John Michael Hammersley

JOHN MICHAEL HAMMERSLEY
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John Hammersley was a pioneer amongst mathematicians who defied classification as pure or applied; when introduced to guests at Trinity College, Oxford, he would say he did "difficult sums". He believed passionately in the importance of mathematics with strong links to real-life situations, and in a system of mathematical education in which the solution of problems takes precedence over the generation of theory. He will be remembered for his work on percolation theory, subadditive stochastic processes, self-avoiding walks, and Monte Carlo methods, and, by those who knew him, for his intellectual integrity and his ability to inspire and to challenge. Quite apart from his extensive research achievements, for which he earned a reputation as an outstanding problem-solver, he was a leader in the movement of the 1950s and 1960s to re-think the content of school mathematics syllabuses.

FAMILY BACKGROUND

John Hammersley was born to a couple with strong international connections. His mother, Marguerite (née Whitehead), was born on 29 June 1889 in Moscow, where her father Thomas was engaged in the export and sale of cotton-spinning and other textile machinery from Lancashire. At the age of 14, she was sent to boarding school in England, thus escaping the difficulties and deprivations faced by her brothers, and documented in [P2], as a consequence of the Revolution of 1917, when the Bolsheviks declared all foreign assets to be owned by the Russian people. Their property was seized, and their families retreated to London via Murmansk in 1918. Early on 1 January 1920, John's uncle George was hauled out of bed by the secret police (the Cheka) and interrogated over a period of three weeks in the Lubianka, sleeping on a bare concrete floor at sub-zero temperatures. George's brother, Alfred, managed to extricate George from the labour camp to which he had been moved, on the grounds that he was about to die. He survived, however, and he and Alfred caught a train that same day to the Finnish border.

John's father, Guy Hugh, was born on 5 March 1883, the second son of a fashionable London gynaecologist who, when Guy was 14, collapsed and died in the prime of life, leaving his family in straitened circumstances. Guy had to leave school, and he took a job as an office boy at the London office of the United States Steel Corporation. By the time of John's birth in 1920, Guy had worked his way up to be in charge of the branch office in Glasgow. There were ups and downs in his career, occasioned by times of retrenchment and recession in the United States. Guy and Marguerite moved back to England, and he was made redundant around 1925. He found work as the London manager for the Youngstown Steel and Tube Company, and later as

European manager for the Bethlehem Steel Company following the Depression in the USA.

Marguerite and Guy were married in 1914, and their only son to survive childbirth, John Michael, was born on 21 March 1920.

EDUCATION

The following extracts from some autobiographical notes present an interesting account of John's life pre-Oxford, as well as insight into his character.

I attended a kindergarten called the Waterside School in Bishops Stortford from 1925 to 1929. It was run by a headmistress, Miss Blandford, and it gave me an excellent start in reading and writing and arithmetic. In my last year, her father, Mr Blandford, gave me an introduction to Latin and algebra.

In 1929 I was sent as a boarder to Bembridge School on the Isle of Wight. This was a school with progressive ideas about teaching arts and crafts and carpentry but little emphasis on anything academic: after a couple of terms at Bembridge, my parents were dissatisfied with what I was being taught and I was sent instead to a more conventional preparatory school, Stratton Park near Bletchley, where I remained from 1930 to 1934.

The man who taught mathematics at Stratton Park, Mr Pilliner, almost put me off the subject by asking me how many blue beans made five. When I failed to answer the conundrum, he said the answer was 5 *and I was a fool: but I had already dismissed this as too obvious to be correct (and in retrospect, the correct answer is probably something like* $5[\text{blue beans}]^{-1}$*). However, my mathematical fortunes were saved shortly after this incident by the arrival at Stratton Park of another teacher of mathematics, Gerald Meister. He had been a housemaster at Sedbergh School, where there was a convention that housemasterships could only persist for 15 years. When his 15-year stint was complete, he decided to try his hand at preparatory school teaching and took up residence at Stratton Park and remained there for a couple of years, after which he taught at Wellington College and next at the Dragon School in Oxford.*

During his time at Stratton Park he gave me a solid education in mathematics and a liking for the subject. This covered plenty of Euclidean geometry (including such topics as the nine-point circle) and algebra (Newton's identities for roots of polynomials) and trigonometry (identities governing angles of a triangle, circumcircle, incircle, etc), but no calculus. Due to his help, I got a scholarship to Sedbergh.

I was at Sedbergh School from 1934 to 1939. There it was traditional in those days for the brighter boys to be shoved on the classical side, and in my first year I was put in the Classical Fifth form, where I completed the School Certificate in classics (the equivalent of four O-levels today) and then at the end of my first year into the Lower Sixth Classical. However, Latin and Greek did not interest me, and after one term in the Lower Sixth Classical I was allowed to migrate to the Upper Sixth Modern to learn some science. I had some excellent teaching in physics from Len Taylor, and in chemistry from Charles [sic] *Mawby*[1]*. My mathematics master was Sydney Adams (subsequently headmaster of Bancrofts School). His knowledge of mathematics was*

[1] N. James Mawby

very sound, but did not extend much beyond what was appropriate to schoolteaching: I recall being puzzled that a continuous function might be non-differentiable everywhere; and although he was able to confirm this, he could not exhibit a specific example for me. I passed Higher Certificate (the equivalent of A-level today) in mathematics, physics, and chemistry in the summer of 1937, but I did not gain a distinction in mathematics. I sat the scholarship examination for Emmanuel College, Cambridge, in December 1937, and also for New College, Oxford, in March 1938, without success in both cases. However, I was awarded a Minor Scholarship to Emmanuel College at a second attempt in December 1938.

CAMBRIDGE

I went to Cambridge as an undergraduate in 1939. The war had just started, and many undergraduates including myself presented ourselves to enlist at the Senate House which served as a recruiting station in Cambridge. At least as far as this recruiting station was concerned, there was not much evidence at that time of making wartime use of people with potential scientific qualification. After a brief medical check-up, I found myself in front of a trestle table opposite a don, disguised in the uniform of a sergeant, and the following conversation ensued.

Sergeant: *Do you want to join the navy, the army, or the air force?*

Me: *I suppose it should be the army — I was in the OTC at school.*

Sergeant: *Which regiment do you have in mind?*

Me: *I have no idea. I have just started to read mathematics here in Cambridge: is there any use for mathematics in the army?*

Sergeant: *No, there is no use for mathematics in this war and in any case you are only an undergraduate. The services have taken just three professional mathematicians from Cambridge, one for the navy to tell them about underwater explosions, one for the air force to explain stellar navigation, and I was the third. My mathematical job is to add up the daily totals of recruits for the navy, the army and the air force respectively.*

I wonder who the 'sergeant' was, maybe a number theorist. Of course, he was wrong[2] *about the wartime uses of science, including mathematics, and about the number of scientists and mathematicians recruited from Cambridge, but I did not know about that until much later. In the meantime, waiting until I was eventually called up, I hung around in Cambridge pretty idly. I remember tutorials from Stoneley, who taught me how to express* $\nabla^2\phi$ *in spherical polar coordinates but not much else; and also tutorials from P. W. Woods, whose favourite subject was the twisted cubic. Pupils would strive to keep him off the twisted cubic for as long as possible by asking him questions on other bits of pure mathematics, but once he was locked on the twisted cubic after the first ten minutes of a tutorial, the rest of the tutorial was a foregone*

[2] He was more or less in agreement with G. H. Hardy, [H2], who felt it plain that "the real mathematics has no *direct* utility in war", but, when asking "does mathematics "do good" in war?", found it probable that technical skill keeps young mathematicians from the front, thereby saving their lives.

conclusion. I was lucky to get a Third Class[3] *in the preliminary examinations in mathematics in the Easter Term in 1940, before being called up for military service in the Royal Artillery.*

WARTIME SERVICE

Despite the assertion by the recruiting 'sergeant' in the Senate House in Cambridge that mathematics was of no military interest in wartime, I did later find uses for it when serving in the Royal Artillery in connection with anti-aircraft gunnery. An aircraft is a high-speed moving target, whose flight path is detected and followed by radar. To hit a target one needs to predict how far the aircraft will have moved in the lapse of time between the gun being fired and the shell reaching it. This calculation was performed by a piece of computing hardware called a predictor. There were two sorts of staff officers who were expected to have an enhanced technical knowledge of anti-aircraft equipment: they were respectively called Instructors in Gunnery (I.G.s) and Instructors in Fire Control (I.F.C.s) Both the I.G. and the I.F.C. had technical expertise in the three components (radar, predictor, gun) of this linkage; but their particular provinces overlapped in the sense that the I.G.s specialized in the gun–predictor pair, while the I.F.C.s specialized in the radar–predictor pair. The School of Anti-Aircraft Artillery (S.A.A.A.) was situated on the Pembrokeshire coast at Manorbier; and the Trials Wing of the S.A.A.A. was at Lydstep about a mile to the east of Manorbier. The function of the Trials Wing was to carry out research on the performance of various pieces of anti-aircraft equipment, both existing equipment and equipment proposed for future use, and to report thereon to the war office and Ministry of Supply. At the Trials Wing there were three I.G.s and two I.F.C.s; and in 1942 I became one of the I.F.C.s, remaining there until the end of the war.

Before that however I was called up for military service in the late summer of 1940, first as a gunner and next as a lance-bombardier at a training camp at Arborfield until being sent to an officer training cadet unit at Shrivenham. I was commissioned as a second lieutenant in the spring of 1941 and posted to an anti-aircraft gun site defending an armament factory near Worsham. At Shrivenham I had been told about the existence of radar; and the Worsham gun site had an early piece of radar equipment which operated with a wavelength of a few metres. Its performance in measuring the distance to a target was reasonable; but its accuracy in measuring the direction to the target was pretty indifferent, relying on interference effects between various dipole aerials receiving signals both directly and also reflected from a large horizontal mat of wire mesh. At any rate it represented the current state of the art at that time; and it interested me considerably. Wanting to learn more about the potentialities of radar, I took the rather unusual step of telephoning divisional headquarters and as a result was selected to train to become an I.F.C.

This training began with a six weeks course on basic wireless technology at the Regent Street Polytechnic, followed by a longer and more specialized course on radar at Watchet in Somerset. At Watchet they had a radar with a ten centimeter wavelength,

[3]Of 33 candidates for the Mathematics Preliminary Examinations in 1940, 11 were placed in the First Class, 15 in the Second, and 7 in the Third.

which at that time had not come into general service for anti-aircraft gunnery. There I learnt about the properties of magnetrons and wave guides. On passing out of Watchet as a qualified I.F.C., which carried the automatic rank of captain, I was posted first to an establishment at Oswestry which trained operators of radar equipment, and next to anti-aircraft brigade headquarters in the Orkneys where I was responsible for the radar installations of the gun sites defending Scapa Flow. Finally in 1942 I was transferred to the Trials Wing at Lydstep.

Amongst the personnel at Trials Wing there was a team of about 40 girls who carried out the computations necessary for analyzing the performance of the anti-aircraft equipment, and I was responsible for directing their calculations. One of their jobs consisted in operating the kinetheodolites for tracking a target. The kinetheodolites were a pair of synchronized telescopic cameras at each end of a base line about a couple of miles long, which could give simultaneous readings of the respective angles to a target (either an aircraft or a radar sleeve towed behind an aircraft). From the resulting data it was possible to compute fairly accurate positions of the target and how these positions depended upon time as the target moved along its flight path. In practice it was just an ugly piece of three-dimensional trigonometry; and when I first arrived at Lydstep it was done with pencil and paper with the aid of a 7-figure tables of trigonometric functions, in accordance with traditions of military surveyors. But while surveyors may conceivably be interested in determining a position to the nearest fraction of an inch, it was nonsense to do so for an aircraft target in view of the more dominant errors inherent in gunnery. One of my first reforms was simply to introduce 4-figure trigonometric tables, and to equip the computing room with desk calculating machines in place of longhand pencil and paper sums. The calculating machines were winkled out of the Treasury, who were keeping them massed in a big cupboard in case they might be of future service for financial purposes.

There were certain bits of mathematics, of which I had no previous knowledge; in particular I needed to learn about numerical methods and statistics. I taught myself from Whittaker and Robinson's book about subjects such as finite differences and interpolation. To describe the trajectory of a shell, given the angle of elevation of the gun firing it, range tables of the sum were available in terms of the Cartesian coordinates of the shell at successive widely spaced intervals along its trajectory. It had not occurred to the compilers of the range tables that it would be more natural to represent this data in terms of polar coordinates; and, even when this was done there remained the non-trivial task of two-dimensional interpolation of this data. There is a result, due to Kolmogorov, that a continuous function of d independent variables can be expressed in terms of a polynomial in $2d + 1$ *functions each of a single variable; but I did not know of this result until well after the war was over. Nevertheless I discovered for myself shortly after arriving at Lydstep that this result was explicitly true in the particular case* $d = 2$ *at least for the polar coordinate versions of* 3.7" *and* 4.5" *anti-aircraft guns. Accordingly we recalculated the range tables of these guns in terms of quadrant elevations and tangent elevations; and were then able to complete the predicted trajectory using* 1*-dimensional interpolations.*

Acquaintance with statistical techniques was the other main gap in my previous mathematical education; and to cover this I obtained leave of absence to return to

Cambridge for a few weeks. The first volume of M. G. Kendall's book on mathematical statistics had just been published. I also read R. A. Fisher's book on statistical methods for research workers. Statistical techniques played an important role at Lydstep in assuring the performance of anti-aircraft radars and predictors, and in liaising with radar developments from the Radar Research Establishment at Malvern.

By the end of the war I had been promoted to the rank of major, and appointed a consultant to the Ordnance Board in London. Anti-aircraft gunfire, which had been pretty inaccurate at the beginning of the war, had gradually improved by the end of the war; in particular the V1 bomb was comparatively easy to shoot down because of the introduction of the proximity fuse in shells. Against this, the V2 bomb was a ballistic missile and so unassailable. In the near future hostilities with nuclear weapons would render discussions with the Ordnance Board about the air defence of London nugatory. Effectively, the chapter on anti-aircraft gunnery was closed.

POSTWAR ACTIVITIES

In 1946 I returned to Cambridge as an undergraduate at Emmanuel College. From time to time there were occasional trips up to London to fulfill my duties at the Ordnance Board, but these had little relevance to the future of anti-aircraft gunnery. Before the war I had done a certain amount of skiing; and I hoped for a half blue for skiing. One of the difficulties was that foreign currency was rationed by the Treasury; and so I needed to earn some Swiss francs by giving some lectures on statistics at any Swiss university that could be persuaded to employ me. Thanks to references provided by Harold Jeffreys, the Federal Institute of Technology (E.T.H.) in Zurich was kind enough to provide the necessary funds. However in those days the university skiing team consisted of four members, and I was ranked fifth in the trials; so I never got a half blue, although I did take part in a joint Oxford–Cambridge match against the combined Swiss universities which was a twelve-a-side match. Needless to say, the combined Swiss universities beat the joint Oxford–Cambridge team.

As a Cambridge undergraduate in the two years after the war I was much more motivated than I had been in 1939/40; and I also had the good fortune to be tutored by better tutors, in particular A. J. Ward and J. A. Todd for pure mathematics and R. A. Lyttleton for applied mathematics. In 1948 I got a first class (Wrangler) in Part 2 of the Mathematical Tripos.

In 1948 I thought I would like to try my hand at an academic job in mathematics or mathematical statistics. There was no opening for me at Cambridge then. I applied for vacant lectureships at Reading University and at St Andrews University, but my applications were not successful. However I did get an appointment as a graduate assistant at Oxford in the Lectureship in the Design and Analysis of Scientific Experiment.

This Lectureship was a small department headed by the lecturer (D. J. Finney) and having two graduate assistants (M. Sampford and myself) together with a secretary and a couple of girls with desk calculators. At that time it was the only established provider of statistical services at Oxford, and its remit was spread quite generally over any and all queries that might be thrown up in various branches of service. It also had to offer lectures and instructions on statistics; for example, it fell to me to give *the lecture course in the Department of Forestry for overseas forest officers on the collection and analysis of data on trees and their growth.*

OXFORD

Hammersley held the position of Graduate Assistant, Design and Analysis of Experiments, at Oxford University, until he moved in 1955 to AERE Harwell as Principal Scientific Officer. He returned to Oxford in 1959 as Senior Research Officer at the Institute of Economics and Statistics. This was a position of roughly the same level as a University Lecturer but with neither formal teaching duties nor a linked College Fellowship/Tutorship.

It was during this period that he began an association with Trinity College which was to last for the rest of his life. When P. A. P. Moran left Oxford for the Australian National University at the end of 1951, Hammersley took over his tutorial duties at Trinity as Lecturer in Mathematics. It was not until his election to a Senior Research Fellowship in 1961 that he became a fellow of the college. In 1969 he was promoted to (University) Reader in Mathematical Statistics, and was elected to a Professorial Fellowship at Trinity, two positions that he retained until his retirement in 1987. It is sometimes said that Hammersley was only the second mathematics fellow at Trinity since its foundation in 1555, following in the footsteps of Thomas Allen (elected in 1564). He was in fact arguably the first such fellow. In the late 16th century all Trinity fellows were required to take the oath of supremacy, an obligation that Allen avoided by departing the College in 1571. It was during that period and later that Allen's mathematical activities developed, although, unlike Hammersley, he is said to have written "little and published nothing" (see [A1]).

Despite the fact that Hammersley held no official position at the University between 1955 and 1959, he took on his first four Oxford DPhil students in October 1956. He retained an office in Keble Road, and he seems to have spent a lot of his time there. From 1959 until his retirement in 1987, he worked in what appeared to be splendid isolation in his office in the Institute of Economics and Statistics in St Cross Road. As far as one could judge, apart from seeing graduate students and teaching a few Trinity undergraduates, he had his time free for research.

It was over Sunday lunch in Oxford shortly after his arrival that he met Gwen Bakewell, who became his wife in 1951. Their first home in Longwall Street was soon replaced by Willow Cottage on the Eynsham Road, where their sons Julian and Hugo were born.

Although his university position was not in mathematics, he was a member of the subfaculty, and he lectured and examined under its auspices. He gained a certain notoriety for his high expectations of undergraduates. For example, one year he offered a non-examinable lecture course on 'Solving Problems' in which few students lasted very long. As a Finals Examiner in 1966, he set (or was at least blamed for) what was the most difficult set of compulsory papers in living memory. 1966 became known as the 'year of the carrot' in honour of one question on differential equations that opened with the phrase: "If a sliced carrot is immersed at time $t = 0$ in β-indolyl acetic acid ..."

Basic mathematical techniques mattered a lot more to Hammersley than many an advanced result. On one occasion in an examiners' meeting, he would not withdraw from the position that a relatively large number of marks, in an advanced probability question, be given for the correct use of partial fractions.

It was not always easy for students and colleagues to rise to the uncompromisingly high intellectual standards set by John Hammersley, but it was a level playing field, and he applied his standards to himself just as to others. To the knowledge of the current authors, he took on only eight doctoral students during his career, and at least five of these continued to successful scientific careers. Students were required to show their worth, as explained by John Halton:

A cousin drew my attention to an advertisement in the Observer . . . , seeking applicants for UK Atomic Energy Authority Research Studentships, to study Monte Carlo methods for a DPhil at Oxford. . . . In a few weeks, I was invited to "present myself for examination" at the UKAEA site at Didcot. With very little idea of what this would entail, I went. There I found a [number of] equally bemused applicants, who were ushered into a large hall furnished with a suitable number of small desks and sat down. John Hammersley strode breezily up to the podium, introduced himself, and asked us to write a four-hour examination, consisting of a dozen or so tough mathematical questions. I attempted to solve each problem in turn, suggested possible lines of approach, and tried to answer the questions posed, with little success. At the end of four hours, the papers were collected and we waited anxiously for the outcome.

Peter Marcer has taken up the story: *What a sleepless night I (and I expect others) had before the interviews the next day, when each of us asked members of the panel, which included John and Professor Flowers as he was then, what the answers were and how one did the questions. Only to be told that John had done the rounds of the theoretical physics department at Harwell, and compiled the examination out of the questions that the members of that department were in the course of trying to answer! That is, there were no answers to these questions as yet, and the panel just wanted to see how we, the candidates, might begin to tackle them! I think that episode sums up John for me, a great mind sometimes puckishly inclined but with great purpose, and above all a great gentleman of the old school. He was a delight to know, and will be sorely missed, and I owe him a great deal.*

As a result of this exercise, Halton, Marcer, David Handscomb, and Jillian Beardwood were awarded studentships under Hammersley's supervision. As 'Monte Carlo' students, they were privileged with access to the Ferranti Mercury computers at both Oxford and Harwell, as well as to the Illiac II while visiting the University of Illinois at Urbana in 1958.

Hammersley was for a period equally at home in California and Oxford. He was a regular contributor to the Berkeley Symposia on Mathematical Statistics and Probability, and was a close friend of the distinguished statistician Jerzy Neyman. He spent the Michaelmas terms of 1958 and 1961 at Urbana, Illinois and Bell Telephone Laboratories, Murray Hill respectively. On both these trips he was accompanied by his graduate students.

He never studied for a PhD, perhaps because of his age following war service, but he was awarded an ScD by Cambridge University in 1959, followed in the same year by an Oxford DSc (by incorporation). He was awarded the Von Neumann Medal for Applied Mathematics by the University of Brussels (1966), the Gold Medal of the Institute of Mathematics and its Applications (1984), and the Pólya Prize of the London Mathematical Society (1997). He was elected to the Royal Society in 1976. He gave the 1980 Rouse Ball lecture at Cambridge University, and published an account in [141].

On retiring from his Oxford Readership in 1987, he was welcomed at the Oxford Centre for Industrial and Applied Mathematics (OCIAM). He reciprocated this act of hospitality by making his extensive mathematical experience available to all who asked.

Many of Hammersley's friends and colleagues gathered in 1990 at the Oxford Mathematical Institute for a conference to recognise his 70th birthday. A volume [G5] entitled 'Disorder in Physical Systems' was published in his honour, with contributions from many whose work had been touched by his ideas. Hammersley gave the closing lecture of the meeting under the title 'Is algebra rubbish?', but he uncharacteristically refrained on this occasion from answering the question.

In more recent years he was to be found at Willow Cottage, reading, doing the crossword, and working on Eden clusters. He died on 2 May 2004 after an illness.

JOHN HAMMERSLEY, MATHEMATICIAN

John Hammersley was an exceptionally inventive mathematician and a remarkable and fearless problem-solver. He had the rare ability to pinpoint the basic mathematics underlying a scientific problem, and to develop a useful theory. He preferred what he called "implicated" mathematics over "contemplative" mathematics; that is, he found the solution of problems to be superior to the "high-rise mathematics" of which he could be sharply critical (see [126, 131]).

The conventional modern classification of mathematics into pure, applied, and statistics can accentuate gaps between these areas, gaps that need to be filled. Hammersley spurned such an attitude; when facing a practical problem, he used whatever he could find in order to solve it. This 'bare hands' approach does not always lead to the neatest solution, although, in Hammersley's case, much of the resulting mathematics has stood the test of time. Several of the problems that he formulated and partly solved have emerged since as landmark problems of combinatorics and probability. For example, his work on self-avoiding walks and percolation is fundamental to the theory of stochastic Löwner evolutions that is now causing a re-think of the relationship between probability and conformal field theory; his results on the Ulam problem underly the proof [B1] that the relevant weak limit is the Tracy–Widom distribution. These two general areas are amongst the liveliest of contemporary mathematics, as witnessed by the award of Fields Medals in 2006 to Werner and Okounkov.

Paper [29], written jointly with K. W. (Bill) Morton, is a landmark of his earlier work in two regards. Firstly, it marks a beginning of Hammersley's extensive study of discrete problems in probability and statistical mechanics. Secondly, the paper contains two problems and a technique that have attracted a great deal of attention in the 50 years since. Despite the title of the paper, 'Poor man's Monte Carlo', the lasting contributions are the clear statement of the problem of counting self-avoiding walks, the use of subadditivity to prove the existence of the connective constant, and the discussion of random media that culminated in Simon Broadbent's formulation of the percolation model.

These and other topics are discussed further in the following paragraphs, complemented by summaries of how John's work has stimulated the relevant fields since.

Computing/Calculating/Estimating

Hammersley's early scientific work was based on the mathematics he had been doing during the war. His first publication [1] arose from independent contributions by Majors Bayley and Hammersley to the discussion following the reading of a paper on random processes by Maurice Bartlett [B2] at a symposium on Autocorrelation in Time Series held in 1946 at the Royal Statistical Society. The problem confronting Bayley and Hammersley arose in trials of anti-aircraft equipment. The details were embodied in "reports not generally available" but [1] contains in condensed form some of the results obtained.

There followed a sequence of papers on essentially unrelated problems, many concerned with hard calculations or estimation. Probably his first significant work was his paper [11] on the estimation of parameters when the parameter space is a discrete set of points. He showed, for example, that, if the unknown mean of a normal population with given variance is assumed integer-valued, then its maximum likelihood estimator is the integer nearest to the sample mean. His interest in issues of this kind arose from a problem of estimating the molecular weight of insulin, and this may have come to his attention during his work as a consultant on statistical problems to members of the university in the natural sciences.

It was a mathematical problem arising in [11] that led to his paper [14] on asymptotic formulae for the sums of products of the natural numbers. Paper [14], read in isolation, may appear to be scantily motivated. However, it does display Hammersley's formidable analytic skills, and it attracted the attention of Paul Erdős who, in [E2], settled one of the open problems posed. It is now clear that, in [14], he was in fact calculating what Cramer [C2] described recently as "remarkable expressions" for the mode of Stirling numbers of the first kind.

Throughout the rest of his scientific career, John Hammersley continued this interest in computing methods and computer science—principally through his work on large-scale simulations (see below).

Applied Probability

In the period between leaving the military and starting his collaboration with Morton, Hammersley appears to have tried his luck at a range of problems in applied probability, hard analysis and large-scale computations. For example, in [9], he considers a problem arising in the design of experiments that may be expressed as follows: given a collection of k counterfeit and $n-k$ genuine coins, how may we detect the counterfeit coins? His interest in stochastic geometry was developed in [10], where he studied the distribution of the distance between two points independently and uniformly distributed over the solid n-sphere. In [15], he proved a special case of a conjecture of Fejes Tóth about the sum of the side-lengths of a convex polyhedron containing a sphere of unit diameter. His paper [27] on Markovian walks on crystal lattices originated from a study of diffusion of electrons in crystals such as the hexagonal close-packed lattice.

Around 1953, he considered a problem on counting blood cells that had arisen at the Clinical Pathology Department of the Radcliffe Infirmary at Oxford. The mathematical problem here turns out to be equivalent to finding the probability distribution of the number of gaps between intervals of random length placed randomly on a circle. Hammersley showed (by typically hard analysis) that it was asymptotically normal. Cyril Domb has given an account in [D] of the history and ramifications of this particular problem, and this work illustrates Hammersley's gift for picking out hard, genuinely interesting problems from the applied sciences and translating them into valid mathematics.

In [40], he extended a classical result of Mark Kac [K1] concerning the number of zeros of a polynomial with random coefficients. Kac's results were for the mean number of real zeros when the coefficients are independent, identically, and normally distributed, and Hammersley gave a substantial, albeit complicated, generalisation. For recent activity in this field, see [F2, F4, R1].

Hammersley's most influential work in applied probability is that on percolation and on the large-scale geometrical properties of n points dropped at random into a bounded region of Euclidean space. We return to these two areas in later paragraphs.

Having sketched Hammersley's early work, we move to his work post-retirement, almost all of which was concerned with the growth of crystals. He worked with Mazzarino on a third-order differential equation arising as a model for the growth of a crystal in a supercooled liquid, see [116, 117]. This 'classical' work was followed by his final two research papers directed at the stochastic model introduced in 1961 by Murray Eden [E1] for growth in biological cells. Despite its apparent simplicity, the Eden model has attracted a great deal of interest over the years.

In the simplest version, the 'cells' are taken to be closed unit squares of the 2-dimensional square lattice. All cells but one are coloured white initially, and subsequently cells are blackened one at a time. The mechanism of growth is as follows. An edge of the lattice is called *active* if it separates a black cell from a white cell. At stage n, an active edge is picked at random, and the associated white cell is coloured black. At time n, there is a cluster C_n containing $n + 1$ black cells. The shapes of the C_n have the same distribution as those of the first-passage percolation model discussed below, when the edge-passage-times of that model are exponentially distributed.

Natural questions of interest about this process are: (i) what is the 'shape' of C_n for large n, and (ii) how large do the 'lakes' of enclosed white cells grow before they are eventually filled in by black cells and disappear? In [120], Hammersley presented non-rigorous arguments suggesting that all lakes in the 'island' C_n lie with high probability within a distance $\mathrm{O}(\log n)$ of the coastline.

In his penultimate research paper [119], Hammersley (with Mazzarino) carried out a large-scale Monte Carlo simulation in which clusters of size of order 10^9 are grown, and various quantities such as the mean cluster-radius are estimated. The authors evinced pride in being able to carry out this huge computational task using only 24 megabytes of a Convex 220 machine, in contrast to comparable simulations of

Zabolitsky and Stauffer [Z] using a Cray 2 with 4 parallel processors and a vast (for the period) store of 2045 megabytes.

A subject of primary interest in these two papers is the 'surface roughness' of a typical cluster. The theoretical analysis carried out in [120] makes use of the theory of *harnesses*, as introduced by Hammersley in 1967. Harnesses may be described loosely as a spatial generalisation of a martingale; they appear to have received very little attention since 1967, although Hammersley's original paper [81] was one of the 45 articles selected and reprinted in [F1] as one of the seminal contributions to the scaling laws that characterise rough surfaces generated stochastically.

Monte Carlo Methods

From the very beginning of his career, John Hammersley sought methods to carry out large computations. The equipment then available was limited and unreliable and, rather as in his army days, he became a master of desk calculators and early computers. He considered it a virtue to use computing resources in an economic and efficient manner, and this attitude remained with him all his life. He once boasted of holding the 1961 world record for keeping a computer (at Bell Labs) working without breakdown for 39 hours.

Credit for the name and the first systematic development of Monte Carlo methods is usually accorded to Fermi, Metropolis, von Neumann, and Ulam. This area fascinated Hammersley. The idea is that one may estimate a quantity through computations involving random numbers. A principal objective is to reduce the degree of variation in the estimate, thereby improving the accuracy of the result.

Hammersley's interest in Monte Carlo methods seems to have been sparked by his attendance at a symposium in Berkeley in the early 1950s, and he gave a Master's level lecture course on the subject on his return to Oxford. In the audience was Bill Morton who had just graduated (in 1952) from Oxford and held an appointment at AERE Harwell. It was around this time that Hammersley organised the workshop on Monte Carlo methods at Harwell during which he met Simon Broadbent.

It was with Morton that Hammersley wrote his paper [29] entitled 'Poor man's Monte Carlo', of which the basic thesis was that one does not necessarily need large high-speed machines to use Monte Carlo effectively. In order to illustrate this main point, the authors draw on a range of examples such as self-avoiding walks. Among the more diverting of the examples is the testing of a quantum hypothesis of Alexander Thom. Thom had measured the diameters of 33 Druid circles in Western Scotland, and, based on the (integer) data, he conjectured that these diameters were intended to be multiples of 11.1 feet. The evidence for this was that 27 of the circles had diameters lying in the range $11.1(n \pm \frac{1}{4})$ for integral n. Hammersley and Morton used simple Monte Carlo methods to test the hypothesis and, as David Kendall suggested in [K4], their work led to a statistical examination which went a long way towards confirming this proposal.

Monte Carlo methods are based upon the use of pseudo/quasi-random numbers, and this raises certain issues of principle. Hammersley's impatience with philosophical discussions involving the ethics or correctness of using pseudo/quasi-random numbers in place of truly random ones is captured in his reply to the discussions at the Symposium on Monte Carlo Methods at which [29] was presented: "The discussion has raised several questions about random numbers: do they even exist; can they be produced to order and if so how; can they be recognised and can we test that they are not imposters? These are diverting philosophic speculations; but the applied mathematician must regard them as beside the point."

Indeed, his intolerance of philosophy as an academic subject seemed to stay with him throughout his life. The Oxford joint school of Mathematics & Philosophy was one of his *bêtes noires*, and various amusing stories have accumulated about the year where he ended up (by default) as Chairman of the Examiners. When the opportunity came for him to chair the Finals examining board, he grasped it enthusiastically, and taped (with his colleagues' permission) a post-meeting discussion on the value of the degree. His further strenuous efforts could not in the end persuade either the mathematicians or the philosophers that the degree should be shelved.

Hammersley's most significant contribution to the theory, as against practice, of Monte Carlo methods is probably his work on antithetic variates. This is a technique for yielding estimates with variances considerably less than those obtainable by a naive approach. This is typically achieved by representing the estimator as a sum of correlated random variables, and it is one of the most popular variance-reduction techniques. Its drawback is that many antithetic sampling plans are too computationally complex to be of practical use in simulations. Despite this, the work of Hammersley and Morton [38] is currently regarded as a major contribution. (See, for example, [R2].) It is interesting, therefore, that in [74] Hammersley and Handscomb claim only the name, not the original idea which, as pointed out by Tukey [T3], can be regarded as an important special case of regression. This technique is now, perhaps, one of the most important in the application of Monte Carlo methods to high-dimensional numerical integration, with applications in many areas including mathematical finance.

The Hammersley–Handscomb monograph [74], published in 1964, is a landmark in the study of Monte Carlo methods and is still much used today. Hammersley's interest in the field seems to have declined following its publication.

Percolation

Percolation was born as a mathematical object out of the musings on random media found in [29], and it has emerged as a cornerstone of stochastic geometry and statistical mechanics. One of the discussants of [29], Simon Broadbent, worked at the British Coal Utilization Association, where he was involved in the design of gas masks for coal miners (see [G3, 110]). Hammersley recognised the potential of Broadbent's proposal for flow through a random medium, and they collaborated on the seminal paper [41], where the critical percolation probability was defined. There are earlier references to processes equivalent to percolation, see [W] for example, but it was Hammersley who initiated a coherent mathematical theory.

The basic model is as follows. Consider a crystalline lattice. We declare each edge of the lattice (independently) to be *open* (to the passage of fluid) with probability p, and otherwise *closed*. Fluid is supplied at the origin of the lattice and allowed to flow along the open edges only. The fundamental question is to describe the size and geometry of the set C of vertices reached by the fluid. The significance of this model is far-reaching in stochastic geometry and statistical mechanics, and the associated mathematics and physics literature is now very extensive indeed. Of primary importance is the existence of a phase transition: there exists a critical value p_c such that C is finite when $p < p_c$, and C is infinite with a strictly positive probability when $p > p_c$. The non-triviality of the phase transition was proved by Hammersley, as follows. Hammersley and Broadbent [41] established a lower bound for p_c in terms of counts of self-avoiding walks and the connective constant. (An account of the connective constant may be found in the next section.) This result was strengthened in [43], where it was shown that $|C|$ has an exponentially decaying tail whenever it has finite expectation. The method developed in [43] is a precursor of a now standard argument attributed to Simon and Lieb [L1, S3] and usually expressed as: finite susceptibility implies exponentially decaying correlations. In [46], he proved an upper bound for p_c in terms of the boundary sizes of neighbourhoods of the origin, and he deduced by graphical duality that $p_c < 1$ for oriented and unoriented percolation on the square grid; this is the percolation equivalent of the Peierls argument for the Ising model, [P1]. This general route to showing the existence of a phase transition is now standard for many models.

In an alternative model, it is the vertices rather than the edges of the crystal lattice that are declared open/closed. Hammersley [54] proved the useful fact that C tends to be smaller for the 'site' model than for the 'bond' model, thereby extending a result of Michael Fisher. The best modern result of this type is by one of his students, see [G4].

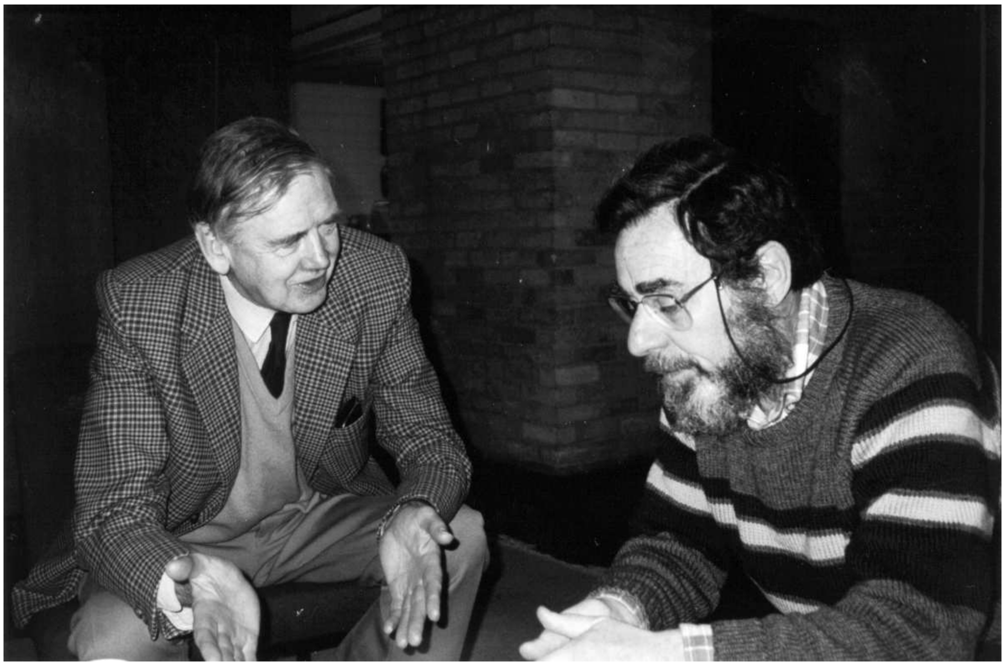
An inveterate calculator, Hammersley wanted to calculate or estimate the numerical value of p_c for the square grid. Theodore Harris proved in a remarkable paper [H3] that $p_c \geq \frac{1}{2}$, and Hammersley's numerical estimates indicated $p_c < \frac{1}{2}$; "what better evidence could exist for $p_c = \frac{1}{2}$", he would ask. He was therefore thrilled when Harry Kesten, [K8], proved the holy grail. This was however only the end of the beginning for percolation.

Percolation Theory has gone from strength to strength in recent years. The main questions are largely solved (see [G2]), and current attention is focussed on the nature of the phase transition in two dimensions. Schramm [S1] predicted that the scaling limit of the perimeters of large critical percolation clusters constitute a stochastic Löwner evolution (SLE)[4] with parameter 6. Smirnov [S4] proved Cardy's formula for crossing probabilities of critical site percolation on the triangular lattice, and indicated how to achieve the full scaling limit. See [S2] for a survey of SLE and associated problems and conjectures.

Self-Avoiding Walks and the Monomer–Dimer Problem

In the paradigm of statistical mechanics, a system is modelled by a set of configurations to each of which is allocated a weight. The sum of all weights is called the 'partition function' and the state of the system may be described via an analysis of this function and its derivatives. In a system of polymers, the first calculation is to

[4]Often termed a *Schramm–Löwner evolution.*



John Hammersley and Harry Kesten in the Mathematical Institute, Oxford University, November 1993.

find the number of such polymers. When the polymers are simple chains rooted at the origin of a lattice, this is the problem of counting self-avoiding walks (SAWs). Let s_n be the number of SAWs of length n on a given lattice. The first serious progress towards understanding the asymptotics of s_n as $n \to \infty$ was made in [29]. The key is the 'subadditive inequality' $t_{m+n} \leq t_m + t_n$ satisfied by $t_n = \log s_n$, from which the existence of the so-called connective constant $\kappa = \lim_{n\to\infty} n^{-1} \log s_n$ follows immediately. This observation, regarded now as essentially trivial in the light of the complicated analysis achieved since, has had a very substantial impact on spatial combinatorics and probability. It marked the introduction of subadditivity as a standard tool, and it initiated a detailed study, still ongoing, of the geometry of typical instances of geometrical configurations such as paths and lattice animals.

The subadditive inequality implies the bound $s_n \geq \kappa^n$. Hammersley invested a great deal of energy into trying to find a complementary upper bound on s_n, but with only partial success. With his student Welsh, he proved in [65] that $s_n \leq \kappa^n \exp(\lambda n^{1/2})$ for some $\lambda < \infty$. This was improved by Kesten [K7] for $d \geq 3$, and such bounds were the best available for some time before it was realised by others that a lace expansion could be used for sufficiently high dimensions, see [M1]. As a result of a large amount of hard work and some substantial mathematical machinery, the problem of counting SAWs was solved by Hara and Slade [H1] in five and more dimensions. The case of two dimensions, for which the bound of [65] remains the best known, has attracted much interest in recent years with the introduction by Schramm of stochastic Löwner evolutions (SLE), and the conjecture that a random SAW in two dimensions converges in an appropriate sense as $n \to \infty$ to a SLE with parameter

$\frac{8}{3}$, see [S2]. This conjecture is one of the most important currently open problems in probability.

Hammersley was happy in later life to learn of progress with percolation and self-avoiding walks. He felt that he had "helped them into existence" for others to solve. The two-dimensional percolation and SAW problems are two of the hottest problems of contemporary probability, in testament to Hammersley's excellent scientific taste.

There is a second counting problem of statistical mechanics that attracted Hammersley, namely the *monomer–dimer* problem. This classical problem in solid-state chemistry may be formulated as follows: A *brick* is a d-dimensional ($d \geq 2$) rectangular parallelepiped with sides of integer lengths and even volume. A unit cube is called a *monomer*, and a brick with volume 2 a *dimer*. The dimer problem is to determine the number $f(a_1, a_2, \ldots, a_d)$ of dimer tilings of the brick with sides of length $a_1, a_2, \ldots, a_d$. Hammersley proved in [77] that the sequence $(a_1 a_2 \cdots a_d)^{-1} \log f(a_1, a_2, \ldots, a_d)$ approaches a finite limit λ_d as the $a_i \to \infty$, but what is the numerical value of λ_d? There is a 'classical' result of statistical physics of Temperley–Fisher [T1] and Kasteleyn [K3], who showed independently in 1961 that λ_2 exists and is given by $\lambda_2 = \exp(2G/\pi) = 0.29156\ldots$ where G is Catalan's constant. Hammersley devoted much energy on theoretical and computational approaches to finding a corresponding result for $d \geq 3$ but, as far as we know, the exact value is still unknown even when $d = 3$.

In its more general form, the monomer–dimer problem amounts to the purely combinatorial question of counting the number $f_G(N_1, N_2)$ of distinct arrangements of N_1 monomers and N_2 dimers on the edges and vertices of a graph G, such that each dimer is placed on an edge, each monomer on a vertex, and each vertex of G either is occupied by exactly one monomer or is the endvertex of exactly one dimer. For this to be possible, G must have exactly $N_1 + 2N_2$ vertices, and the *density* p of the configuration is defined as the ratio $2N_2/N_1$. Hammersley proved in [77] that the number of p-density configurations on the cube of volume n in d dimensions is of order $\lambda(d, p)^n$ for some function λ. He spent much effort on obtaining bounds for λ, but, even today in 2-dimensions, our knowledge is very limited. See, for example, [F3].

The dimer problem is very much alive today. The two-dimensional model turns out to be related to the Gaussian free field and to stochastic Löwner evolutions with parameters 2, 4, and 8, see [K5, K6] for example.

First-Passage Percolation, and Subadditive Processes

Percolation is a static model in the sense that each edge is either open or closed, and water is considered to flow instantaneously along open edges. Hammersley and Welsh formulated a time-dependent version of this model in [75], and dubbed this 'first-passage percolation'. To each edge of the lattice is assigned a random passage-time, and the time $a_{x,y}$ for water to reach a given point y, having started at x, is the infimum over all paths π from x to y of the aggregate passage-time of edges in π. This pioneering paper [75] is now recognised as one of the first works of mathematical significance in the theory of the spread of material, whether it be disease, fluid, or rumour, through a random medium. The basic problem was to prove the existence of a speed function $\sigma_x = \lim_{n\to\infty} n^{-1} a_{0,nx}$, where 0 denotes the origin of the lattice.

Hammersley and Welsh realised that the key lay in the use of subadditivity, $a_{0,x+y} \leq a_{0,x} + a_{x,x+y}$, the difference with previous applications being that this inequality involves *random variables* rather than deterministic quantities.

They proved a version of the subadditive limit theorem for stationary stochastic processes indexed by d-dimensional space, the first 'subadditive ergodic theorem'. They realised that this is best done in the context of a general set of assumptions, rather than the specific situation outlined above, and thus their paper gave birth to one of the principal techniques for the analysis of spatial random processes. The search began for the 'right' combination of definition/theorem, and this was found by John Kingman [K9] in one of the classic papers of 20th century probability. Despite later elaborations, it remains fascinating to read this early literature, and especially the dialogue of [K10, 95]. Kingman's invited review article [K10] (with published discussion) appeared in the Annals of Probability. Hammersley's contribution to this discussion was too extensive to be accepted as such by the journal editor, and it appeared later as [95]. It is there that the condition of pathwise subadditivity is replaced by the weaker assumption of 'superconvolutivity' of the associated probability measures.

In an earlier application of subadditivity to spatial systems pursued jointly with his students Jillian Beardwood and John Halton, Hammersley made a fundamental contribution to the study of typical instances of problems in operations research. Drop n points at random into a plane region R of finite area. What is the length of the minimal spanning (Steiner) tree and the minimal travelling salesman path on these points? They showed in their classic paper [47] that the answer is (in essence) proportional to $c_R\sqrt{n}$ for some constant c_R, and they developed also a higher-dimensional theory. The key was to encode the problem in such a way that the natural length-scale is $\sqrt{n}$, and then to use a type of spatial subadditivity. This theorem was central to the later work of Karp on a probabilistic analysis of the random Euclidean travelling salesman problem, [K2]. Further developments are described in the Festschrift paper [S5] by Steele.

The title of [S5] makes play on Hammersley's own famous title 'A few seedlings of research', published in 1972 in the Proceedings of the Sixth Berkeley Symposium. In this inspiring account of how to do mathematical research, Hammersley showed in particular how to use subadditivity to solve (in part) the now famous Ulam problem: in a random permutation of the first n natural numbers, what is the length l_n of the longest increasing subsequence? It turned out for geometrical reasons related to [47] that the answer is asymptotically $c\sqrt{n}$. This was the starting point of a major area of probability theory. Hammersley claimed a back-of-the-envelope argument to show $c = 2$, but the formal proof eluded him and was found by Vershik and Kerov [V] and Logan and Shepp [L2] in the context of random Young tableaux. Interest turned then to the size of the deviation $l_n - 2\sqrt{n}$. Many partial results preceded the remarkable proof by Baik, Deift, and Johansson [B1] that $(l_n - 2\sqrt{n})n^{-1/6}$ converges as $n \to \infty$ to the famous Tracy–Widom distribution of random matrix theory.

Random Fields

One of the most important topics in modern statistics is the Bayesian theory of image analysis. In this study of spatial random systems, it is useful to have a classification of those probability measures that satisfy a certain 'spatial Markov property', namely: the configuration inside any region V depends on the configuration outside V only through the states of the vertices on its boundary ∂V. Some limited theory of such measures was developed by Averintsev, Dobrushin, Spitzer, and others around 1970. This was generalised to an arbitrary network by Hammersley in 1971 following a suggestion of Clifford (see [C1, 87]). The ensuing theorem, commonly termed the Hammersley–Clifford theorem, though never formally published, is much used in probability and statistics. It states that a positive measure is a Markov field if and only if it has a Gibbsian representation in terms of some potential function. The methods used by Hammersley were much clarified by later authors including another of his students, Grimmett [G1], who reduced the proof to an exercise in the inclusion–exclusion principle.

In Michaelmas Term 1971, Hammersley offered a graduate course on Markov fields at the Mathematical Institute. He promised a solution to the corresponding problem in which the assumption of positivity is relaxed. It was typical of the man that he had not yet proved the result, and indeed the 'theorem' was disproved through the discovery of a counterexample by a Rhodes Scholar, John Moussouris, in the audience. See [M2, 131].

Educational Issues

Great changes were made during John Hammersley's lifetime in the teaching of mathematics in schools, and he was for a period at the forefront of the debate. From the 1950s onwards, he argued fiercely that schoolchildren and undergraduates should be trained to solve problems, and that the curriculum should be designed accordingly. He lectured on this topic around the UK, and he contributed to the development of the School Mathematics Project (SMP). Not being a man of equivocal views, his uncompromising stance was seen by some as a provocation, but he had many supporters and admirers. However, the SMP proved no panacea for him: while it 'modernised' aspects of mathematical teaching, it introduced abstract theory without a sufficient problem element.

Hammersley frequently published his lectures in the Bulletin of the Institute of Mathematics and its Applications (IMA). His principal article [126] on mathematical education appeared thus under the title 'On the enfeeblement of mathematical skills by 'Modern Mathematics' and by similar soft intellectual trash in schools and universities'. This serious, if typically prolix, *critique* of school mathematics compelled a rebuttal from Bryan Thwaites, [T2], tempered as follows: "I have, however, a profound reluctance to [reply to Hammersley's "charges"]. The reason is that my admiration of the man and my opinion of his paper are in great conflict. Much of my admiration stems from his mathematical achievements; but it also rests firmly on my judgement that it was he, more than any other Englishman, who finally set going the long-overdue reforms in school mathematical curricula."

Through his 'popular' articles, Hammersley expressed his powerfully held views on many matters, primarily scientific and educational. These writings are erudite, provocative, and skilful with language, if sometimes self-indulgent. His thoughts on mathematical research were published alongside those of Michael Atiyah in [A2, 131], and include some notable expressions: "... perfuse his professorial piddledom", "Pure mathematics is subject to two diseases, resulting from rigour and from axiomatisation", "whatever algebra can accomplish, some other branch of mathematics ought to be able to accomplish more elegantly", "... and the production of neater solutions is merely a matter for theory builders". He loved a good phrase, even (perhaps, especially) when it risked going a bit too far. In reality, he would accept any theory that proved its worth.

As Hammersley wrote to Atiyah in [131]: "I don't quarrel, but I am prepared to enter the lists. ... it is the jostling and jousting between different sorts of mathematicians and scientists, between different temperaments and unlike tastes, that advances knowledge as a whole. So much the more fun, variety is the spice, and so on!"

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