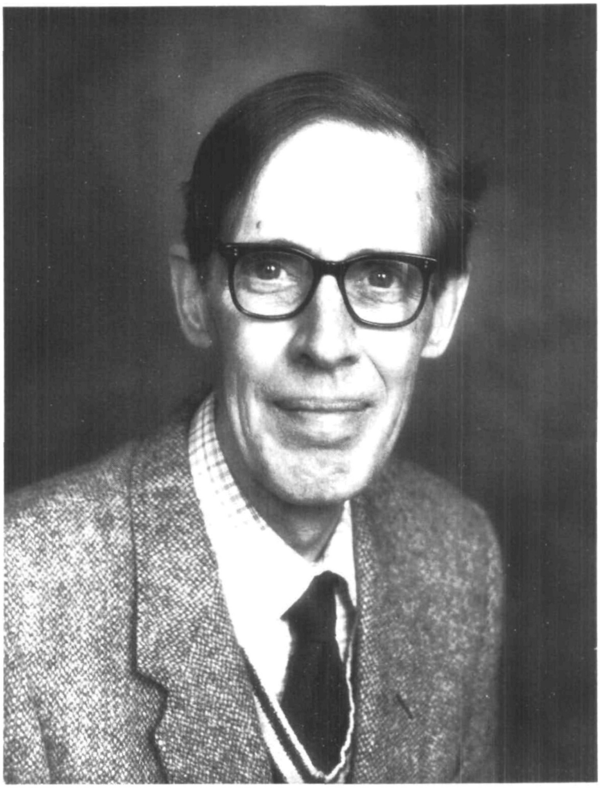

GERD EDZARD HARRY REUTER, 1921–1992

# OBITUARY

# GERD EDZARD HARRY REUTER

## 1. *Introduction* (*DGK*)

This is a cooperative work. Section 2 is by Kendall (DGK) and covers Harry Reuter's early years and the long probabilistic partnership with DGK. Section 3 is by Bingham (NHB) and covers Harry's work in pure mathematics, while Section 4 is by Sondheimer (EHS) and covers Harry's work in applied mathematics. Sections 5 and 6 are again by DGK and give details of Harry's family and academic career, and of his return to Cambridge. We wish to thank Professor J. V. Armitage and many other friends for providing us with valuable additional perspectives on Harry's life and work.

## 2. (*DGK*)

2.1 *Early days.* Harry Reuter was the son of Dr Ernst Rudolf Johannes Reuter and his first wife Charlotte, born in Berlin on 21 November 1921.

Ernst Reuter was a figure of historical importance. He escaped persecution as a Social Democrat only by going into exile first in England and then in Turkey, but after the war he returned and became Lord Mayor of what then became Berlin: he held office during the Soviet blockade of 1948–9, embodying personally the city's dogged resistance. His memory is celebrated today by the Ernst Reuter Platz in Berlin, and also by the Free University of Berlin which he helped to found in 1948.

In 1935 Harry, like many others, was sent from Germany to England, where he joined the family of Charles and Greta Burkill in Cambridge. (Harry Burkill—their other adopted mathematical son—says that in that household 'mathematics was in the air'.)

Harry was educated at the Leys School, under an exceedingly generous arrangement set up by the school in association with the Society of Friends 'Germany Emergency Committee'. From there he went on to Trinity College, Cambridge, and took Part II of the Mathematical Tripos in 1941.

In that year he joined the Royal Naval Scientific Service, and spent the war mostly in London, but he liked to recall the following incident. He had been sent on a mission to a naval base in the far north of Scotland, and was stopped at a control point and asked to show not only his Admiralty pass, but also his personal passport. The entry 'Place of birth: Berlin' caused great consternation.

When hostilities were over Harry was sent (like myself) to debrief German scientists. I achieved nothing more striking than bringing back offprints for the Royal Astronomical Society, but Harry and his colleagues (one was John Todd) brought off a great coup. They learned of the Mathematical Research Institute at Oberwolfach in the Black Forest, and decided to visit it despite the fact that it was in the French zone of occupation. When they found that it was destined to become an Officers' Club for the French Army, they protested vigorously *and won the day*—surely a famous victory, and one from which the whole mathematical world has benefited ever since.

The war over, Harry returned briefly to Cambridge as a pupil of Frank Smithies, who introduced him to functional analysis. He saw a lot of Littlewood and also of Mary Cartwright, with whom he worked on non-linear differential equations, but he soon departed to join Max Newman's team in Manchester, where he worked with Jack Good, Walter Ledermann, James Lighthill, Brian Pippard, Ernst Sondheimer, and others. It should be noted that he contributed substantially to both pure and applied mathematics in that period. The 'pure' papers will be discussed below in Section 3, and the 'applied' papers in Section 4.

2.2 *Markov chains.* Harry's work in probability began in 1952 when he and Walter Ledermann wrote two path-breaking papers **[11, 12]** on discrete-state Markov processes in continuous time. Their idea was to start with a truncated system involving the states $(1, 2, \ldots, N)$, and then to let $N$ tend to infinity. This was, of course, entirely in accord with contemporary differential equations thinking—'first compactify the state-space, and then blow up the boundary to infinity'.

At this time I was in Princeton as a Visiting Lecturer working with Will Feller, and when I received a letter from Ledermann reporting their extremely impressive results, I showed this to him. Feller was aghast, and explained to me that he had himself tried the same idea and worked very hard at it, but had abandoned the project when he became convinced that it simply *would not work*. (It was some years before Feller actually met Harry in person, and then—in Feller's own words—'it was love at first sight'.)

At that time I was myself busy with various aspects of birth-and-death processes, at first in an applied (not to say non-rigorous) spirit. I sent my necessary and sufficient criterion for the occurrence of an 'explosion' in a birth-and-death process (found simultaneously by Dobrushin in the USSR) to Harry and Walter, and looked forward eagerly to a meeting.

It was a simple matter to visit other East Coast Universities from Princeton, and one such visit was to have spectacular consequences. Mark Kac invited Diana and myself to Cornell, and there I met Kai-lai Chung. He said to me: 'Have you seen the little note (in Russian) by Kolmogorov on two special but very peculiar Markov processes?' I had not, but with help from Diana (who during her wartime career in the Women's Royal Naval Service had learned some Russian) I was able to puzzle out what Kolmogorov was doing.

Now Feller, in the preceding year, had given a long course on aspects of the Hille–Yosida theory of semigroups in the context of probability theory, and Alan James had written up really excellent notes on this. I borrowed his notes and worked through them, and was tremendously excited to realise that such a semigroup analysis of Kolmogorov's two special Markov chains (now always called K1 and K2) ought to illuminate what was going on. Accordingly, when a year later we returned to England on the *Queen Mary*, I could scarcely wait to get to Durham where I was to meet Harry for the first time at the British Mathematical Colloquium. A long and productive collaboration **[13–19]** then followed.

It turned out that Kolmogorov's analysis yielded enough information to identify the resolvent operators for the two processes, and in that way we were able to find the infinitesimal generators and their exact domains. We therefore set about the task of describing the behaviour of the sample functions themselves, bearing in mind Paul Lévy's recent path-theoretic classification of such processes. In this way we obtained essentially complete descriptions of the sample-function behaviour in each one of the two Kolmogorov examples. That work was presented as an invited paper [**13**] at the Amsterdam International Mathematical Congress in 1954.

Fortunately for us, Kolmogorov came in person to Amsterdam. Indeed, it was then that he gave (in French, and then again in German) the first account of what came to be known as KAM theory. I had already met Kolmogorov briefly at an International Statistical Institute meeting in Belgrade, when I had given him a provisional note on our joint work. At the Amsterdam meeting, Kolmogorov and Neyman held a luncheon party in the Zoo to which Harry and I were invited, and there Kolmogorov was able to answer a question that had puzzled us: 'do such Markov processes always have analytic transition functions?' He was able to show us a counter-example due to Yuskevič in which some of the transition functions fail even to be of class $C^2$.

The importance of all this was that we now had a menagerie of examples displaying various forms of 'pathological' behaviour, but we were to be surprised by another so-called pathology not suspected at that time (the existence of Markov semigroups for which *all* states are 'instantaneous').

Later we began to study in various ways and from various points of view the limiting behaviour of such countable-state Markov processes when the (continuous) time $t$ tends to infinity. It was already known that the limits $p_{i,j}(\infty)$ always exist. An interesting question is: when can one leap in time from $t = 0$ to $t = \infty$, and so immediately determine $p_{i,j}(\infty)$ for all values of $i$ and $j$ when the initial first (right-hand) derivatives $q_{i,j}$ at $t = 0$ are given, *without first having to compute the transition probabilities* $p_{i,j}(t)$ *throughout the intermediate range* $0 < t < \infty$? (Of course, it is here supposed that we are talking about the situation in which the $q_{i,j}$ do determine the $p_{i,j}(t)$ uniquely for each finite $t$, for otherwise the question does not make sense.)

This problem was finally solved by us at a British Mathematical Colloquium in Exeter [**19**]. (We had by this time established a useful, if unprincipled, practice of signing up for a Colloquium and then spending all the time working together.)

The solution we obtained was surprisingly simple, and involved only elementary lattice considerations. Its use can be (and has been) successfully taught to undergraduates, and the technique gives directly what one most needs to know when dealing with practical examples.

2.3 *Later collaboration with DGK.* The paper [**19**] was our last joint work, but after this our collaboration still continued though at a more informal level.

One such joint activity was the creation of the Stochastic Analysis Group (STAG for short). This was initiated on 20 December 1961 at a meeting in Oxford where I was then based, with Harry as Chairman and myself as Secretary. It had two aims: (1) to create a home for probabilists, and (2) to create more effective contact between the London Mathematical Society and the Royal Statistical Society. Meetings were arranged on what might be called a parasitic basis: thus when an LMS or RSS conference was organised, we would beg to be given an additional morning or afternoon session for our STAG activities—and we were never refused, and never charged. (We had absolutely no money anyway.) God bless such generous administrators!

There is no doubt that STAG was a great success. It caused the probabilists in the United Kingdom to think of themselves *as a community*, and designedly it faded out as soon as its objects were fully realised. That such a measure of success had been achieved was evident when the London Mathematical Society (during the Presidency of Dame Mary Cartwright and the Treasurership of Sir Edward Collingwood) joined Joe Gani in setting up the (now immensely influential) Applied Probability Trust, and began to think of the Royal Statistical Society as a helpful and supportive sister organisation. Such a success story, like good wine, needs no bush!

One associated initiative was the LMS Instructional Conference in Probability that was held in Durham in the Spring of 1963. This did much to interest pure mathematicians in the subject, and paved the way for a similar Conference on Algebraic Number Theory held in the University of Sussex in 1965, thus establishing what became a regular and valuable part of the LMS programme. The choice of Durham as venue in 1963 was a natural consequence of the fact that Harry was by now Professor there, and ever since there has been a close association between that University and the LMS.

It was at about this time that Harry and I began to get dazzlingly gifted research students, of whom I will mention here only Rollo Davidson, John Kingman and David Williams. These and many others formed a community of like-minded researchers for which Harry was very much a father figure.

In 1966 Harry and I were invited to a NATO conference on Probability at Loutraki in Greece. This saw the birth of Delphic semigroups, and also gave to Harry and myself an opportunity for adventure. We planned to reach (if not to climb) Mount Chelmos, using Frazer's translation of (and notes on) Pausanias as a guide book. The travel agent attached to the Conference gave us all the necessary rail times, but omitted to mention that because of a national holiday no trains would in fact be running on that weekend. However, we took a bus from Corinth to Diakofton, spent the night there, and then early the following morning we walked through the lemon groves and climbed up the sleepers and through the dark snake-infested tunnels of the mountain railway track that zigzagged at a daunting height from one side to the other of the great Diakofton gorge, until at length we arrived at the Megaspelion monastery, where we spent the night. Early the next morning we set off for Chelmos and the source of the Styx, but we lost our way in the forest, and so saw only the snows on the summit of our desired peak. We spent another night at the monastery, and then returned home (the trains were now running again).

When Rollo Davidson was tragically killed on the Piz Bernina in 1970, Harry characteristically gave a tremendous amount of his time in helping Ted Harding and myself to bring out the Davidson memorial volumes *Stochastic analysis* and *Stochastic geometry*, and shortly afterwards he joined us (as its first Chairman) on the Davidson Memorial Trust.

It was through the generosity of my old wartime rocketeer colleagues Mary and Robert Rankin that Harry and I and my elder son Wilfrid Kendall (then a student at Oxford) were able to spend a holiday together in a cottage on the Isle of Arran in 1976. When not admiring the seafowl, or looking for agates, we talked mathematics. Wilfrid (following the recent work by Simon Broadbent and myself on collinearity testing) was busy determining the statistical distribution of the 'shape' of a random gaussian triangle, and it was our joint discussion of this that was to pave the way to the creation of what later came to be known as the Statistical Theory of Shape. (It is of interest that F. L. Bookstein was in fact building up his own approach to a closely related discipline at about the same time.)

For his part, Harry showed us a recently arrived letter from Zhen-ting Hou (of the China Institute of Railways at Changsha) that raised and solved new problems in

Markov chains, and this initiated a China–UK probabilistic interchange that continues to this day, to our own great advantage.

Shortly after this we were able to meet Professor Hou in London, where he invited Harry and myself to visit China and give research lectures in his Institute. It took some time to work out the practicalities of this, and by then it had become clear that Harry was not well enough to undertake the journey, so David (and happily also Sheila) Williams went in his stead. We gave lectures on Markov chains, shape theory and data analysis (while Sheila gave lectures on British life and customs) at Guangzhou, Xiangtan, Changsha and Xi'an, and among other topics the so-called Germ Conjecture was mentioned.

The Germ Conjecture had been proposed by me in the book *Stochastic analysis*. It was suggested by my readings in quasi-analytic function theory, and goes like this.

CONJECTURE. *If Markov chains* $(p_{i,j}(\cdot): i,j \geqslant 1)$ *and* $(p^*_{i,j}(\cdot): i,j \geqslant 1)$ *satisfy*

$$p_{i,j}(t) = p^*_{i,j}(t) \quad \text{whenever} \quad 0 \leqslant t \leqslant \tau_{i,j},$$

*where the* $\tau_{i,j}$ *are positive, then the chains* $p$ *and* $p^*$ *are identical.*

This problem intrigued our hosts, and soon after our return to England there arrived from our Chinese colleagues very interesting *affirmative* solutions in a number of important special cases. Harry worked carefully through these difficult calculations, and was able to confirm their correctness. He wrote an invaluable review of the topic in [**33**].

Later, Harry gave much thought to another instance of the Germ Conjecture that was being studied by Sophia Kalpazidou of the Aristotle University of Thessaloniki. Here the Markov chains are supposed to possess a special sort of 'cycle structure'. Recently, her work has been finished and it appears that once again the Germ Conjecture has been verified.

At the time of writing, M. E. Ribe of the Chalmers University of Technology in Göteberg is working on yet another interesting class of cases, this time comprising Markov chains for which one might expect the Conjecture to *fail*. Ribe's work is not yet complete, and his final results are eagerly awaited.

I have myself wondered whether the general Germ Conjecture could be resolved by the use of non-standard analysis, but to work in that area is beyond my scope.

In preparing these notes I have tried to cover reasonably thoroughly the fundamental contributions that Harry made to theoretical probability, where he showed himself to be one of the great masters. In all he wrote 18 papers on Markov processes and related matters, alone or with others, his long 1957 paper in *Acta Mathematica* being of special importance [**18**]. His many contributions to applied mathematics were written in the same careful style—thus, for example, the two papers on epidemics (with Atkinson and Ridler-Rowe) greatly sharpened the degree of precision attainable in that subject.

In 1986 a tribute to Harry by his friends was published as a Special Supplement *Analytic and Geometric Stochastics* to the journal *Advances in Probability*. This contains a bibliography, a list of Harry's formal research students—there were many informal ones—and a portrait. All of these are reproduced here by kind permission of the Applied Probability Trust. (The bibliography has been slightly extended.)

### 3. *Contributions to pure mathematics* (*NHB*)

Harry Reuter's formative mathematical years were spent in Cambridge, where he acquired the excellent background in analysis that was to serve him so well throughout his career. Cambridge was most noted as the home of the Hardy–Littlewood school of classical analysis, but Frank Smithies—under whom Harry studied after the war—had introduced functional analysis in his influential lectures.

Harry began his career as an analyst in Manchester. His first published works in pure mathematics were two short isolated papers, **[1]** extending a result of Gabriel on subharmonic functions, and **[5]** a brief proof of the boundedness property of the Hermite orthogonal system from Mehler's formula.

From Littlewood, and his pupil M. L. (later Dame Mary) Cartwright, Harry acquired an interest in differential equations, particularly non-linear ordinary differential equations of the second order. (An account of the Littlewood–Cartwright work in this area is given by Swinnerton-Dyer **[S8]**.) In her study of forced oscillations in non-linear systems, Cartwright **[S1]** had considered equations of the form

$$\ddot{x}+kf(x)\dot{x}+g(x)=kp(t) \quad (k>0), \tag{i}$$

and shown that ultimately (that is, for large enough $t$) all solutions $x(t)$ are bounded:

$$|x|<B, \quad |\dot{x}|<B(k+1).$$

In **[8]**, Harry weakened her conditions, and in a sequel **[9]** considered the more general equation

$$\ddot{x}+kF(\dot{x})+g(x)=kp(t). \tag{ii}$$

Cartwright and Littlewood had proved in 1947 that if in (i) the 'damping factor' $kf(t)$ is positive, $g(x)$ is close enough to linearity, and $p(t)$ is periodic, then (i) has a unique periodic solution to which every other solution converges. In **[10]** Harry extended their work to the case of almost-periodic $p(t)$.

Ideas introduced by Levinson in the 1940s were important ingredients here; so too was the use of fixed-point theorems such as that of Brouwer. For background and references, see Cesari **[S3**, §9.7].

This early interest in asymptotics of differential equations returned to form a coda in Harry's work in pure mathematics. In **[32]**, written with Terry Lyons at Imperial College, he studied exponential bounds of the form $x(t)=O(e^{kt})$ for solutions to

$$\ddot{x}+f(t)x=0$$

for suitable $f$, $0\leqslant f(t)\leqslant 1$; the prototype for results of this kind can be traced back to Lyapunov in the early 1890s. In **[34]**, written with Dame Mary Cartwright in his (and her) retirement in Cambridge, he considered van der Pol's equation

$$\ddot{y}+k(y^2-1)\dot{y}+cy=bk\cos t$$

for large $k$ and $0<b<2/3$ (that $b=2/3$ is critical was known, from earlier work of Littlewood and work of Noel Lloyd in 1972). Again, key use was made of methods of Levinson. The van der Pol equation has played a major role throughout the history of these studies by Littlewood, Cartwright and Reuter; for background on it, see, for example, **[S2]**.

Harry had begun work in his Manchester days on a monograph on differential equations, but never published it. One of my regrets is that I learned of the existence of the manuscript only the week after Harry threw it away (in the early 1980s). In reply to my protestations of horror, Harry merely smiled, and said that he would no longer wish his name to be associated with it—presumably he felt that the subject had moved on. Be that as it may, he did publish a short elementary book on differential equations [**35**], in the Library of Mathematics series edited by his collaborator Walter Ledermann.

Harry's Manchester days brought him into contact with Jack Good, with whom he wrote [**4**] on integral transforms possessing inversion formulae of Fourier type (sine, cosine and Hankel transforms, for instance). These had been studied by Watson and by Titchmarsh, both in 1933; for a textbook account, see [**S9**, Chapter VIII].

One of Harry's most characteristic interests was in differentiability properties of Markov transition probabilities. In 1951, Kolmogorov had studied such questions. Harry was able to extract the analytic core of this work, and generalise it, in his study [**14**] of Volterra equations with completely monotone kernel. This is the only paper Harry wrote in German, and it was one of his favourites. Such Volterra equations are also important in Kingman's theory of regenerative phenomena [**S6**, **S7**].

Harry's interest in functional analysis is evident in his work on Banach lattices [**15**, **17**]. An abstract lattice is a Banach lattice for which the norm is additive on the positive cone:

$$x, y \geqslant 0 \quad \Rightarrow \quad \|x+y\| = \|x\| + \|y\|;$$

the prototype is a concrete lattice of the form $L(\Omega)$, where

$$\|x\| = \int_{\Omega} |x(t)|\, dt.$$

If one also has

$$x \wedge y = 0 \quad \Rightarrow \quad \|x+y\| = \|x-y\|,$$

one can represent an abstract lattice in concrete form (Kakutani [**S5**]). In [**15**], Harry studied the links between contraction semigroups and (Markov) transition semigroups, in the framework above. His results generalise the analytic core of a probabilistic construction of Doob, converting a sub-Markovian transition semigroup into a Markov one (for countable Markov chains; there is a version for diffusions due to Feller). In [**17**], with Bonsall, Harry proved a fixed-point theorem: if $P$ is a transition operator, $x \geqslant 0$, and $f$ is the liminf along a subsequence of Cesàro averages of $P^n x$, then $Pf = f$. This is related to Kakutani's mean ergodic theorem [**S5**].

Harry's work with Ledermann on the differential equations for Markov transition probabilities belongs more properly to his work on probability theory, and is described in Section 2.2. It suffices to point out here that Harry's work in this area forms an important contribution to the very productive interaction between probability theory and the analytic theory of semigroups; for background, see Hille and Phillips [**S4**, Chapter XXIII, §4].

## 4. *Contributions to applied mathematics* (*EHS*)

Harry Reuter was primarily an analyst who, as the list of his papers indicates, liked to work on problems with an 'applied' flavour. Applied mathematics is a subject with notoriously elusive boundaries: I have picked out five papers, [**2**], [**3**], [**21**], [**29**] and [**30**], which may rank as contributions to applied mathematics and which well illustrate Harry's range and power as a mathematician. I myself was involved with him, right at the beginning of his mathematical career (and my own), in the work on the 'anomalous skin effect' (papers **[2]** and **[3]**); in commenting on papers **[21]**, **[29]** and **[30]** I have relied on help given by Professors Trevor Stuart and Colin Atkinson and Dr Chris Ridler-Rowe, to whom I express my warmest thanks.

In 1946 I was a raw research student at the Cavendish Laboratory in Cambridge, having taken a physics degree during the war. A. B. (later Sir Brian) Pippard, back in Cambridge, was applying the microwave techniques he had helped to develop in work on radar during the war to study the anomalous conduction properties of pure metals at low temperatures and high frequencies. Under these conditions, when the mean free path of the electrons becomes larger than the depth of penetration of the applied electric field, Ohm's law (that venerable corner-stone of physics) no longer applies and has to be replaced by a non-local relation between current and field. Pippard, in addition to carrying out superb experiments, developed a qualitative theory of this startling anomalous skin effect, and I tried to make it quantitative. I re-derived the basic integro-differential equation, using a method developed by Klaus Fuchs in a pre-war study of the conductivity of thin metallic films. I could see that I had an exciting problem on my hands, but my mathematics was not up to solving the equation. I took it to Professor Besicovitch, who was at that time trying to teach me to play billiards, but Bessi said that this sort of thing was not in his line and I had better show it to Dr Friedlander. Gerard Friedlander told me that it looked interesting, but his wife was not well and he was just moving to Manchester; he could not look at it just then, but would make a note. And I regretfully put my equation on the shelf of unsolved problems.

I well remember the excitement when, about six months later, a letter came from Harry Reuter in Manchester (I barely knew him at the time, having met him once or twice at the Burkills'), saying that he happened to be working on integral equations, and was I still interested in the solution of that equation which Friedlander had shown him? Thereupon a stream of formulae began to pour forth from Manchester which I struggled to interpret and understand. Finally, the solutions had to be computed—a non-trivial problem in those days. I approached Maurice Wilkes, then busy developing the Cambridge EDSAC, who told me to come back in about two years' time.... So I spent the hot summer of 1947 hammering away at a Marchant electrical calculating machine—and great was the delight when the results fitted Pippard's measurements quite well.

Two further comments may be of interest. First, there was alarm, just as we were getting ready to submit our paper **[3]**, when a note in the *Physical Review* seemed to suggest that we had been pipped at the post. Fortunately (for us), the author had not fully grasped the subtleties of the problem, and paper **[2]** was written to put him right. Secondly, a curious aspect of the story is that Harry received first the (mathematically) 'hard' limiting case of the problem (corresponding to diffuse surface scattering of electrons), when the full apparatus of Wiener–Hopf technique is needed for the solution; it was only later that I also sent him the 'easy' limiting case (of specular scattering), when a simple Fourier transform suffices. Perhaps this was just as well—if he had seen the easy problem first, Harry might not have thought it worth the bother. It was my great good fortune to have served as a middle-man between men with the physical insight of Brian Pippard and the analytical skill of Harry Reuter. For background to subsequent developments related to the basic work on the anomalous skin effect, see Chapter 3 in the recently published history of solid-state physics **[S10]**.

I turn to the other 'applied' papers co-authored by Harry. Paper **[21]** is a note written by Harry Reuter and Keith Stewartson when both were professors in

Durham. It considers the boundary layer equations for the flow of viscous, electrically conducting incompressible fluid past a semi-infinite flat plate in the presence of a magnetic field. The authors show that the boundary-value problem has no solutions when $\beta > 1$, where $\beta$ is the square of the ratio of the Alfven velocity to the fluid velocity a long way upstream of the plate. This result implies that, under these conditions, the notion of a boundary layer originating at the leading edge is no longer appropriate. It seems likely that the physical motivation for this investigation came from Keith, and that the elegant mathematical proof was supplied by Harry.

Paper **[29]** by Colin Atkinson and Harry Reuter considers the integro-differential equations characterising the deterministic model for the spread of an epidemic. Special cases had been considered earlier by David Kendall and Denis Mollison; Atkinson and Reuter were able to prove an existence theorem for travelling-wave solutions of the equations, making very general assumptions about the probability density function. This proof was described as a *tour de force* by Denis Mollison, in a subsequent survey paper. The three-man paper **[30]** deals with the non-linear diffusion equation in the case where the diffusion flux as well as a source term are non-linear, and wave solutions are shown to exist if and only if the wave speed exceeds a critical value. This theory has applications in population dynamics, laminar flame theory and nerve propagation. David Kendall has referred somewhere to the importance of pursuing a substantial mathematical enquiry for its own sake, in the confident belief that it will ultimately find some rewarding if unexpected application. It seems to me that Harry Reuter's mathematical work illustrates this admirable maxim to perfection. And to give an example of a 'rewarding if unexpected' application of the formalism of paper **[29]**, it has been suggested that it can serve to describe the flight of a cuckoo which, after a random flight, decides to lay an egg and chooses a tree at random from whatever neighbourhood it may have arrived at, the location of the tree being chosen from a probability distribution.

I myself never again collaborated scientifically with Harry Reuter, but should perhaps mention that I later found his little book **[35]** on elementary differential equations a life-saver in my own lecture courses. Here he succeeds in giving a limpid and interesting account of a subject that normally tends to be rather turgid: a triumph of the Reuter style. When, in the 1970s, we were both professors in London, our paths would cross occasionally when Harry came to my College to attend the London Probability Seminar. Nick Bingham has reminded me that we would exchange pleasantries in German—not because either of us felt at all German, but presumably just to irritate the rest of the company. Every encounter with Harry Reuter, with his wry sense of humour and his gentle wisdom, was an uplifting experience.

## 5. *Family and career* (*DGK*)

Harry married Eileen Legard in 1945, and they had four children: Timothy, Penelope, Stella and Elizabeth (my god-daughter). The Reuter and Kendall families spent two seaside holidays together at Rhosneigr in Anglesey. Timothy seemed in those days destined to become an ornithologist, but instead he went to Peterhouse as a historian and is now a Member of Monumenta Germaniae Historica in Munich. He and the three daughters are all married, and there are eleven grandchildren. Harry's first years in university teaching were spent at Manchester, where he joined Max Newman's famous team in 1948. He then went as Professor of Pure Mathematics to Durham in 1959, and to Imperial College in 1965. At Imperial College he eventually became Head of the Department of Mathematics, and he finally retired in 1983. His only sabbatical was spent at Yale (1958–9). Throughout this period Harry was an active and devoted member of the London Mathematical Society, as Vice-President, as Secretary, as Journal Secretary, and as Editor of the *Proceedings* and the Monographs.

Harry's willingness to help younger colleagues from any part of the world was legendary; see, for example, the reference to him in Jie-zhong Zou's paper in *J. London Math. Soc.* (2) 38 (1988) 356–366. Harry's formal research students (as already mentioned, there were many informal ones) were as follows.

| | |
|---|---|
| E. J. R. Archinard | A. M. Ben Lashiher |
| J. R. Choksi | A. G. Cornish |
| R. A. Doney | D. J. Emery |
| J. Hawkes (shared with S. J. Taylor) | J. Lane |
| D. Mannion (shared with DGK) | J. Ortega-Sanchez |
| C. J. Ridler-Rowe | P. W. Riley |
| R. S. Slack | R. Trottnow (shared with Y. N. Dowker) |
| D. Williams (shared with DGK) | |

## 6. *The return to Cambridge in* 1983 (*DGK*)

Harry's later days were very peaceful, despite a progressive illness which he bore with great patience and dignity.

It was a wonderful change for me to have him living in the same city. At 47 Madingley Road, one was always sure of a warm welcome from Harry and Eileen, and equally sure to find support and wisdom, and a very gentle reproof if one had done something *really* outrageous. Indeed, I suspect that for many others besides myself, Harry had become a touchstone of integrity. Many specious compromises were quietly dropped after a chat with him.

In his last years Harry came to know my student (and later colleague) Huiling Le, whom David Williams and I had met in Xi'an, and who to our great gain came in due course to Cambridge. So in the end Harry did make real contact with China, after all.

Harry Reuter died in Cambridge on 20 April 1992.

In several places in this memoir we have made use of material first published in earlier obituaries in the *Journal of Applied Probability*, and in *The Times* and *Guardian* newspapers.

## *Publications of G. E. H. Reuter*

1. 'An inequality for integrals of subharmonic functions over convex surfaces', *J. London Math. Soc.* 23 (1948) 56–58.
2. (with A. B. Pippard and E. H. Sondheimer) 'The conductivity of metals at microwave frequencies', *Phys. Rev.* 73 (1948) 920–921.
3. (with E. H. Sondheimer) 'The theory of the anomalous skin effect', *Proc. Roy. Soc. London Ser. A* 195 (1948) 336–364.
4. (with I. J. Good) 'Bounded integral transforms', *Quart. J. Math.* 19 (1948) 224–234.
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