this paper many of the basic constructions of the modern theory of designs may be found. Two disjoint 2-(7, 4, 2) designs (biplanes) are constructed by taking complements of triple systems S_7. It is shown that there is a 2-$(15, 3, \lambda)$ design (without repeated blocks) for each value of λ in the range $1 \leqslant \lambda \leqslant 13$. The first non-trivial 3-designs are constructed; they have parameters $(2^n, 4, 1)$ and they are resolvable into 1-designs. Kirkman constructs the 3-designs by combinatorial means, although once again a geometrical interpretation is possible, in terms of planes in the affine space of dimension n over the field with two elements.

Although these results were all quite new, it might be argued that Kirkman's most forward-looking combinatorial paper was the one [**57.2**] which appeared a few years later in a journal even more obscure than usual, the *Transactions of the Historical Society of Lancashire and Cheshire.* In this paper he claims to give 'the first instance of a purely tactical problem being brought *completely* under the domain of Algebra'. He sets out to construct projective planes in which each of the blocks can be obtained from the first by cyclic permutations. Thus he arrives at the concept of a *difference circle*, now called a cyclic difference set, as well as the idea of a multiplier which preserves the resulting design. On the constructive side, he finds difference circles for the projective planes of prime order 2, 3, and 5, and also for the orders 4 and 8. He remarks that 'it was a matter of surprise ... to discover by accident' that there is a difference circle of order 4, thus proving that the order of a projective plane need not be a prime. He admits that he cannot demonstrate the non-existence of a plane of order 6, but it is 'improbable'.

In this series of papers Kirkman has established an incontestable claim to be regarded as the founding father of the theory of designs. Among his contemporaries, only Sylvester attempted anything comparable, and his papers on Tactic seem to be more concerned with advancing his claims to have discovered the subject than with advancing the subject itself. Not until the *Tactical Memoranda* **[18]** of E. H. Moore in 1896 is there another contribution to rival Kirkman's.

6. The decade from 1853 to 1863 was, in terms of mathematics, the most productive of Kirkman's life. During that time he attacked, and claimed to have defeated, two theories, Polyhedra and Groups. In order to understand what he meant by these claims his papers must be considered in their historical context.

His first paper on polyhedra **[53.2]** was read to the Manchester Society on 13 December 1853. (In this paper he used the spelling 'polyedron', and he continued to do so for the rest of his life. He considered the usual spelling to be an affectation, but his arguments are neither clear nor convincing [**55.2**, p. 418].) The paper **[53.2]** begins with a proof of Euler's formula, from which several simple properties of polyhedra are deduced. The class of polyhedra in which every vertex (or *summit*, in his terminology) is trivalent (*triedral*) is the subject of special attention, and he considers in detail the case when there are n faces and one face (the *base*) has $n-1$ vertices. The methods are illustrated by the complete enumeration of the 8-edra with triedral summits. In modern terms, he lists the planar 3-connected trivalent graphs with 12 vertices. He remarks that

> ... the reader will gladly forego the consideration of the cases in which $n > 8$, to say nothing of the polyedra whose summits are not all triedral.

But his readers were not so lucky, since he was soon to fill many hundreds of pages with the investigation of just those problems.

Eighteen months later, Kirkman sent two papers on polyhedra to the Royal Society in London. The first one, entitled "On the enumeration of x-edra having triedral summits and an $(x-1)$-gonal base" **[55.1]**, is a continuation of his Manchester paper. The enumeration is carried out by a recursive method, in which the 'continual evanescence' of triangular faces plays a major part. Some rather unwieldy formulae are obtained, and they are illustrated by explicit calculations for the case $x = 10, 11$, and 12. Even at this early stage, it is clear that he had concluded that the difficulty in the enumeration of polyhedra lies, not in the method of generation, but in the recognition of when the same polyhedron is generated in several different ways.

The second of the 1855 papers **[55.2]** is not part of the main stream of his polyhedral investigations, but it has some historical interest. It is concerned with *closed polygons* in polyhedra: that is, circuits of edges which pass through each vertex just once. Kirkman explains that the closed polygons can be employed as a means of representing polyhedra, and he states some propositions which purport to guarantee their existence. As stated, the propositions seem to be either trivial or false. Rather more significant is the fact that he gives a simple (and correct) criterion for the non-existence of closed polygons, and an example based on the 'cell of a bee'. Thus, in 1855, he had already made a contribution, albeit negative, to the general study of closed polygons. Nowadays the closed polygons are known as Hamiltonian circuits, since Hamilton encountered them in his Icosian Calculus and made them famous with his Icosian Game. However, Hamilton's idea was not conceived until 1856, and it involved only one polyhedron, the regular dodecahedron, whereas Kirkman had discussed the general problem a year earlier.

In 1856 Kirkman sent two more papers on polyhedra to the Royal Society. These are rather longer than his contributions for the previous year, and they are by no means easy reading. The first one **[56.1]** is devoted to the study of self-dual (*autopolar*) polyhedra. It begins with an explanatory note, written by Cayley, 'who has wisely judged that to this investigation of a subject so new and intricate, some such statement should appear by way of introduction'. Kirkman's main theorem is a fundamental result on the recursive generation of polyhedra, which in modern times is known as 'the wheel theorem'. Roughly speaking, it asserts that every polyhedron can be reduced, by deletion and contraction of edges, to a pyramid. Applying the technique suggested by this result, he develops methods for the enumeration of autopolar n-hedra, and carries out the enumeration as far as $n = 8$.

The next paper **[56.2]** is 'On the k-partitions of the r-gon and r-ace'. The problem is to determine the number of ways in which an r-gon can be partitioned by $k-1$ non-crossing diagonals; two partitions are regarded as the same if they are equivalent under the action of the dihedral group on the vertices of the r-gon. In fact, this is a generalization of the problem investigated in two earlier papers **[53.2, 55.1]**. If a trivalent polyhedron has x faces and one of them is an $(x-1)$-gon, then its dual is a polyhedron which has x vertices, one of them $(x-1)$-valent, and all faces triangular. Removing the $(x-1)$-valent vertex and the faces incident with it leaves an $(x-1)$-gon partitioned into $x-3$ triangles—that is, the case $k = r-2$ of the general problem stated above. Naturally, the symmetries of the base must be taken into account, since equivalent partitions correspond to the same polyhedron. Kirkman begins with an account of the various axes of symmetry. He then remarks that in order to enumerate the k-partitions, it is useful to know the number of *k-divisions*—that is, the number of configurations when the symmetry of the r-gon is disregarded. Suppose this number is $\Delta(r, k)$. Using a beautifully simple argument, he obtains the following recursive formula for Δ:

$$2k\Delta(r, k) = r \sum_{h=0}^{r-4} \sum_{s=0}^{k-2} \Delta(h+3, s)\, \Delta(r-h-1, k-s-2)\,.$$

Calculations based on this recursion lead him to propose the following explicit formula for Δ,

$$E(r, k) = \frac{(r)_{k-1}(r-k-1)_{k-1}}{(k-1)!\, k!},$$

where $(n)_m = n(n+1)\dots(n+m-1)$. However, the proof that E satisfies the recursion is left to 'the learned and industrious reader', and he gives only an unconvincing heuristic demonstration. Cayley [3] soon gave an equally unconvincing argument that E satisfies the recursion, a fact that was finally established with due rigour by G. N. Watson in 1963 [28]. (In 1891, possibly when his collected papers were being prepared for publication, Cayley returned to the problem and gave a long and complicated direct proof that $\Delta(r, k) = E(r, k)$ [5].) The remainder of Kirkman's paper is taken up with a series of intricate calculations which are required to determine the number of k-partitions of an r-gon. The resulting equations are so complicated that the general problem is solved only in principle, although the author does go through the calculations involved in checking the case $k = 8$, $r = 10$.

Towards the end of 1857 Kirkman wrote two more papers on polyhedra. The first one [**57.3**] develops the idea of classifying polyhedra by reduction to pyramids. He says that a polyhedron is *r-gonous* if it can be reduced by deletion and contraction of edges to an r-pyramid, but cannot be so reduced to an 'ampler pyramid'. The first class of r-gonous polyhedra is constructed by partitioning the base of the r-pyramid and simultaneously performing a dual operation on its crown. This class is enumerated in the paper. He describes all 8 of the 4-gonous polyhedra (there are just 3 which belong to the first class) and gives some other examples, including the regular dodecahedron (8-gonous of the second class). In the description of this example, the famous 'Hamiltonian circuit' is used. The last paper of this batch [**57.4**] is a simplified and improved account of the result of an earlier one [**55.1**].

It is convenient to break the sequence of papers at this point, even though the work was to continue without interruption. The story of Kirkman's 'general solution' to the problem of polyhedra needs a little more introduction.

7. By 1857 Kirkman had established himself as a mathematician. He had published papers on combinatorics, geometry, hypercomplex numbers, and polyhedra, as well as a couple of papers on partitions of numbers [**54.1**, **57.1**]. As a consequence of this flood of work, the leading British mathematicians of the day had come to respect his originality and his industry. He had corresponded regularly with Cayley, and it was probably at Cayley's instigation that he was proposed for a Fellowship of the Royal Society in 1857. The citation refers to his work on quaternions and partitions. The coveted F.R.S. was conferred on 15 June 1857—a remarkable honour (even then) for a middle-aged country clergyman, who had been active as a mathematician for about ten years only.

For a man of Kirkman's character, such recognition was not a laurel to rest on, but a quickthorn to spur ambition. If the critical years of his private life were 1839–41, when he was 'enticed' to Croft and was married, then the critical years of his mathematical life must be 1857–58. Once again he was enticed, but this time the expected reward was fame, rather than comfort, peace, and security.

At this time, it was the custom of the Académie des Sciences in Paris to propose yearly a subject for a Grand Prix de Mathématiques. The question for 1855 had been too difficult, and the prize was held over to 1857, when, no candidates having come forward, it was adjudged to be impossible. Thus, in February 1858, the Prize Commission proposed that the original question should be replaced by the following: Perfectionner en quelque point important la théorie géométrique des polyèdres. The prize, a gold medal worth 3,000 francs, was to be awarded in 1861.

It is not hard to imagine the thrill with which Kirkman read the announcement of the Grand Prix in the *Comptes Rendus* [7]. His mathematical career had been founded on the more parochial challenge of the Prize Question in the *Diary* for 1844. Now, here was a prize question of international repute, and the subject was one in which he had already made great strides. But that was not all. On the next page of the *Comptes Rendus* he discovered another challenge. The prize question for 1847 had also proved to be inordinately difficult; the competition had been prolonged for many years, but eventually it had been decided to substitute a quite different question.

> Quels peuvents être les nombres de valeurs des fonctions bien définies qui contiennent un nombre donné de lettres, et comment peut-on former les fonctions pour lesquelles il existe un nombre donné de valeurs?

This was proposed for the Grand Prix of 1860. It was a sequel to the work of Cauchy and others, on groups of substitutions and the corresponding 'many-valued functions'. If π is a permutation of the set $\{1, 2, \ldots, n\}$, and f is an expression in the variables $x_1, x_2, \ldots, x_n$, then we may define a new expression πf by the rule

$$(\pi f)(x_1, x_2, \ldots, x_n) = f(x_{\pi 1}, x_{\pi 2}, \ldots, x_{\pi n}).$$

Thus, there is associated with f a group G of permutations, consisting of those π for which $\pi f = f$. The number of distinct expressions which can be derived from f by the action of all $n!$ permutations of the subscripts is equal to the index of G in the symmetric group S_n. In the nineteenth century it was customary to refer to this number as 'the number of values of f'. For example, Ruffini had shown that there is no function of five variables which takes 3, 4, or 8 values, which should be interpreted as meaning that there is no subgroup of index 3, 4, or 8 in S_5. The old terminology seems unsatisfactory today, but it was clear then.

Kirkman probably knew enough to realise that the prize question for 1860 was very much in the French tradition. He did not know that Bertrand, a member of the Prize Commission, was giving a course on Cauchy's work that very year (1858) at the Collège de France. Nor did he know that among the class were two rather bright young Frenchmen, Camille Jordan and Émile Mathieu [**16**]. Even if he had known, it is unlikely that the knowledge would have influenced him. As he read the *Comptes Rendus*, a grand conception of his mathematical future began to form in his mind: he would enter for, and win, both Prizes.

8. A memoir submitted for the prize of 1860 had to be received in Paris by 1 July of that year. This meant that Kirkman had just over two years to make a contribution to the theory of groups, while at the same time continuing his work on polyhedra for the prize of 1861. The fact that he succeeded in submitting a long memoir, written in French, is thus remarkable in itself.

His first publication on groups is a short note [**60.1**] in the Report of the British Association meeting held in Oxford in August 1860. In it he refers, somewhat cryptically, to 'a memoir which will shortly see the light'. Undoubtedly this is the memoir which he had sent to Paris, and which at the time of the Oxford meeting would have been under consideration by the Prize Commission. The memoir was eventually published [**61.2**], and so it is possible to assess what he had achieved.

He begins with some basic definitions and theorems, involving the right cosets (*derangements*) and left cosets (*derivates*) of a permutation group G, regarded as a subgroup of S_n. The permutations P which belong to the normaliser $N(G)$ of G are characterised by saying that $PG = GP$ is a *derived derangement* of G. It is shown that if G has order k and the number of subgroups conjugate (*equivalent*) to G in S_n is m, then the number of derived derangements of G is $n!/km$ (from which it follows that m is equal to the index of $N(G)$ in S_n). The group G is said to be *modular* if it consists of another group H and some derived derangements of H, that is, if G has a proper subgroup H and is itself a subgroup of $N(H)$—in this case H is a normal subgroup of G. He proceeds to construct extensions of cyclic groups, including the affine groups of permutations $x \mapsto \alpha x + \beta$, of prime degree. By a further process of extension, he constructs the projective groups of order $N(N-1)(N-2)$ and degree N, where $N-1$ is prime, and remarks upon their 3-fold transitivity. He then considers split extensions (*grouped groups*) and direct products (*woven groups*). Finally, he explains how a 'many-valued function' with specified properties may be constructed from a suitable group of permutations.

Much of the material in the memoir is not new, but, if nothing else, it demonstrates that Kirkman was very quick to learn. He had mastered the works of Cauchy, and the little that had been written in English, concerning groups. He had probably seen the papers that Mathieu had published in the *Comptes Rendus* for 1858 and 1859, which contained, without proof, some results about the affine and projective groups. On this raw material he had, rightly or wrongly, imposed a notation and terminology of his own invention. Also, he had employed to advantage his formidable talent for combinatorial constructions.

In March 1861 the report of the Commission for the Grand Prix of 1860 was published [**19**]. Three memoirs had been submitted. Numbers 1 and 2 were highly commended—these were by Mathieu and Jordan respectively. Number 3 (Kirkman) was described in the following terms.

> ... Mémoire no. 3 se fait remarquer dès le début par une notation ingénieuse qui est certainement susceptible d'apporter des simplifications dans l'étude des groupes de substitutions; mais ce travail n'est qu'une ébauche; et malgré son étendue il renferme peu de faits nouveaux ou réellement importants.

The Commission conceded that all three candidates had achieved something, but not, in their view, enough to justify the award of the prize. History has been a little kinder, at least to Mathieu and Jordan, for their memoirs [**14, 9**] are now regarded as important milestones in the development of group theory.

Soon after hearing the outcome of the Grand Prix, Kirkman communicated his results to the Manchester Literary and Philosophical Society (29 April 1861). The *Proceedings* of the Society contain a statement of the Commission's decision, and a summary of the main results of his essay [**61.1**]. The essay itself appears in the *Memoirs*, together with a long supplement [**61.2**], which begins with a brief comment on the Grand Prix.

> Three memoirs were presented, but no prize was awarded. Not the briefest summary was vouchsafed of what the competitors had added to science, although it was confessed that all had contributed results both new and important; and the question, though proposed for the first time for the year 1860, was, contrary to the *very frequent* custom of the Academy, withdrawn from competition.

The remainder of the supplement is devoted to some new researches, in which a major part is played by what Kirkman called *didymous factors* of a permutation. If γ

is a cyclic permutation of order n, then its didymous factors are the n involutions which, together with the powers of γ, form a dihedral group of order $2n$. One application of this concept is a new construction of the projective groups, different from the one given in the main memoir and from that given by Mathieu. A similar construction is applied to the projective groups of degree $q+1$, when q is a power of a prime. He asserts that in the prime case the group is self-normalising (*maximum*), whereas in the prime power case it is not. As an example of the latter fact, he constructs a group of degree 9 and order 1512, which is now known as $P\Gamma L\ (2, 8)$ or the first of the Ree groups. In 1892, F. N. Cole claimed to have discovered this group [**6**].

Despite his evident displeasure at the result of the Grand Prix, Kirkman did not abandon the study of groups. Three minor papers were published within a year of the first memoir: another report to the British Association [**61.4**], some "Hints on the theory of groups" [**61.5**], and a paper in which the schoolgirls problem is discussed from a group-theoretical point of view [**62.2**]. The 'Hints' paper is probably the first systematic account in English of the elements of group theory. In April 1862 he communicated some more results "On non-modular groups" to the Manchester Society [**62.3**, **62.4**]. In these papers he employs combinatorial arguments based on didymous factors (which he calls the 'tactical method') to construct the simple groups of order 168 and 660 as groups of degree 7 and 11. He agrees with Galois that it is unlikely that there are any groups of degree p and order $\frac{1}{2}p(p^2-1)$ when p is a prime greater than 11, and he gives a tactical proof of this fact when $p = 23$. Finally, he states some combinatorial propositions which he had discovered in the course of this work, among them the existence of a 2-$(11, 5, 2)$ design, or biplane with 11 points.

The next paper on groups [**63.1**] begins with the correction of a statement in the prize essay, concerning a particular maximum group of degree 16. In the original paper Kirkman had given the order of the group wrongly; here he explains the error and gives the correct order, 322560. This is, in fact, the group of affine transformations $\mathbf{x} \mapsto A\mathbf{x}+\mathbf{b}$ of the vector space of dimension 4 over GF(2), and the translations form a normal, elementary abelian, subgroup of order 16. Kirkman's discovery of the group is an impressive testimony to the power of the tactical method in his hands. He goes on to claim that the general case, which Jordan had discussed in his memoir [**9**], can also be treated by the tactical method. He ends with a complicated quibble about one of Jordan's theorems.

The last of Kirkman's major publications on groups is entitled "The Complete Theory of Groups" [**63.2**]. The grand title indicates that he had solved, to his own satisfaction, the problem of compiling lists of transitive groups. His method is a recursive one, proceeding from a *base* group to a transitive group in which the base is the subgroup fixing one symbol. A group is described by its *title*, which gives the classes of the constituent permutations. He asserts that:

> In every case in which a possible group is indicated by a completed title, I have thus far found that the construction follows by the most simple and rapid *tactical methods*.

He refers the reader to a memoir, of which this is an abstract, for general theorems justifying the procedure. However, the abstract does contain a list of transitive groups of degree 10 or less. This list, with the later corrections, justifies his claim to have an effective method for relatively small groups. The memoir containing a full account of the work was sent to the Literary and Philosophical Society of

Manchester, but it was never published, possibly because it contained intemperate comments on the result of the 1860 Grand Prix. By 1883 the memoir had disappeared completely [**83.2**, p. 81].

On this falling note, the five years of Kirkman's labours on the theory of groups came to an end. In more propitious circumstances younger men might have been encouraged to follow in his footsteps, but it was not to be, for the theory of groups lay dormant in England for thirty years, until Burnside took it up in the 1890s. By that time Kirkman's work had been almost completely forgotten, and the subject itself had been transformed by the continental mathematicians.

9. Although he was proud of his work on groups, Kirkman considered it to be of secondary importance compared to his work on polyhedra. In 1858, when he had formed the grand conception of winning the prizes of 1860 and 1861, he already knew far more about polyhedra than he knew about groups, and he had resolved to work on both subjects concurrently.

At this time, his work on polyhedra was being published in Manchester. There are three notes in the *Proceedings* for 1858, of which the first [**58.1**] is the most significant. It contains an interesting account of some of the difficulties which had been encountered in his earlier attempts to classify polyhedra.

> I have tried to refer the figure to its amplest, *i.e.* most angled, face as a base, and the figure has obstinately refused to acknowledge one face rather than another for a foundation.... I have endeavoured to generate the figures by defined processes from pyramids; but not only is the solid in general deducible from different pyramids, but often in a great number of ways from the same pyramid, no one of which will allow another to be a better way than itself. And when I have attempted by authority to put the work into the hands of some one of these methods, all equally clamorous and confident, I have invariably found that, in spite of all I could do to prevent it, the operator would persist in carving the same polyedron over and over again in different postures, so that it was impossible to know how many really distinct ones had been generated.

He suggests a new line of inquiry, based upon a kind of incidence matrix and some equations associated with it. However, he did not persevere with this approach, for in his next major paper [**59.1**] he returned to the old methods. This paper is concerned with the enumeration of partitions of a polygon in terms of the number of *marginal faces* (faces which have at least two successive edges in common with the polygon). The author accomplishes the desired enumeration when there are just two marginal faces, and claims that it is possible when there are three. But the general case presents great difficulties. At the end of the paper he announces that he has found an inductive method for the enumeration of the *j-nodal k-reticulations* of the *r*-gon—that is, the *k*-partitions of the *r*-gon when *j* additional internal vertices are allowed. This represents a substantial advance in the assault on the general problem of enumerating polyhedra. However, the reader is warned that 'the subject is far too extensive to be here discussed, and the process of computation is very tedious'.

This was probably the time (March 1859) when Kirkman began to plan his 'complete solution' to the problem of polyhedra. At first it was written in French, for the Grand Prix of 1861, but the result of the 1860 competition changed all that. Thus it came about that, on 10 May 1861, the Royal Society in London received a communication [**61.3**] announcing the completion of his labours on polyhedra. It contains an outline of the methods used, in which particular emphasis is placed on symmetry. The memoir giving details of the theory arrived at the Royal Society on

3 January 1862, intended for publication in the *Philosophical Transactions*. Only one thing was clear—it was exceedingly long. There were twenty-one sections listed in the introduction, for example:

> *Section sixteenth* analyses a *polyarchipolar summit;* effaceables are restored about all like archipoles; the *polyarchine reticulation* laid bare by the removal of these polar summits is reduced, and afterwards constructed with enumeration of results: the formulae for *polyarchine coronation* are given, and the results of effacement are enumerated and registered.

The referees read a few pages of this, and pronounced it unreadable. The author offered to help them read it, but the offer was declined [**89.1**, p. 72]. Eventually a compromise was reached: the first two of the twenty-one sections would be published, and the whole memoir would be preserved in the archives of the Royal Society. (It is still there.) The two published sections take up over forty pages of the *Transactions* [**62.1**], so the entire work might have filled a complete volume.

Kirkman begins with an apology:

> It is unfortunate that my previous labours on the partitions of the R-gon, that is, on plane reticulations, are of little utility for this problem of polyedra, by reason of their too great generality, and of their not giving the number of *marginal triangles* in each partition. Yet the fundamental theorem on the k-divisions of the R-gon [**56.2**, p. 225] has been the key to the *greatest difficulty* in this theory, which is to find the number of the asymmetric plane reticulations which have a given *marginal signature.*

The first section deals with the symmetry of polyhedra. It seems likely that it contains, implicitly, the classification of finite groups of isometries in three-dimensional space, but the terminology is so complicated that it is difficult to translate any of the theorems into modern terms. The second section contains an exposition of the intricate system of Tables which Kirkman had developed for recording his results. The details of the calculations involved are given in the nineteen unpublished sections. In due course the Royal Society was persuaded to publish some of the actual tables [**62.5**], including all the polyhedra with eight or fewer faces, and some of those with nine faces. (Corrections are given in [**71.1**].) Apart from this, all that is known about the general theory is the little that Kirkman himself later wrote about it. For over a century the uncompromising tip of the iceberg has successfully deterred investigation of its submerged portion.

Incidentally, the Académie had several candidates for the Grand Prix of 1861, but the prize was not awarded.

10. The years 1861–63 had been less than kind to Kirkman. He had failed to obtain recognition in Paris, his 'complete solution' to the problem of polyhedra had been consigned to the archives in London, and his final memoir on groups lay mouldering somewhere in Manchester. It is perhaps not surprising that he was bitter and disillusioned. He never published another paper in the journals of the Royal Society, and between 1868 and 1891 he published nothing in Manchester. The consequences of his break with the institutions were bad enough, but, to make matters worse, his resentment spilled over and he conceived grudges against individuals.

Until this time his relations with Cayley had been cordial. The correspondence between the two men is mentioned, with the customary flowery compliments, in one of his earliest papers [**48.2**]. Cayley's introductory note to the first long paper on polyhedra [**56.1**] may have been a sign that he was uneasy about Kirkman's habit of inventing complex terminology and using it at great length. The arrival of the great polyhedral memoir was the breaking point. Clearly, there were problems about putting a memoir of that length in the *Philosophical Transactions*, and Cayley cannot have been alone in thinking that it could not be printed in full. Unfortunately, Kirkman not only held Cayley entirely responsible, but hinted darkly that his motives were questionable [letter to Sir John Herschel, undated, Royal Society Library]. The fact that Cayley himself wrote an article on polyhedra, and published it in Manchester in 1862 [**4**], added insult to injury. In truth, Cayley only rediscovered results already found by Kirkman in his very first work on the subject [**53.2**].

If the rift between Kirkman and Cayley was unexpected, since Cayley was the most equable of men, then the rift between Kirkman and Sylvester was more predictable. Both men were, to say the least, unusual. Sylvester's interest in combinatorial problems began in his youth, and MacMahon [**13**, p. xi] relates how, at the age of 15, he solved a problem in combinations posed by the proprietors of an American lottery. Kirkman tells a very similar story in one of his papers [**57.2**, p. 128], and it may be inferred that he had heard it from Sylvester himself. There is nothing to suggest any coolness between them at that time (1857). However, in 1861 Sylvester published a short series of papers on 'Tactic', a subject which he claimed to have invented. His treatment of Kirkman in these papers is decidedly odd. Speaking of the schoolgirls problem, he asserts [**23**] that he had long been aware of the result and, furthermore,

> ... it is not improbable that the question, under its existing form, may have originated through channels which can no longer be traced in the oral communications made by myself to my fellow-undergraduates at the University of Cambridge long years before its first appearance, which I believe was in the *Ladies' Diary* for some year which my memory is unable to furnish.

In another paper [**24**], he discusses the problem of arranging the 84 triples of 9 symbols in seven disjoint triple systems, each one of them being further resolved into 'days', as in the schoolgirls problem. He claims to have discovered how to do this 'very many years ago', but concedes that it was first published 'by a mathematician whose name I forget'. In a postscript he admits that the original reference is a paper [**50.2**] by Kirkman.

The forgotten man replied promptly, and with nicely chosen words [**62.2**], to the allegation that he had filched the schoolgirls problem.

> My distinguished friend Professor Sylvester ... volunteers *en passant* an hypothesis as to the possible origin of this noted puzzle under its existing form. No man can doubt, after reading his words, that he was in possession of the property in question of the number 15 when he was an Undergraduate at Cambridge. But the difficulty of tracing the origin of the puzzle, from my own brains to fountain named ... at that University, is considerably enhanced by the fact that, when I proposed the question in 1849, I had never had the pleasure of seeing either Cambridge or Professor Sylvester.

Although his words and actions did nothing to endear him to the mathematical establishment, Kirkman was clearly anxious to have more contact with the leading mathematicians of the day. After the 1861 British Association meeting in Manchester, Hamilton visited Croft and presented the Rector with a copy of the Icosian Game. Later, Kirkman wrote to him saying that he wished for 'the good fortune to be nearer such a mathematician as you' [**8**, p. 136]. He seems to have sought help from De Morgan, for in the library of Trinity College, Cambridge, there is the following letter from De Morgan to William Whewell, Master of the College, dated 15 October 1863.

> ... there is a man of science very poorly provided for, and the mere mention of him can do no harm ... I allude to Kirkman ... His mathematical papers on polyhedrons and other things are very deep ... Cayley can tell you all about him.
>
> He is buried at Croft and very much desires a better field of action in which he may be able to see a little more of intellectual life, over and above his clerical doings. He has barely £180 a year, and is an active man, moderate, and of orthodox repute. ... He has worked for many years at subjects which will not bring him before the general eye, and is a staunch enthusiast and *con amore* mathematician.
>
> I never saw him but once, but I saw in him a man like either of ourselves, of strong build; and he looks as if he would not be easily tired out.
>
> It might come in your way to say a word for him.

Perhaps it was unwise to mention Cayley's name; at any rate, nothing came of it.

This was the low ebb of Kirkman's life: 'buried' at Croft, nearly 60, and estranged from his fellow-mathematicians. Probably, the fiction that he was not a 'real mathematician' began at this time. It must have been convenient for the mathematical establishment to look on him as a crazy cleric who had invented an amusing puzzle about schoolgirls. And it must be said that he himself did little to dispel this illusion.

11. It is strange that the disappointments which followed his great labours did not deter Kirkman from working at mathematics. Although he never again approached the phenomenal intensity of the years 1855–62, he continued writing and publishing mathematical articles for the rest of his life. Most of this later work was derived, directly or indirectly, from his earlier researches on designs, groups and polyhedra.

Following the demise of the *Lady's and Gentleman's Diary*, its place as a forum for mathematical questions had been taken by the *Educational Times*. Many of the contributors were amateurs, but questions set by the leading mathematicians of the time also appeared regularly. Some of the more interesting questions and solutions were reprinted, together with some additional material, in a half-yearly publication entitled *Mathematical Questions with their Solutions from the Educational Times*. The volumes of this journal, from its inception in 1862 to Kirkman's death in 1895, are a veritable treasury of his later work. There are at least fifty questions posed by him, many of which appear with a 'solution by the proposer', and there are also solutions by him to questions set by other people. Not infrequently his solutions contain somewhat bilious historical comments, relating to his lack of success in the Grand Prix of 1860, or the failure of the Royal Society to do justice to his memoir on polyhedra. Another feature is that some of his questions are expressed in quite appalling verse. Surprisingly, his talent for versification seemed to improve a little as he passed the age of 80, and one of his less excruciating attempts will be quoted in due course.

Several of the contributions to the *Educational Times* contain quite interesting mathematics, and these have been included in the list of Kirkman's major mathematical publications. For example, the solution to Question 1539 **[65.1]** involves some simple applications of his theorems on grouped groups. Three years later, his answer to Question 2696 **[68.4]** contains a 'solution' to the general quintic equation. This stemmed from an earlier note **[68.1]** in the Manchester *Proceedings*, in which he had demonstrated the fallacy in a purported solution by one Judge Hargreave. This was followed two weeks later by Kirkman's own claim concerning 'how to solve the famous quintic by a method which leads directly to the solution of all algebraic equations' [**68.2**]. Two papers on this great discovery were published [**68.3**, **68.4**] before he recognised his error. In September 1868 he admitted his mistake in Manchester [**68.5**], and a more detailed retraction, with uncharacteristic humility, was published in the *Philosophical Magazine* [**68.6**]:

> This restatement and proof of the theorem of my former paper is I believe, all true and in part, I hope, new. The application that I made of it to the solution of equations of the $(n+1)$th degree, if $n > 3$, is alas! all new and not true ... I hope the scientific reader will pardon the nonsense of the latter part of my preceding communication, and I wish that he may live to confess, like myself, that his mathematical powers are the worse for wear.

This little episode illustrates the difficulties of the forgotten man of Croft, and the consequent loss to mathematics in Britain. Kirkman was the man best qualified to understand and disseminate the Galois theory of equations, but his isolation from the mathematical world meant that his gifts were wasted.

In the *Mathematical Questions* for 1869, there is a discussion [**69.1**] by Kirkman of three questions which had been proposed by W. Lea, concerning the existence of a 4-(11, 5, 1) design, a 4-(15, 5, 1) design and a 3-(16, 4, 1) design. Lea himself had already given a solution to the first problem, and now Kirkman provides an alternative construction. He admits that he cannot construct the second design, and says that he will be 'agreeably surprised' if it can be done. (The fact that no such design exists was finally proved by Mendelsohn and Hung [**15**] in 1972.) The third design is more amenable, for it is a special case of the family of 3-$(2^n, 4, 1)$ designs which he had discovered in 1852.

A couple of years later, in a "Note on Question 3167" [**71.1**], he turned again to his lists of polyhedra. The reader of this note will learn that the relative frequency of 4-sided faces in the set of polyhedra with eight faces is 1123/3384. This ineffable information is followed by a long and sarcastic account of the author's grievances against the French Academy and the Royal Society. Such diatribes were to be a recurrent feature of his writings from this time on.

12. In addition to his substantial output of mathematical papers, Kirkman also found time to write many articles and pamphlets on theological and philosophical matters. In his clerical guise he was known as a 'Broad Churchman'. He supported the rebel Bishop Colenso, who believed that the first five books of the Old Testament need not be taken literally, and this action may have affected his chances of preferment in the Church. In the years 1865–73 he published many pamphlets on Church matters, some of them in a series devoted to the cause of 'Free Enquiry and Free Expression'. A list of the pamphlets may be found in the British Museum *Catalogue of Printed Books*, and the *National Union Catalog* of the U.S.A. The titles alone are worth reading: Truth against Tradition, On Clerical Dishonesty, Where is the firmament which God created on the second day? and so on.

Kirkman was also greatly concerned with more general matters of theology and philosophy. His first publications on these topics appeared in the Manchester *Proceedings*—a paper "On the absurdity of ontology" in 1858, and one "On the relation of force to matter and mind" in 1865. Following the failure of the Manchester Society to publish his final memoir on groups, he seems to have transferred his allegiance to the Literary and Philosophical Society of Liverpool, and their *Proceedings* contain several of his philosophical papers as well as a clutch of mathematical ones. The first of the Liverpool papers, Parts I and II of "Philosophy without assumptions", appeared in 1872, followed by Part III in 1874. A book with the same title was published by Longmans, Green and Co. of London in 1876. Kirkman believed in making his own position quite clear, and in the preface to the book he declaims:

> ... I believe that this God in the beginning made all things, not *ex nihilo*, but out of nothing but Himself. I believe that He is at this moment exactly as at the first, if ever there was a first, ... I believe in a Trinity in Unity; namely I believe first in God the Unrevealed and Unrevealable; secondly, in God the Revealed; and thirdly, in God the Revealer.
> ... All this I state for myself, not imposing it on others, nor as a creed received from authority, but as my glorying inference, which I care not to call demonstration, from what I know.

The book is, however, not so much an apology for his own Christian beliefs as a spirited attack on materialism. Not only does he oppose the drift of scientific opinion towards a materialistic and evolutionary philosophy, but he is scathing in his denunciation of the leaders of this movement. It will be recalled that he had attended the Oxford meeting of the British Association in 1860, and he may have been present on the famous occasion when 'Soapy Sam' Wilberforce, Bishop of Oxford, poured scorn on Darwin's theory and was rebuked by T. H. Huxley. From the tone of his book it is clear that, if he had been present, he would have aligned himself, with Disraeli, 'on the side of the angels'.

He is at his most vigorous when he attacks the windy generalizations of the materialists, and the unfortunate Herbert Spencer suffers much at his hands. Spencer had attempted to define Evolution as 'a change from an indefinite incoherent homogeneity to a definite coherent heterogeneity'. Kirkman's rendering, 'a change from a nohowish untalkaboutable all-likeness, to a somehowish and in-general-talkaboutable not-all-likeness', seems to have appealed to P. G. Tait, who had himself crossed swords with Spencer and who described the paraphrase, with little justification, as an 'exquisite translation' [**25**]. Their common dislike of Spencer drew Kirkman and Tait together, and it was to lead, indirectly, to more contributions to mathematics from the elderly Rector of Croft.

13. The series of mathematical papers which Kirkman published in the Liverpool *Proceedings* between 1875 and 1879 was probably intended to bring his great work on polyhedra before a wider audience. Unfortunately, although the papers were written with due allowance for the difficulties of the reader (a feature not notably associated with the earlier papers) the limited circulation of the journal meant that the work remained as obscure as ever. The first of these papers [**75.1**] describes the *janal* dodecahedra—that is, the dodecahedra with two-fold symmetry, while the second [**76.1**] deals with polyhedra which have 14 faces and 14 vertices. The third [**78.1**] contains a complete description of the nonahedra with 9 vertices, and a table of the numbers of nonahedra for each possible number of vertices, all 'very old results ... never before communicated'. The fourth [**78.2**] gives a simple account of a part of his general theory: 'Given the described and constructed P-acral $(Q-1)$-edra, along with the described and constructed symmetrical P-acral Q-edra, to construct directly without omission or repetition the asymmetrical P-acral Q-edra.' The fifth [**79.1**] contains a new method for the study of self-dual polyhedra, which is used to generate all of them having ten or fewer vertices.

A second series of papers on polyhedra arose from a problem put to Kirkman by Tait. The latter, in his work on the four-colour problem, had shown that a four-colouring of the faces of a trivalent polyhedron is equivalent to a three-colouring of its edges, and so he became interested in methods of constructing an edge-3-colouring. One promising approach is based on Kirkman's closed polygons: if a trivalent graph has a Hamiltonian circuit, then an edge-3-colouring is obtained by colouring the edges of the circuit red and blue alternately, and the remaining edges green. Thus Tait was led to conjecture that every trivalent polyhedron has a Hamiltonian circuit. He consulted Kirkman on the subject, and Kirkman sent the problem, in his worst verse, to the *Educational Times*. His own 'solution' [**81.1**] is inconclusive; the question, he says, 'mocks alike at doubt and proof'. (Tait's conjecture was eventually disproved by Tutte [**27**] in 1946.) In the course of his remarks, he mistakenly claims that Hamilton had based the Icosian Game on his observation [**57.3**, p. 160] that the regular dodecahedron has a Hamiltonian circuit. This was repeated by Tait [**26**], but it cannot be true, since the Icosian Game was invented in 1856. Of course, Kirkman had discussed the general question of closed polygons a year earlier.

Although he could not solve Tait's problem, Kirkman's interest in trivalent polyhedra was revived and he wrote several papers on this subject. They are all concerned with the case where no face has fewer than five sides. If there are f_i faces with i sides ($i \geqslant 5$) then simple counting arguments, and Euler's formula, lead to the equation

$$f_5 - (f_7 + 2f_8 + 3f_9 + \dots) = 12,$$

in which, it will be noted, f_6 does not appear. For example, if the faces are all either pentagons or hexagons, then there must be exactly twelve pentagons and the number of hexagons is not determined. Kirkman [**83.3**] gave a list of all nineteen polyhedra of this kind with $1 \leqslant f_6 \leqslant 7$, using the methods of his great unpublished memoir. At about the same time he composed three fanciful verses (Questions 6964, 7081, 7120) for the *Educational Times*, in which the Sultan of Borneo, Apollo and the Muses, and the goddess Minerva become embroiled in the study of trivalent polyhedra. An article in the *Mathematical Questions* [**83.4**] and two papers in the Liverpool *Proceedings* [**83.1**, **84.1**] were needed to resolve their difficulties. Clearly, Kirkman regarded such whimsies as part of a programme to popularize his work on polyhedra. The reader is frequently assured that the current problem is but a trifle, whose solution is merely an exercise in applying the methods of the great memoir. But there is little evidence that the questions were, in any sense, popular—their solutions were almost always given by the proposer.

14. In 1876 Tait had published a long paper on knots, in which he had listed the knots with seven or fewer crossings. At that time he had been deterred from extending his lists because of the considerable amount of tedious labour involved. It must have been natural to put the problem to Kirkman, since he was not easily deterred by such tasks. In fact, the collaboration between the two men resulted in the first tables of knots with 8, 9, and 10 crossings.

Kirkman's first paper [**84.2**] on knots appeared when he was nearly 80 years old. He considered what would now be called the alternating projections of knots and links—in other words, he studied 4-valent, 2-connected plane graphs. If such a graph is given, each vertex can be regarded as the crossing of two threads, and by assigning the crossings over and under alternately the graph is decomposed into a number of circuits, corresponding to closed threads. If there is just one circuit the graph defines a knot, in the modern sense (Kirkman called it a *unifilar* knot), and if there is more than one, it defines a link. He suggests two examples with which 'the beginner can amuse himself': the graph of the regular octahedron, which is trifilar (in fact, the Borromean rings), and the graph obtained by drawing 'a square within a square askew, and filling up with eight triangles', which is unifilar (the bowline knot). These examples belong to his class of *solid* knots, since their graphs are the plane projections of polyhedra. He regards the enumeration of this class as a simple matter, at least for someone who is familiar with his great work on polyhedra. His main efforts are directed towards the enumeration of the remaining class, where the graphs are 2-connected but not 3-connected.

From a topological point of view Kirkman's first work on knots is unsatisfactory, since he regards two knots as different if their graphs are not isomorphic. His knowledge of topological equivalence was scanty, and he writes:

> The right definition, in the sense of Listing and Tait, I find not easy to seize, and I cannot work on reticulations between equivalence and identity, nor pause to consider the deformations of a knot A into a knot B that can be effected by twisting the tape of A. I content myself by exhausting the forms that differ according to my definition; and I leave to a more competent hand the reductions to be made by twisting.

The paper [**84.2**] was communicated to the Royal Society of Edinburgh on 2 June 1884, and in their *Transactions* it is followed immediately by Tait's second paper on knots. Tait acknowledges Kirkman's labours and uses them to extend his census to the '8th and 9th orders of knottiness'.

On 26 January 1885 Tait received from Kirkman the list of graphs required for the tenth order of knottiness, and he set to work to extend his census once more. Kirkman's paper [**85.1**] was again published alongside Tait's. The latter contains a postscript, written in September 1885, in which Tait says that he has now received Kirkman's data for the eleventh order. This time he jibbed at the work of classifying the graphs topologically, but it was eventually completed by C. N. Little.

Perhaps the most interesting aspect of Kirkman's work on knots is the fact that, gradually, he came to understand the topological niceties of the problem. In addition to the two papers in the Edinburgh *Transactions*, he published four notes in the *Proceedings*. The first one [**85.2**] contains proofs of some theorems, specifically related to unifilar knots, which has been stated in his paper on the tenth order of knottiness. The next paper [**85.3**] describes the twisting operations of Listing and Tait, and a new construction of his own, which he names 'unkissing'. This is a procedure which reduces the number of crossings in a knot or link, and he claims that it will be useful in the topological classification of knots with $n-1$ crossings, if the graphs with n vertices are known. He adds that he will soon present Professor Tait with the 'requisite unifilars' on twelve vertices, which will doubtless assist him in the classification problem for the eleventh order. The prospect of receiving yet more reams of information from an octogenarian Rector must have been quite unnerving for poor Tait.

The last two of Kirkman's papers on knots [**86.1**, **86.2**] continue in the same vein. The latter contains an investigation of a 12-filar knot with 180 crossings, which has 'zoneless symmetry of the highest possible complexity'.

15. Thomas Penyngton Kirkman was one of those fortunate men who defy the commonplace generalities about age. He did no original mathematics until he was

40, he flourished between the ages of 45 and 55, and he continued to work at mathematics until his death in his 89th year.

After his entanglement with knots, he took up once more the main themes of his life's work. The routine at Croft Rectory must have been then, as it had been for the preceding forty years, centred on the intellectual life of the Rector. He simply could not exist without doing mathematics and writing mathematics. The *Educational Times* was his sounding-board, and verse remained one of his instruments. Question 7933, probably composed in 1886, is one of his more tolerable efforts. It tells how thirteen foolhardy people assuaged the fury of the gods by arranging themselves into a projective plane:

Thirteen at the board! they gaily mock
    At ancient fear and awe.
Then, gloom and thunder—a baleful shock
Unmans them all, and, conscience-struck,
    They hovering o'er them saw,
Frowning in flame, the angry Puck,
    Who cried: "D'ye brave the law?
Before another year can fly,
Some shall sicken and one shall die."
Small boot to tell what wail and moan
    Arose; ye better far
Read how, by softened Puck, was shown
    The way that woe to bar.
    "Fail not for a year, when the moon is round,
    That four of you repair,
For her noon half-hour, to the rustling mound
    Of Druid rites; and there,
Pacing slow the sacred ring,
With drooping foreheads, softly sing,
    In weather foul or fair,
Praise to the Fairy Queen and King;
    Then loud when noon is gone,—
Titania loveliest, regal Oberon,
    Command that Puck
    Ward off ill-luck
From the sorrowing twelve and one."

"New fours, for thirteen moons, be told
Their penitent watch on the hill to hold;
But no two twice, of the banned thirteen,
May see together the moonlit scene."

Macfarlane, in his biographical essay [12], tells the story behind another verse. Kirkman had sent him a riddle about kinship, to which he published a solution in 1888 [11], and Kirkman then sent the problem to the *Educational Times*:

Baby Tom of Baby Hugh
The nephew is and uncle too;
In how many ways can this be true?

Perhaps the old man was learning, at long last, to be brief.

Unfortunately, he never learnt to conceal his bitterness: two papers on polyhedra [**87.1**, **89.1**], both written when he was over 80, contain the usual rambling stories of his grudges and grievances. It seems however that he was reconciled with the Manchester Literary and Philosophical Society, for four of his last papers appear in their new series of combined *Memoirs and Proceedings*. Two of them deal with groups [**91.1**, **91.2**], and two with partitions of numbers and polygons [**93.1**, **93.2**].

In the summer of 1892 Kirkman resigned the living at Croft and moved a few miles away to Bowdon, near Manchester. He continued to send questions and solutions to the *Educational Times* [**94.1**, **94.2**], writing as always in a unique and vigorous style.

Thomas Penyngton Kirkman, mathematician, died on 4 February 1895. His wife, Eliza, who had sustained him for over fifty years and borne his seven children, died ten days later.

Notes

1. Section 7. Dr P. M. Neumann has pointed out that the prize question for 1860 was first proposed in 1857 (*Comptes Rendus*, 44 (1857), 794). However it seems certain that Kirkman did not learn of it until 1858, when he read the announcement of the Polyhedra question (for 1861) and the Groups question (for 1860) in the same issue of the *Comptes Rendus* [**7**]. In later years he wrote of 'the two glittering medals of 1858' [**71.1**], and of the announcement 'early in 1858' of the two competitions [**89.1**]. Oddly enough, the report of the 1860 Prize Commission [**19**] also refers to the question as having been proposed in 1858.

2. Section 10. Thanks are due to the Master and Fellows of Trinity College, Cambridge, for permission to quote from the correspondence between De Morgan and Whewell.

Major mathematical publications of T. P. Kirkman

46.1 "On a problem in combinations", *Cambridge and Dublin Math. J.*, 2 (1847), 191–204. Dated 23 Dec. 1846.

48.1 "On mnemonic aids in the study of analysis", *Memoirs, Manchester Lit. Phil. Soc.*, 9 (1851), 29–45. Read 8 Feb. 1848.

48.2 "On pluquaternions, and homoid products of sums of n squares", *Phil. Mag.* (3), 33 (1848), 447–459, 494–509. Dated 19 Dec. 1848.

49.1 "On the meaning of the equation $U^2 = V^2$", *Cambridge and Dublin Math. J.*, 5 (1850), 97–110.

49.2 "On the complete hexagon inscribed in conic sections", *Cambridge and Dublin Math. J.*, 5 (1850), 185–200. Dated 3 Dec. 1849.

50.1 "On the triads made with fifteen things", *Phil. Mag.* (3), 37 (1850), 169–171. Dated 6 Aug. 1850.

50.2 "Note on an unanswered prize question", *Cambridge and Dublin Math. J.*, 5 (1850), 255–262. Dated 23 Aug. 1850.

50.3 "On bisignal univalent imaginaries", *Phil. Mag.* (3), 37 (1850), 292–301. Dated 3 Sept. 1850.

51.1 "On linear constructions", *Memoirs, Manchester Lit. Phil. Soc.*, 9 (1851), 279–296. Read 18 Mar. 1851.

52.1 *First mnemonical lessons in geometry, algebra, and trigonometry* (John Weale, London, 1852).

52.2 "Theorems in the doctrine of combinations", *Phil. Mag.* (4), 4 (1852), 209. Dated 6 Aug. 1852.

52.3 "Note on combinations", *Phil. Mag.* (4), 5 (1853), 11–12. Dated 3 Dec. 1852.

53.1 "Theorems on combinations", *Cambridge and Dublin Math. J.*, 8 (1853), 38–45.

53.2 "On the representation and enumeration of polyedra", *Memoirs, Manchester Lit. Phil. Soc.*, 12 (1855), 47–70. Read 13 Dec. 1853.

54.1 "On the k-partitions of N", *Memoirs, Manchester Lit. Phil. Soc.*, 12 (1855), 129–146. Read 10 Jan. 1854.

55.1 "On the enumeration of x-edra having triedral summits and an $(x-1)$-gonal base", *Phil. Trans. Royal Soc. London*, 146 (1856), 399–412. Received 13 June 1855.

55.2 "On the representation of polyedra", *Phil. Trans. Royal Soc. London*, 146 (1856), 413–418. Received 6 Aug. 1855.

56.1 "On autopolar polyedra", *Phil. Trans. Royal Soc. London*, 147 (1857), 183–216. Received 19 June 1856.

56.2 "On the K-partitions of the r-gon and r-ace", *Phil. Trans. Royal Soc. London*, 147 (1857), 217–272. Received 13 Nov. 1856.

57.1 "On the 7-partitions of X", *Memoirs, Manchester Lit. Soc.*, 14 (1857), 137–150. Read 7 April 1857.

57.2 "On the perfect r-partitions of r^2-r+1", *Trans. of the Hist. Soc. of Lancashire and Cheshire*, 9 (1856–7), 127–142. Read 21 May 1857.

57.3 "On the partitions of the r-pyramid, being the first class of r-gonous x-edra", *Phil. Trans. Royal Soc. London*, 148 (1858), 145–161; (also *Proc.*, 9 (1857–9), 4–5). Received 14 Oct. 1857.

57.4 "On the triedral partitions of the x-ace, and the triangular partitions of the x-gon", *Memoirs, Manchester Lit. Phil. Soc.*, 15 (1860), 43–74; (also *Proc.*, 1 (1857–60), 11). Read 17 Nov. 1857.

58.1 "On the general solution of the problem of the polyedra", *Memoirs, Manchester Lit. Phil. Soc.*, 15 (1860), 92–103; (also *Proc.*, 1 (1857–60), 25–28). Read 26 Jan. 1858.

58.2 "New formula in polyedra", *Proc. Manchester Lit. Phil. Soc.*, 1 (1857–60), 41.

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61.1 "Theorems on groups", *Proc. Manchester Lit. Phil. Soc.*, 2 (1860–2), 73–97. Read 16 April 1861.
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61.5 "Hints on the theory of groups", *Messenger of Mathematics*, 1 (1862), 58–68, 187–192. Dated 25 Sept. 1861.
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68.5 "Note on the correction of an algebraic solution", *Proc. Manchester Lit. Phil. Soc.*, 7 (1868), 221–223. Received 8 June 1868. Postscript 7 Sept. 1868.
68.6 "Note on the resolution of algebraic equations", *Phil. Mag.* (4), 36 (1868), 264–267.
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