

Recollections of Logicians, Mathematicians, and Philosophers

John L. Bell

For John Crossley on his 85th birthday

John Crossley played a major role in my becoming a mathematical logician. But before meeting him I had other aspirations.

In my youth, I dreamt of becoming a theoretical physicist, and Einstein, George Gamow and Fred Hoyle headed my list of scientific heroes. I was initially accepted at Oxford to study physics, but by the time I got there my interest had shifted to mathematics. This was in no small part due to the influence of my earliest mentor, the Cambridge mathematician and astronomer E. H. Linfoot, about whom I have written elsewhere ([1]).

Linfoot had an extensive mathematical library. It included a copy of John Kelley's pedagogic masterpiece *General Topology* which he was kind enough to lend me. Kelley's book was my introduction to the elegant abstractions of 20th century mathematics, which supplanted the tortuous intricacies of the traditional mathematical analysis I had wrestled with up to then. From Kelley's book I learned, not just about general topology, but about set theory and the concept- which I found bewitching - of a Boolean algebra. Many years later I had the good fortune to meet Kelley, who proved to be as engaging in person as he was as an author.

When I went up to Oxford I had just the vaguest notion of logic. I had heard of Gödel, but only in connection with his contributions to relativistic cosmology which I had learned about through my previous attendance at Fred Hoyle's Cambridge lectures on the subject.

My undergraduate college at Oxford, Exeter, lacked a pure mathematics Fellow while I was there, so I was "farmed out" to a number of mathematics tutors in other colleges: in my first year to the functional analyst David Edwards at Lincoln College, and in my second year to the topologist Brian Steer at Hertford. During that time I developed an interest in set theory. I studied in particular Gödel's - yes! that very same Gödel of cosmological fame - trailblazing monograph *The Consistency of the Axiom of Choice and of the Generalized Continuum Hypothesis with the Axioms of Set Theory*. This had stimulated my curiosity about logic, and so I was excited to learn at the beginning of my third undergraduate year that my tutor was to be John Crossley, a Fellow of St. Catherine's College, -who was at the time the sole mathematical logician on the Oxford faculty. At our first meeting I was struck by his youth - he was 26, just 7 years my senior - and also his warmth, his informality and his enthusiasm. His Beatles-style haircut and colourful Carnaby Street gear also made a considerable impression on me. I felt we hit it off instantly, and I was sure that it was going to be rewarding, and great fun, to become his tutee. My expectation was fully realized. Despite the fact that I was still an undergraduate, and had yet to undergo the ordeal of Schools, John treated me as if I were already his graduate student. John's own research centred on recursion theory, and he made a considerable effort to interest me in the subject. He suggested that I read through Hartley Rogers' notes on recursion

theory (the well-known book having not yet been published). At John's suggestion, I presented at his seminar an exposition of Myhill and Shepherdson's 1955 paper *Effective operations on partial recursive functions* ([13]). (I was later to meet both Myhill and Shepherdson.) But recursion theory simply did not appeal to me.

By a happy coincidence, John had arranged to teach a graduate course on model theory that year, devoting the first part of his presentation to a proof of the completeness theorem using Boolean algebraic methods. With my enthusiasm for Boolean algebras, I was thrilled at this prospect of attending John's lectures. Alan Slomson, already a graduate student of John's, and I were delegated to take notes on the lectures. Alan and I proved ideal collaborators, developing such an enthusiasm for model theory that John suggested we give the course the following year, after I had taken Schools and officially become John's graduate student.

Using as a basis the notes we had taken on John's course, Alan and I wrote up our own course, which we gave in Oxford in 1965-6. Later John encouraged us to polish up our course notes into publishable form, and smoothed the way for the resulting book to be published by North-Holland. Thus was Bell and Slomson: *Models and Ultraproducts* [2], the first textbook on model theory, born. Alan and I both recognized how important a part John's generosity and encouragement had played in its birth.

John bubbled with enthusiasm for logic, and I recall that one day he proposed a drive to Bristol to attend a lecture by the logician Georg Kreisel in the mathematics department at the university there, which was at the time the centre of British activity in mathematical logic. So Peter Aczel (John's first doctoral student, who later, of course, went on to a distinguished career as a mathematical logician), Alan and I all piled into John's car and roared up to Bristol. Immediately before our departure John insisted, in his droll way, that we all "belt up" – an amusing double entendre instructing his passengers both to button their lips and to fasten the seat belts which he had installed, long before their use had become mandatory in passenger vehicles. We had good reason to be grateful for the presence of these belts, since John showed little respect for speed limits. As for Kreisel, all I then knew about the man was that his name is an anagram of "Keisler", a logician whose papers I had begun to study (and whom I was first to meet some 10 years later), so I had no idea what to expect. I cannot now recall the topic of Kreisel's lecture, but whatever it was I found it baffling. Oddly enough, a couple of years later Kreisel was to act, very briefly, as my surrogate research advisor.

John was generous to all his students, and to me in particular. On a number of occasions I enjoyed his and his wife Stella's hospitality at dinner in their house in Kidlington north of Oxford. One evening John invited me to dine at St. Catherine's High Table. I felt somewhat nervous when I was directed to take one of the College's characteristically modernistic high-backed seats next to the Master of the College. This was Alan Bullock, the eminent historian, author of *Hitler: A Study in Tyranny*. Bullock, affable and down-to-earth, made me feel very much at my ease.

John encouraged me to apply for Senior Scholarships in Oxford colleges to support my graduate studies. One of these was at Christ Church, a college with which I had had no contact as an undergraduate. Well-known as the grandest college in Oxford, for centuries it had served as one of the chief academic incubators of the British upper class. It seemed to me most unlikely that I had much of a chance of being awarded a scholarship at this venerable institution, but John insisted that I submit an application there. I was pleasantly surprised when my application was successful, and I was awarded "a Senior Scholarship at the

House of £500 per annum, tenable for 4 years, including free rooms and meals." This happy outcome I owed to John's support and encouragement.

As organizer-in-chief of the 1965 Summer School in Mathematical Logic ([10]) John arranged for all his students, including Alan and myself, to attend. Held over a three-week period during August and September in a hall of residence of Leicester University at Oadby, a village just outside Leicester, this conference, my first, was a formative experience. Not only did I have the opportunity to meet – and learn from – some of the world's most distinguished logicians, but during the course of the conference, which proved very enjoyable, I made a number of enduring friendships. The summer school featured lecture courses designed as introductions to advanced topics in mathematical logic. Alan and I attended C. C. Chang's *Ultraproducts and Other Methods of Constructing Models* and Dana Scott's *Measurable Cardinals*. These were superb expositions, from which we both learned much. Chang and Scott were quite approachable, and during the conference I became in particular quite friendly with Chang, or "C.C." as he was generally known. C.C.'s Chinese urbanity dry wit, and delicate guitar playing, appealed to me, and I was well aware that, as one of the world's outstanding logicians, he could teach me a great deal. Learning that he would be taking sabbatical leave from UCLA in 1966-7, I urged him to spend it in Oxford, so that I might study with him that year.

Moshé Machover, at the time a visiting lecturer in the Bristol mathematics department, also attended the Leicester conference, where he delivered an introductory course on set theory. I had first met him at John's seminar in the old Mathematical Institute the previous year. I recall that on the blackboard announcing his talk his name had been chalked up as "Dr. M. MacOver". I was surprised when, instead of the dour Scot I had expected, an intense, strong-featured young man, evidently of Middle Eastern origin, marched into the lecture room. I do not recall whether Moshé and I were introduced after that lecture, but I do know that we quickly hit it off at the Leicester conference. Right from the start I felt an affinity, a comradeship with him. While I admired the acuity of his intellect, I was even more strongly drawn by the strength of his personality and by his whimsical sense of humour. We were to become close friends and colleagues.

I met Bill Lawvere for the first time at the summer school. Amiable yet intensely serious, he had a burning ambition: to establish category theory as the organon of mathematics. I recall him showing me a paper he was then in the process of writing in his large, sprawling hand. It was entitled *The Category of Categories as a Foundation for Mathematics*. Knowing next to nothing of category theory beyond the bare definitions, I could not then grasp the import of what he was trying to achieve. Years were to pass before the course of my own work led me to a partial understanding of the thinking of this visionary mathematician, but when that understanding finally came, it was a revelation.

The philosopher of science Imre Lakatos (whose colleague at the London School of Economics I was later to become) also attended the summer school. He could often be spotted at the centre of an entourage of disciples, all engaged in earnest discussion. Another attendee was the outstanding mathematical logician Haim Gaifman, brilliant, intense and combative.

In October 1965 Alan Slomson and I began the course of lectures on model theory that John had arranged. The affair took place in a cramped lecture room in the old Mathematical Institute at 10 Parks Road. The first few lectures introducing the theory of Boolean algebras were my responsibility; I still recall how

nervous I was during the first of these. Among the audience of 10 or so was Wilfrid Hodges, who became John's student, and later an eminent model theorist.

I first met George Wilmers when he became a graduate student of John's in 1966. We soon became close friends. George had (and has) a number of qualities I admired. In addition to his gifts as a mathematician, he was an excellent pianist, linguist, and chess player. In George were combined acuteness of intellect, sensibility to beauty, and a curious dreaminess. He would gaze at you abstractedly with his dark eyes, his mind seemingly elsewhere, and then, as if out of the blue, produce an observation of startling penetration. I recall a conversation in which Wittgenstein's apothegm "Death is not an event in life: we do not live to experience death" somehow came up. A few moments went by, and then George, who had seemed not to be listening, observed "In other words, life is an open set." I have never forgotten George's aperçu.

When George deigned to pay attention to what one was saying, however, the dreamer would be suddenly replaced by a formidable critic, a merciless gadfly questioning every proposition one had the temerity to put forward. I often had to scramble to justify some unreflective remark of mine he had elegantly punctured.

George's earliest work in logic was in nonstandard models of set theory and arithmetic. Later, at Manchester University, he took up the study of probabilistic uncertain reasoning in social contexts, and the theory of voting.

At this time I also met David Park, a brilliant logician turned computer scientist who, having been an undergraduate at Oxford in the 1950s, had returned briefly to his alma mater before taking up an appointment at Warwick. He had an anarchic streak which greatly appealed to me. I have never forgotten the occasion when, as we were walking down the High, he remarked, with a sweeping gesture taking in the college façades, "in my day we wanted to blow all this up". I was saddened to learn that David died in 1990.

During the Michaelmas term of 1965 Abraham Robinson was resident in Oxford as a research fellow at St. Catherine's College. I attended the series of lectures he delivered on Nonstandard Analysis, the revolutionary approach to analysis, based on infinitesimals, he had recently formulated. Some thirty years later I wrote a letter to his biographer Joseph Dauben in which I reported my impressions of Robinson's lectures.

*As I recall, the lecture hall was [always] packed – the audience included Moshé Machover, Alan Slomson, Peter Aczel, John Wright, Frank Jellett, John Crossley, and Joel Friedman (his student who had accompanied him from UCLA). These lectures were very absorbing – it was evident that Robinson was presenting something of fundamental importance – and delivered with what I can only describe as an endearing lack of slickness. For example, he had a circuitous method of proving mathematical propositions at the blackboard which apparently proceeded as follows. To prove a proposition P , he would start by assuming not P . He would then prove P completely independently of the assumption not P , deduce that the latter must be false, and then finally infer the truth of P . This is not the familiar form of *reductio argument*:*

$$\frac{}{\vdash \neg P \rightarrow P}$$

$$\vdash P$$

but rather what I came to call the “Robinsonian” form:

$$\frac{}{\vdash P}$$

$$\frac{}{\vdash \neg \neg P}$$

$$\vdash P$$

At the end of the course Robinson held a party to which all the members of his audience were invited. I remember this as a very warm and enjoyable occasion. In my letter to Dauben I also remarked:

The only other time Robinson and I met was (I think) in Amsterdam sometime in the early 1970s. Of this brief encounter I recollect only that his friendliness to me seemed undiminished, despite the fact that not long before I had been involved in organizing an antimilitary logic conference which had not met with the approval of all logicians.

In my opinion, Abraham Robinson was not only a mathematician of great originality, he was truly, in Wittgenstein’s sense, a human being.

I got to know Moshé Machover well during my first graduate year. After meeting at the Leicester conference, we saw each other at Abraham Robinson’s lectures in Oxford, to which Moshé regularly commuted from Bristol. On several occasions he and his wife Ilana made me welcome at their Bristol flat, which was on the ground floor of a house in The Paragon, a curved Georgian terrace, evidently once fashionable, but by the 1960s sadly decayed. Ilana and I shared an interest in Russian literature. Whenever I mentioned a Russian novel that I had read in English translation, she would insist that the Hebrew version was far superior. We were both very fond of the Russian satirists Ilf and Petrov; it gave me considerable pleasure to translate into English an amusing short story of theirs, The Soviet Robinson, and present it to her.

Moshé was both mathematician and active socialist. He had joined the Israeli Communist Party as a teenager only to be expelled (absurdly, as a “Maoist”) for rejecting the Party’s pro-Zionist line. In 1962 he and a small group of like-minded anti-Zionists – including Akiva Orr and Shimon Tzabar, both of whom I was later to meet – thereupon established the Israel Socialist Organization, known as Matzpen (“Compass”), dedicated to the establishment of a socialist, secular Middle East, uniting Arabs and Jews. Naturally, this declared aim led to the vilification of Matzpen from all sides, and a number of its members, including Moshé, were essentially forced into exile.

Moshé’s strength of conviction and lucidity of thought and expression made a great impression on me, and I looked up to him as a mentor. At once activist and perfectionist (that rarest of combinations) he brought to his political and economic analyses the same exemplary standards of rigour and clarity that distinguished his work in mathematical logic.

John had meanwhile become a Fellow of All Souls on his appointment as Lecturer in Mathematical Logic, and was naturally very pleased about it. He obligingly presented me with a key to his college room so that I might have access to his typewriter on which I was preparing my Diploma dissertation. For the Diploma

one was also required to undergo an oral examination. John became sufficiently concerned at my habitual late rising to rush over to my rooms in Christ Church on the day of my Diploma *viva-voce* and drag me out of bed. Thanks to him, I showed up on time and at all went well at the examination.

John had arranged to go on leave during the first half of the academic year 1966-67, and C.C., who, as I hoped, had arranged to visit Oxford that year, was to act as my research supervisor in his absence. I looked forward eagerly to working with him. Everything went well, even jokily, to begin with: I recall that, with mock ceremoniousness, he would address me as “Your Most Senior Scholarship”, to which I would respond in kind with “Your Highly Esteemed Professorship”. But this pleasant jocularly came to an abrupt end when he gave me the official assignment of presenting some work at a seminar he had organized. The work in question was Jack Silver’s recent Berkeley dissertation on large cardinals and constructible sets. This is a technical tour-de-force written with extreme economy, and I had some difficulty in getting to grips with it. My presentation in the series of seminars clearly did not satisfy C.C., to the point at which he finally got up in the middle of one of my lectures and proclaimed to the assembly that I didn’t understand what I was doing, or words to that effect. While that may well have been true, I was stung by this public dressing-down.

Sadly, my soured relations with C.C. continued at one remove when sometime afterwards I heard a rumour attributing to him the belief that in producing our book *Models and Ultraproducts*, Alan and I had “plagiarized” his 1965 Leicester lectures on ultraproducts. I don’t know whether he actually believed this. Our exposition did draw on the published version of his lectures (which we had attended), but proper references and attributions were supplied. Perhaps we should have made explicit acknowledgement of the influence of his lectures in our introduction. His pique may well have been compounded by the fact that our effort appeared some time before his and Keisler’s book on model theory, whose publication had been delayed by the collapse of their intended publishers van Nostrand. Eventually their book was published under the North-Holland imprint and quickly became the standard reference.

Several footnotes to this affair. Some years later I was told by Wilfrid Hodges that C.C. had expressed “surprise” at my reaction to his reproof. Apparently the public chastisement of graduate students was no more than standard practice in the Tarski school in which C.C. had originated. I feel fortunate that I largely avoided such “education by humiliation”. I met C.C. on one further occasion at a conference in the early 70s. We exchanged pleasantries, but there was little warmth. Not long afterwards I learned that he had abandoned research in logic (but not his professorship at UCLA) and joined an Oriental religious sect. He died in 2014 at age 87.

During my last year in Oxford Kreisel turned up for an extended visit, and John arranged for all of his current graduate students to have occasional audiences with the great man. It had been arranged for Kreisel to deliver a course of lectures for which Jane Bridge, a fellow student of John’s, was deputized to take notes, a task I recall she did not relish. My few meetings with him seemed to go reasonably well. When I expressed an interest in infinitary languages, he gave me a copy of a handwritten draft of his paper ([11]) on the subject that later appeared in Barwise’s conference volume.

Imre Lakatos had meanwhile heard that Alan Slomson and I had been giving lectures on model theory in Oxford, and wrote to us proposing that we offer our course at the LSE. Alan and I readily fell in with the idea, but a few weeks before we were due to begin we received an apologetic letter from Lakatos calling

the whole thing off, on the grounds that his graduate students were, in his words, “revolting” against overwork.

By the end of 1967 I felt ready to leave Oxford and so, on the strength of Lakatos’s earlier offer, I wrote to him to enquire whether there might be a vacancy in his department (philosophy) at the LSE. In his reply he told me that there had been a vacancy in logic, but that had recently been filled. Nevertheless he thought I might be interested in a position in the recently founded mathematics department at the LSE. He mentioned one minor obstacle: the closing date for applications had expired some weeks earlier. But he genially assured me that he would get round this by “smuggling my application into the file.” And he was as good as his word, for a couple of months later I was summoned for interview at the LSE. The interview itself (which I cannot now recall) must have gone well, for a few days afterwards I received the offer of an Assistant Lectureship in Mathematics there. I took this up in 1968. Coincidentally, both Moshe Machover and Wilfrid Hodges all accepted positions in London University at the same time, Moshe at Chelsea College and Wilfrid at Bedford.

In 1969 the eminent logicians Leon Henkin and Andrzej Mostowski were at All Souls College as Visiting Fellows and I had the privilege of meeting both of them. Henkin was a droll man, very indulgent to his two young sons, who clearly didn’t take him very seriously. Mostowski combined old-world graciousness with wit and warmth in an appealing way. At a pleasant lunch with Jane Bridge and her family to which both Mostowski and I had been invited, I remember him pointing out with evident amusement that his name and that of his hosts were cognates: in Polish “most” means “bridge”, and so his name could be translated “Bridgeson”. While at All Souls, the political situation in Poland reached a crisis point, and he cut short his visit to Oxford to return there.

In the summer of 1970 I attended the conference in mathematical logic ([11]) held at Bedford College in London. Among the participants were Sol Feferman, Haim Gaifman, Angus Macintyre, Jeff Paris, Hartley Rogers, C.C. Chang, H.J. Keisler, Wilfrid Hodges, Peter Aczel, Max Dickmann, Yoshindo Suzuki and Alan Slomson. My contribution to the proceedings was a brief account of a paper I had coauthored with Francis Jellet, *An effective implication in functional analysis* ([3]). As I gave my talk I wondered who the distinguished-looking elderly gentleman in the audience might be. It turned out that he was Arend Heyting, a founding father of mathematical intuitionism. At that time I knew next to nothing about intuitionistic mathematics, but I now surmise that the reason for Heyting’s attendance at my talk may have been the occurrence of the word “effective” in its title. If that was indeed the reason, then Heyting must have been disappointed, for by “effective” I meant merely the eschewing the use of the Axiom of Choice - a far cry from the term’s intuitionistic meaning. After my talk one of the participants, the brilliant set-theorist Robert Solovay, came up to me and rapidly outlined another method of proving our result. This is the sole occasion on which I met Solovay, but the sheer speed of his thinking left an indelible impression.

Imre Lakatos, my original LSE contact, was one of the most colourful members of the LSE faculty. Trained originally in mathematics, he had been an active member of the Hungarian communist underground during the war. Later he rose to a prominent position in the Hungarian Ministry of Education, only to be purged, as were many sincere communists, in the early 1950s. He escaped to the West during the 1956 uprising, took a second Ph.D. at Cambridge, and joined the LSE philosophy department, where Karl Popper—the great philosopher of science who had become Lakatos’s idol after his disillusionment with communism—still taught. (Popper retired from the LSE in 1969, so he and I were, technically, colleagues

for a year.) Lakatos's academic fame rested on *Proofs and Refutations*, the published version of his Cambridge thesis. In this brilliant, polemical work, the tangled history of the development of Euler's polyhedron formula $V - E + F = 2$ is dissected with infectious brio. The text bears witness to the continuing influence on him of the Marxist dialectics of his youth – a fact causing some embarrassment to the work's conservative editors when it was finally published in book form in 1976, two years after its author's death. But by the time I met Lakatos he had become a model reactionary, an "inverted Stalinist" in Moshé Machover's apt phrase. Imre was shrill in his support for U.S. policy, in Vietnam and, indeed, everywhere else; he identified the Soviet Union as an evil empire, long before Ronald Reagan popularized the phrase. I felt fortunate that I was not a member of his department, since, had I been, our widely divergent political views might have sparked off something more serious than the mere verbal sparring we happily, and harmlessly, engaged in.

Imre's volatility, capacity for dramatization, and massive, yet curiously vulnerable ego were all reminiscent of the stage Hungarian, as if straight out of a Molnar play. Imre was known for the sharpness of his wit, an attribute to which I can testify when I recall the occasion on which he had invited Michael Dummett (who had been my D.Phil. examiner at Oxford) to speak at his seminar. Before Dummett's lecture Imre and I had been going at it, as usual, hammer and tongs (if not sickle): somehow the subject turned to the Soviet authorities' recent incarceration of political dissidents in psychiatric wards, a repellent policy with which Imre seemed to associate all "leftists" such as myself and "liberals" such as Dummett. Imre was still gnawing away at this bone of contention as we entered the lecture hall. Soon after the start of Dummett's lecture, Imre excitedly scribbled a note and passed it to me. It read: "The political views of people like you and Dummett will eventually land both of you in a psychiatric ward." Quickly I scrawled back: "If so, you'll be there with us." Imre topped this with: "Yes, but I'll be the warden!"

Clausewitz, famously, defined war as "the continuation of politics by other means". For Imre, this phrase served to define philosophy, or his brand of it, at least. He was still sufficiently Marxist to cleave to the view that "the philosophers have interpreted the world – the point is to change it", even if the world he was now bent on changing had shrunk to no more than the arena of professional philosophy. He fought an unceasing battle against the Oxford philosophical tradition, which he saw as both ingrown and effete, and whose domination of the British academic scene he thoroughly resented. (I took it that he forgave me my attendance at that antique institution on the grounds that I was a mathematician, not a philosopher.) Imre's combative nature caused him to fall out even with his allies. In an unparalleled act of apostasy, he provoked a schism in the Popperian "church" by rejecting his mentor's doctrine of pure refutationism, and substituting his own "methodology of scientific research programmes". Master and ex-disciple hurled anathemas at each other virtually until the day of Imre's death. Paul Feyerabend, the prominent philosopher of science and, latterly, controversialist, was another former ally of Imre's who showed up at LSE now and then. When Feyerabend came to advocate his notorious "anything goes" attitude toward science (which struck me as a pose, a way of cocking a snook at the academic establishment – a bit of a lark in fact), Imre took up the cudgels in defense of scientific responsibility, denouncing Feyerabend's anarchism as an outrageous dereliction of intellectual duty.

But Imre himself had an irresponsible streak, which sometimes got him into trouble. I well recall the occasion on which Mostowski, then visiting Oxford, came to speak at Imre's seminar. We had all been invited to dinner afterwards by Moshé Machover. On the way over Imre treated us to a somewhat boastful account of how he had managed to extinguish a fire in his flat's curtains by pulling them down with his

hands – his fingers, amazingly, suffering no ill effects. I observed that either this was a miracle or he must have asbestos fingertips. My remark amused everyone except Imre, who rarely appreciated other people's humour. Later, at dinner, Mostowski mentioned his concern about mounting an effective defense for a number of his students who, as political dissidents, had run into difficulties with the Polish regime. He then asked Imre what he would do in similar circumstances. Imre responded flippantly that they would simply have to sink or swim – it was no responsibility of his. Shocked by this reply, Mostowski told Imre that he regarded his attitude as nothing short of irresponsible. The evening ended on a somewhat sour note. Before this contretemps Imre had sung Mostowski's praises without cease; I noted that, a few days later, he had changed his tune completely.

Imre's communist past naturally made him an object of suspicion in the eyes of the British political authorities. Rumour had it that, despite his fervent disavowal of Marxism, he never succeeded in acquiring British nationality. I recall entering his office one day and spotting an unopened parcel on his desk addressed to "Sir Imre Lakatos". Pointing at it, I remarked, "Wishful thinking, eh?" His reply: "Yes, now they're confusing me with Sir Karl!" – Popper having been knighted in the 60s.

Imre ran his department along the lines of a Renaissance studio, with a touch of Hollywood thrown in. Responsibility for the grand design was his alone, leaving minor details to be filled in at his direction by his apprentices – that is, his graduate students. When Mike Hallett, whose reputation in the philosophy department had been secured by obtaining the top First of his year, expressed the desire to study the philosophical foundations of set theory under my supervision, Imre raised two objections: that mathematics was generally much too difficult for philosophers, and that in any case he, Imre, had already assigned Mike his niche in the grand scheme of the methodology of scientific research programmes (I can no longer recall what that niche was). In the end I convinced Imre that Mike's mathematical talents were more than adequate to meet his first objection. But Imre refused to give way entirely on the second, insisting that Mike find a way of fitting his work into the grand design.

Imre had fought hard to achieve his position, and could be very aggressive in its defense, as is shown by the following episode. My then student Graham Priest wanted to obtain his Ph.D. in philosophy, rather than in mathematics, as would normally have been the case in my department. I told him that I'd look into the matter, suggesting that a compromise solution might be to call it "philosophy of mathematics." Somehow Imre got wind of this, and as a result I received a midnight phone call from the great man, in which he spluttered, with his Bela Lugosi accent, "I am ze philosopher of ze mathematics at ze LSE and you have no status in awarding degrees in philosophy." So Graham had to be content with a doctorate in mathematics. This, fortunately, did not prove an impediment to his later illustrious career as a philosopher.

It was through Imre – as editor-in-chief of the British Journal for the Philosophy of Science – that I came to write my first book review, of S.W.P. Steen's *Mathematical Logic: with Special Reference to the Natural Numbers*. Steen had acquired a legendary reputation amongst Cambridge mathematics undergraduates for the baffling obscurity of his lectures. Although I had never attended Steen's lectures myself, I had seen the arcane examination questions, expressed in his own peculiar notation, it was his habit to set in Part III of the Cambridge Mathematics Tripos. These would typically take an arcane form such as *Show that $\exists! d \forall Y \in L \mathcal{S}$* . While at Cambridge Imre had attended Steen's lectures, and the experience still rankled. He required from me less a review of Steen's book than a demolition job. Although I could not disagree with Imre that Steen's tome, of some 640 impenetrably dense pages, was hardly a masterpiece of exposition, I was

unwilling to assume the role of hatchet man, and so I turned in a brief neutral review à la *Mathematical Reviews*. Imre was dissatisfied with my effort, claiming that it was too short, by which I knew he meant that it was insufficiently critical. To satisfy him, I returned to the drawing board and produced a piece peppered with references to the book's shortcomings - which were, truth to tell, all too easy to find. I vowed that in future I would review only books that I could praise without reserve, a promise I have kept.

The rift between Imre and Karl Popper was underlined in an amusing way. After Imre's death Mike Hallett was commissioned to catalogue his extensive collection of philosophical books, which his partner Gillian had bequeathed to the LSE library. This involved Mike's making a number of trips to Imre's Hampstead flat, where the books were assembled on shelves stretching from floor to ceiling. These shelves were supported by uprights, a number of which had been camouflaged to resemble the spines of books. According to Mike, the section devoted to Popper contained a support bearing the title *The Open Society, by One of Its Enemies*.

Imre died suddenly in 1974, at the early age of 51. With his passing the colour faded out of the LSE philosophy department.

I first got to know the Argentinian logician Max Dickmann in 1969, following the Logic Colloquium in Manchester. At that time we had a shared interest in infinitary languages, which in Max's case was to culminate in a book *Large Infinitary Languages*, published in 1975. Early in 1972 Max took up a position in the mathematics department of the Catholic University of Santiago in Chile. The election not long before of Salvador Allende's socialist party had excited real hopes that a peaceful transition to socialism there might be in the offing. George Wilmers and I were keen to visit the country, and the fact that the Allende government had vastly expanded the budget for universities had made Max sanguine that he might be able to arrange positions for us in his new department. Soon after his arrival in Chile Max confirmed that positions could be arranged for both of us, George to come the following year and myself the year after. But our plans were dashed by the military coup of September 11, 1973, in which General Augusto Pinochet unleashed a fascism of extreme ferocity on Chile. In the ensuing holocaust thousands of people were rounded up; many later vanished without trace. It was fortunate for Max that he happened to be out of the country at the time of the coup, for he would almost certainly have been among the "disappeared". And George, too, was lucky in this regard. Max had fixed a position for him in Santiago, which he was to take up in September 1973. George had arranged leave from his department in Manchester for the coming academic year, and was all set to take off. In the last week of August my wife Mimi and I threw a farewell party for him at which we toasted his impending departure with a resounding "Next year in Santiago!" George was virtually about to board his flight to Chile when news of the coup broke. Had that horror taken place just a few days later George might well have wound up in the league of the vanished.

Max had been rendered jobless after the Chilean coup and my wife Mimi and I offered him the use of our spare room as a London base while he searched for a new position. During the weeks he lodged with us we spent many evenings in heated political and mathematical discussion, deepening my feelings of camaraderie with him. I came to admire the mental toughness he showed in not allowing the loss of his job and the obliteration of his political hopes to demoralize him. But of course he still had mathematics – Max was, and is, one of the most dedicated mathematicians I have ever met.

Max later took up a position in Paris with the CNRS, from which he has now officially retired. But this fact has had no effect on his mathematical activity, which remains undiminished.

I think it was in 1970 or 1971 that I met, on a visit to Cambridge, the outstanding functional analyst David Fremlin. I recall being highly impressed by his superior technique as a mathematician. We collaborated on two papers ([4], [5]) both concerned with a pet topic of mine, equivalents of the axiom of choice.

In 1971 I became involved in what for me remains my most stimulating collaboration with my fellow-logicians. This was the organization of the Bertrand Russell Memorial Logic Conference, an initiative launched by a number of us opposed to NATO financing of conferences in mathematical logic.

During the 1960s a number of British logic conferences had received funding from NATO, thus becoming officially identified as "NATO Advanced Study Institutes". The funding of scientific conferences by military organizations such as NATO seems to have gone more or less unquestioned until in 1969 a public protest against such financing was mounted at the NATO supported logic conference held in Manchester. (I was not a registered participant at the Manchester conference, but I attended the meeting for a couple of days, staying with Peter Aczel.) The resulting declaration, which concluded with the phrase *we believe that scientific conferences should not be linked with organizations of this [i.e., NATO's] character*, attracted nearly 40 signatures.

But this protest was ignored, and early in 1971 it emerged that the international logic conference to be held in Cambridge that summer was again to be funded by NATO. A small group of logicians, Max Dickmann, Moshe Machover, Alan Slomson, Yoshindo Suzuki, George Wilmer and myself, resolved to launch a stronger protest. We thought that maximum impact would be achieved by staging a counter-conference timed to coincide with the Cambridge meeting. In promulgating our meeting we insisted that mathematicians should take seriously the social implications of their activity and that accepting money from military bodies such as NATO is intellectually and morally incompatible with this aim.

We thought it would be fitting to dedicate the conference to the memory of Bertrand Russell, who had died the previous year. It was felt that Russell, old radical that he was, would have been sympathetic with the anti-military aims of our gathering. We approached the Russell Foundation and received its support.

The conference duly went ahead and was held in Uldum, Denmark in August 1971. There were sixty or participants, including Peter Aczel, Jane Bridge, Anders Kock, Bill Lawvere, Per Lindström, Per Martin-Löf, Bill Tait, Janos Onyskiewicz (a logician who, twenty years later, was to become the Minister of Defense in the immediate post-communist Polish government), Graham Priest, Jan Smith, Ross Skelton, Aldo Ursini and Peter Clark. Given Lakatos' enthusiastic support of everything NATO stood for, I expected him to forbid the students in his department to attend the meeting. To my surprise, however, he issued no such interdict, and three of his students actually did participate.

During the conference I had brought to Per Lindström's attention some problems on a logical system I had introduced in my thesis: weak second-order logic with variables ranging over elementarily definable sets. While I had not been able to solve these problems, Lindström, an exceptionally talented logician, formulated elegant solutions to them which he wrote up and contributed to the published conference Proceedings ([9]).

The Russell Conference's mix of mathematics and socio-political themes was likely unique. It was also modestly successful in achieving our acknowledged goal of preventing future NATO financing of logic conferences.

We were extremely fortunate in securing the presence at our conference of Alexander Grothendieck, then in his early forties, arguably one of the greatest mathematicians of the 20th century, and who had, several years previously, become an anti-establishment radical. I was struck, as were many of us at the conference, by Grothendieck's sincerity and luminous intensity. With his shaved head and simple attire, he had the appearance of a Buddhist monk; from his talk one quickly became aware that one was in the presence of a man with a remarkable moral power. Himself an offspring of left-wing political radicals, Grothendieck's own political radicalization in the 1960s had led him to form the group *Survivre*, dedicated to combating the ideology of scientism, the elevation of science into the "religion" of modern society. His pacifism had led him to resign his position at the *IHES*, the French counterpart of the Institute for Advanced Study, when he learned that it was being partially funded from military sources. He was currently embroiled in a dispute at the Collège de France over his proposal to conduct a course on scientism. An active opponent of the American intervention in Vietnam, he had spent several months in Hanoi under sustained aerial bombardment while teaching at the city's "subterranean university". He gave a vivid account of his experiences during one of the evening discussions which took place at the conference; he also introduced a discussion on scientism and gave a lecture on his work in algebraic geometry. While the actual content of his talk was largely over my head, I was greatly impressed by the spellbinding manner of its delivery. I recall that, after speaking nonstop for two hours without notes, he paused, extracted from his shirt pocket a slip of paper scarcely larger than a postage stamp, glanced at it as if to remind himself of something, returned it to his pocket, and then resumed lecturing for another couple of hours. He was very approachable and talked to everybody; in particular I had a number of animated conversations with him. He described his early life, remarking that he had been in an internment camp in France as a boy during the war. At one point our conversation took a musical turn and he confided to me that his favourite recording was Jascha Heifetz's version of the Bach solo violin sonatas, which happened to be my favourite also. One master acknowledging two peers, I remember thinking at the time.

As a mathematician I felt Grothendieck was parsecs beyond me, but he never betrayed the slightest hint of condescension in that regard.

During the session on scientism Grothendieck read a preliminary draft of an editorial, entitled *The New Universal Church*, for a forthcoming issue of his group's publication *Survivre et Vivre*. This excited considerable response, both pro and —from some "hard left" brethren—con. It must be admitted that the latter had a point: by comparison with his unexampled mathematical sophistication, Grothendieck's political attitude was perhaps somewhat naïve. Later I came to compare his "naivety" with that of Tolstoy, an unwavering refusal to compromise, white-hot in its intensity. It also seems to me that the *Survivre* group can be seen as a forerunner of the ecological movement and the Green Party, which of course itself encountered a good deal of opposition from the communists in its struggle for emergence.

In 1972 Dana Scott arrived in Oxford to take up the position of Professor of Mathematical Logic. I had recently become interested in his and Robert Solovay's approach to independence proofs in set theory via Boolean-valued models. I had given a lecture course at the LSE on the topic, based on Scott's unpublished notes on the subject. I had heard that Scott was about to launch the *Oxford Logic Guides*, a new series of logic

books with the Oxford University Press, and I contacted him with the suggestion that, if he himself was considering his notes as a book, perhaps he might be interested in publishing my account of the subject in the *Oxford Logic Guides*. I was very pleased to receive an enthusiastic reply from him. This resulted in my returning to Oxford a number of times to consult with him on the progress of I was making with my book. I recall these meetings with pleasure. The book ([7]), to which Scott generously contributed a Preface, was published in 1977, and is still in print.

In 1974 I received an invitation to speak at a conference on set theory and hierarchy theory to be held the following summer in the small resort town of Bierutowice, located some 60 miles from Wroclaw in the mountains of south-west Poland. Soon after this an invitation arrived from the Polish Academy of Sciences to visit Warsaw and meet some of the logicians there. Just before the conference was due to begin in September 1975, Andrzej Mostowski died suddenly on a visit to Canada. The organizers of the Wroclaw conference quickly decided to dedicate their meeting to Mostowski's memory.

Alan Slomson had also been invited to speak at the Bierutowice meeting, and we decided to travel there together by train from London. The journey there, which passed through East Germany, was long and tedious, especially once we had crossed the Polish border, where the train slowed to a crawl and even went into reverse for an alarmingly extended period. But eventually we arrived in Wroclaw, where a bus was waiting to take us to Bierutowice. The meeting lasted 10 days or so and was both stimulating and enjoyable. I was delighted to see John there. I met a number of Eastern European logicians there, including the Polish logicians Wojciech Guzicki, Henrik Kotlarski, Stanislaw Krajewski, Wiktor Marek, and Marian Srebrny, as well as the Czech logicians Petr Vopenka, along with his students Bohuslav Balcar and Petr Stepanek. Also present was the German set-theorist Ulrich Felgner. Vopenka's presence at the conference was unusual. As a prominent supporter of the liberalization introduced by the erstwhile Czech leader Alexander Dubcek, he had fallen into disfavour with the regime (re-) installed in Czechoslovakia after the Warsaw Pact invasion in 1968, and as a result his contacts with foreign mathematicians was strictly limited by the political authorities. He seemed very pleasant, but the fact that he did not speak English (even though apparently understanding it) prevented me from having any close contact with him. I was accordingly surprised when, at the end of the talk (on models of set theory) I gave at the meeting (later published as [6]) he stood up and proceeded to make a number of comments which one of his students in the audience orally translated into English on the spot. I recall being worried that that his remarks might prove to be to the effect that the result I had just presented was wrong, trivial or, at best, already well-known. So I was relieved when (through his interpreter) he said that he found my result "interesting", and that my argument could be used to prove a sharpened version, which he later outlined for me. I also recall a number of animated conversations I had with his students Balcar and Stepanek, to whom I outlined some of my ideas on Boolean-valued models of set theory.

When the conference ended, I travelled on to Warsaw, to take up my invitation to visit the Polish Academy of Sciences. I was accompanied by Marian Srebrny, who had been delegated to be my guide in Warsaw. To get there we boarded a night train in Wroclaw, bulging with passengers in numbers so much in excess of the supply of seats that neither of us was able to obtain one. We stood in the carriage corridor all night. But Marian proved to be an engaging travel companion, and his command of English enabled us to while away the hours in animated conversation. When we arrived in Warsaw early the following morning Marian took me to the hotel in which I had been billeted. This turned out to be a fancy establishment whose typical guests were besuited Western businessmen. After a sleepless night on a crowded train, Marian and I

arrived in a somewhat dishevelled state, and lacked both suits and ties, causing the desk clerk at the hotel to eye us dubiously. He was politely incredulous when Marian introduced me as “Professor Bell, who has a reservation here made by the Academy of Sciences.” But a consultation of his reservation book and a checking of my passport finally overcame his doubts sufficiently for him to summon a bellhop to escort me to my room, which, happily, proved to be most comfortable.

Marian shepherded me around Warsaw and introduced me to a number of logicians, who one and all made me welcome. I was particularly pleased to meet Helena Rasiowa, whose book *The Mathematics of Metamathematics* (written with Roman Sikorski) had been a formative influence on my own work. I recall also an enjoyable lunch I had with Henryk Kotlarski and his wife and young son.

The Polish Academy of Sciences was housed in the Palace of Culture and Science, a gargantuan edifice with a high tower occupying the centre of Warsaw. It had been a “gift” to the Polish people from the Soviet Union after the Second World War and was built in the ornate and oppressive Stalinist architectural style. I learned that it did not enjoy much popularity locally. This was brought home to me by one of the logicians (I cannot now recall who) I met when visiting the Academy. He asked me if I knew why the view of the city from the top of the Palace of Culture’s tower was regarded as one of the best in Warsaw. I ventured to suggest that the tower’s height must make the view from it quite panoramic. He said, “yes, of course. But the real reason is that it offers the only vista in Warsaw where the Palace of Culture itself cannot be seen.”

My departure from Warsaw at the end of the week occasioned a minor drama. I had booked a seat on a westbound train to the Hook of Holland which was scheduled to depart at midday on the Friday. Several of my newfound friends had arranged to meet me at a cafe for a drink before my departure. By 11 a.m. I thought we should be leaving for the station, but my friends assured me that it would take no more than a few minutes to get there and that there was no need to rush. But when we all showed up at the station we found that the schedule had been unaccountably changed and the train had already departed. We thought that there might be a later train that day going westward but on enquiring at the ticket office the extremely unhelpful official in attendance stated categorically that no train connecting to the West would be available before Monday. While my Polish friends would have been happy to put me up for a few extra days, my Polish visa expired the following day and I did not want to go through the rigmarole of extending it. We were about to leave the station and make new arrangements for my accommodation when Wiktor Marek suddenly showed up to bid me a late farewell. He told us that as he entered the station he saw a noticeboard listing the departure times of the day’s trains, and had noted that the train I had intended to catch was scheduled to depart an hour early. But he had also observed that a train to the West originating in Moscow was due to stop in Warsaw at midnight. The ticket official had made no mention of this fact. My friends, familiar with the vagaries of the Polish railway system, told me that I might contrive to board this train by bribing its conductor on its arrival in Warsaw. I resolved to do this, and, relieved, we returned to the cafe to reignite the farewell party. I duly returned to the station that night and succeeded in boarding the promised train in the manner my friends had suggested. So all ended well.

I was sorry that my visit to Warsaw had been so brief. Sadly, I have not returned since.

Starting in the early 1970s, Moshe Machover and I both contributed courses to the Master of Science program in logic and foundations of mathematics at London University. Moshe gave the majority of these courses and to begin with I contributed just a course on Boolean algebras and model theory. Later I

expanded my offering to a course on axiomatic set theory, which I based on the elegant lecture notes on the subject that Moshe had written up, in his characteristic bold hand, some years before. One day Moshe suggested that we should collaborate on working up our course notes into a book on mathematical logic, covering all the major branches of the subject, giving the student what we were pleased to call a “balanced diet”. I was flattered by Moshe’s invitation to work with him on the project, since I knew that, as a superb expositor, he was more than capable of producing an excellent book on his own. But I also knew how much he enjoyed collaborative work and active intellectual engagement with others, as exemplified by the memorable “Sunday Schools” he regularly held at his house in which each of the participants would present a paper on a topic related to the foundations and philosophy of mathematics.

Before embarking on this project, Moshe had, with typical foresight, prepared a typographical legend for the numerous mathematical symbols he planned to use in the typescript of the text. This specified various coloured underlines and other devices to indicate the intended type face of a letter, for example brown for italic and green for bold. These we carefully inscribed in the finished typescript which, in those remote days before the arrival of word processors, had been skillfully prepared by no less than four secretaries in our respective university departments. When the proofs for the book arrived, Moshe proposed checking them collaboratively. He suggested, sensibly, that one of us read the typescript out loud and the other follow it on the printed proof. In reading out the typescript it was also necessary, as in Victor Borge’s “phonetic punctuation”, to read out the various typographical devices Moshe had devised. This resulted in absurdities such as “ $T \rightarrow P$ ” being enunciated as “brown tea implies green pea”, which occasioned much mirth. Since the book was upwards of 600 printed pages in length, we had to devote numerous evenings to this curious activity, but it was worth it since the finished product proved to contain remarkably few typos and misprints. Thanks to Moshe’s careful planning, our book, *A Course in Mathematical Logic* ([8]) is still in print after 45 years. My pride in being part of this project remains undiminished.

In May 1978 I was invited by Ken McAloon, the breezy, brilliant American logician then heading the *Equipe de Logique* at Université de Paris 7, to speak at a Logic Colloquium he had organized at the *École Normale Supérieure*. While in Paris we stayed in Max Dickmann’s apartment. The night before the meeting was to take place Max hosted an uproarious dinner attended by various logicians including Alastair Lachlan, Angus Macintyre and, of course, Ken. Wine and talk flowed freely. I recall Ken announcing that the venerable American logician W. v. O. Quine (then visiting Paris) would be speaking at the Colloquium. “In fact,” he went on to say in his droll way, “I invited all three of them, William, van Orman and Quine.” He also warned us that Jean Dieudonné, the formidable “scribe of Bourbaki” and a sworn enemy of logic, would be present at the meeting. According to Ken, it was common knowledge among Paris logicians that Dieudonné attended Logic Colloquia solely in order to deride the subject and taunt its practitioners. The Paris logicians had learned not to take Dieudonné’s interventions very seriously.

The following morning I gave my talk (entitled *Isomorphisms of structures in S-toposes*) without eliciting any comment from the audience. Dieudonné was present as predicted. I had not seen him before but he was unmistakable: a big burly man with a stentorian voice; dressed in a blue suit, he struck me as being perfect for the role of the Marseille police chief in *The French Connection II*. In the discussion following the talks, Dieudonné launched into his characteristic anti-logic diatribe: logic is a mere “prophylactic” and mathematically sterile, logicians were not “proper” mathematicians, etc. This last observation provoked some members of the assembly into protesting “But what about Gödel?” Dieudonné’s response to this was cheeky, but memorable: “Gödel wasn’t a mathematician, but a *metamathematician*.” As Quine reports below

(and I recall the exchange), on being challenged to explain why, if he despised logic, he was present at a meeting of logicians, Dieudonné coolly observed “I was invited”.

After the meeting a cocktail party for the participants was held in one of the larger rooms of the ENS. Ken had arranged for a copious supply of Martinis to be laid on and as a result everyone mellowed quite rapidly. Dieudonné was not present (at least I didn't see him) but Quine was. At some point he and I fell into conversation. He was affable, and, as I expected, an engaging conversationalist. It was clear that he also enjoyed the martinis (as did I). At some point our discussion turned to quantum logic, a subject in which I had just begun to take an interest. I was startled when he suddenly asked me if I was John S. Bell, the famous quantum physicist and inventor of the Bell inequalities. I told him that alas, I was not, but flattered to be confused with the great man. I'm not sure if Quine ever figured out who I was.

In his autobiography ([14]) Quine records his impression of the meeting:

In 1978 a Logic Colloquium was afoot in the École Normale Supérieure. .. Dieudonné was there, a harsh reminder of the smug and uninformed disdain of mathematical logic that once prevailed in the rank and vile, one is tempted to say, of the mathematical fraternity. His ever hostile interventions were directed at no detail of the discussion, which he scorned, but against the enterprise as such. At length one of the Frenchmen asked why he had come. He replied “J'étais invite”.

I don't recall Quine expressing any such opinions in the course of our conversation.

In the 1960s the mathematics department at the University of Bristol had, through the efforts of its Chair, the mathematical logician John Shepherdson, become the centre of mathematical logic in Britain. He recruited a number of logicians, including Fred Rowbottom, Brian Rotman, and John Mayberry. Fred was a man of few words but deep thoughts who had made a reputation for himself through his work on set theory in the early 1960s. Later he developed an interest in topos theory. He had a couple of gifted graduate students in this area, John Zangwill and Jonathan Chapman, for both of whose Ph.D. dissertations I served as external examiner. I was especially impressed with Zangwill's elegant development of the internal language of a topos which he termed *local set theory*. I later used his formulation as the basis for my own book on topos theory. Fred and Chapman later collaborated on a book on topos theory themselves.

I got to know Brian Rotman well in the early 1970s. He was a regular participant in Moshe's “Sunday Schools” and his contributions, presented in his questioning, quicksilver way, were always stimulating. They reflected his wide cultural interests, which embraced linguistics, semiotics, film, theatre and postmodernist philosophy. He had become especially interested in viewing mathematics through the lenses of semiotics and linguistics, and at the Sunday School he presented a couple of papers on this topic. These were later to flower into his influential books *Signifying Nothing: The Semiotics of Zero* and *Ad Infinitum... The Ghost in Turing's Machine* (the latter of which I reviewed for *Philosophia Mathematica*). After 20 years in the Bristol department he left it to work as a freelance writer and playwright. In 1990 he and his wife, the American theatre director Lesley Ferris, emigrated to the United States where he eventually became a distinguished professor in the Department of Comparative Studies at Ohio State University.

I did not meet John Mayberry until the middle 1970s but we got to know one another well. A native of Illinois, he had completed a Ph. D. there under Gaisi Takeuti. In 1966 he joined the Bristol department and remained there until his retirement in 2004. It came as a shock when he was stricken by the cruel stroke

from which he died in 2016 at the age of 76. I contributed the following eulogy which was read out at his funeral.

I remember John with great affection. It was always a keen pleasure to sit down and talk with him into the night about everything under the sun - and far beyond. His largeness of mind embraced literature, music, art, history, philosophy and last but hardly least, mathematics - all were grist to his conversational mill.

I cherish the memory of the week I spent with him in Bristol in 2010 - the last time we passed in each other's company. This was soon after my wife Mimi (whom he knew well) had died and I was searching for consolation and diversion. John provided both, in abundance. Having lost his own wife a few years before, he was familiar with the pain I was trying to assuage and he quickly moved to relieve it with humour, for example by describing, with his characteristic rueful wit, the two of us as " a pair of grumpy old men on displeasure bent".

There was something appealingly old-fashioned about him. His very name, John Penn Mayberry, possessed a certain dignity which seemed of a piece with his gentlemanly American manner and the care he always took in expressing himself. His wit was sharp, occasionally self-deprecating, ever ready to puncture the ridiculous, but never cruel. It was as if Mark Twain, still retaining his birth name Samuel Langhorne Clemens, had somehow stepped into the pages of Henry James.

He was quick to ridicule what he saw as pretension, but generous in his praise of those whose intellectual attainments he admired, even when he disagreed with their conclusions, or, more importantly for him, the assumptions from which those conclusions flowed.

As a mathematical logician, John had little interest in proving flashy theorems. His goal, from which he never swerved, was to identify the basis on which mathematics - and logic - ultimately rests. He had no truck with infinite regresses and spurned the idea that the world is just "turtles all the way down". He wanted to know where, exactly, the intellectual buck stops. Philosophically, he was the very opposite of a formalist. I was made aware of this time and again in our conversations when he would gently, but persistently, attempt to persuade me that formalism and abstraction (which have a certain appeal for me) could have no content without a properly formulated solid foundation. As a philosopher of mathematics he was, in spirit, Fregean rather than Hilbertian, even though he was critical of some of the details of Frege's analysis of number.

I venture to think that John would be amused, but not displeased, to be described as a gentleman and a scholar. His passing leaves a hole that can never be filled. I shall miss him greatly.

And indeed I do.

One of my colleagues at the University of Western Ontario was the distinguished philosopher of science William Demopoulos. We wrote a couple of joint papers. I reported on our collaboration at a memorial meeting held in his honour soon after his death in 2017.

In his contribution to my 2011 Festschrift "Vintage Enthusiasms" Bill recalled:

I retain fond memories of both collaborations; they proceed quickly and almost effortlessly.

When I first read this I was touched, since I, too, retain fond memories of our collaborations. Their rapidity and effortlessness were, in retrospect, all the more remarkable given that both of us were men of strong opinions who might

quickly have clashed. For his part Bill was not overawed by mathematicians (myself in particular) or indeed anybody else. I shall always recall his withering assessment of anyone who failed to impress him philosophically: He doesn't have a philosophical bone in his body. I hoped that he did not, privately, hold that opinion of me. But I confess I was never certain. Be that as it may, our collaborations were pleasing to both of us and resulted in some satisfying joint publications:

- (1) *Frege's Theory of Concepts and Objects and the Interpretation of Second-Order Logic*, Philosophia Mathematica, (3), 1, 1993.
- (2) *Elementary Propositions and Independence*, Notre Dame J. of Formal Logic, 37, No.1, 1996.

Before coming to Western I had not paid much attention to Frege but soon after my arrival Bill got me interested in Frege's work, in particular in "Frege's Theorem", the result implicit in the Grundgesetze that Hume's Principle implies the existence of a model of Peano's axioms for arithmetic.

Bill's shift of attention from the foundations of physics to Frege and the foundations of mathematics had begun some time before I showed up at Western, and it was his enthusiasm that initiated paper (1), in which we argued against Hintikka and Sandu's claim that Frege lacked the concept of a function as an arbitrary correspondence, resulting in a restriction in his conception of second order logic. In our paper, we claimed that Frege's conception of a function in fact coincides with the set-theoretic notion of an arbitrary correspondence, in which case the range of Frege's function variables is in one-one correspondence with the power set of the domain of individuals. Our thought was that whatever covert role the neglect of Cantor's theorem might have played in the inconsistency of Grundgesetze, it was unlikely that Frege sought to ignore that theorem by assuming that the totality of functions, like the totality of expressions, is countably infinite.

I recall Bill's ridicule of (he could be very scathing) the title of Hintikka and Sandu's paper The skeleton in Frege's cupboard: the standard versus nonstandard distinction. Bill scornfully dismissed the notion of a "skeleton" in Frege's cupboard and gleefully quoted the old nursery rhyme

Old Mother Hubbard, who went to the cupboard,
to get her poor dog a bone;
But when she got there
The cupboard was bare,
And so the poor dog had none.

Bill proposed that the middle lines would serve as a suitable epigraph for the paper, and so it appears.

Paper (2) originated in the following way. I had been thinking about Wittgenstein's "elementary propositions" in the Tractatus and had the "brainstorm" that these could be seen as free independent generators of arbitrary propositions in the algebraic sense. (Thus I was bold enough to claim that Wittgenstein had implicitly introduced the concept of a free Boolean algebra.) I mentioned this to Bill, who quickly agreed and suggested that we collaborate on a paper analyzing various notions of independence arising in connection with elementary propositions in the philosophy of logical atomism.

The paper was quickly written –almost effortlessly, indeed. But getting it published was another matter. Bill had had much more experience than I in publishing philosophy papers (almost all of mine up to that point having been in mathematics), so he took on the responsibility of submitting our paper to a suitable philosophy journal. Bill first submitted it to Mind which quickly rejected it, giving the curious explanation that, although our paper may have had some merit, Mind’s policy was to reject 9 papers out of 10, and ours was one of the 9. Mind’s policy was , in short, to reject 9 papers out of 10 before submission. This struck me as being of an absurdity comparable with the injunction on a top secret document to destroy it before reading. I tried to make light of the affair by quoting:

What is Mind?

No Matter.

What is Matter?

Never Mind.

Still, the rejection was irritating, especially to Bill who had had an excellent track record in getting his philosophy papers accepted for publication.

Bill next submitted the paper to Nous, whose then editor happened to be an acquaintance of his. After receipt of the paper had been acknowledged, months went by with no word from the journal’s editor. Every time I ran into Bill I would ask if there had been any News from Nous, and on learning that there was none, remark “Is no news from Nous good news from Nous, or is it bad news from Nous?”. Finally a letter from the editor arrived informing us that he had received two reports from the referees, one enthusiastically recommending publication and the other recommending rejection. The second referee’s reasons for rejecting our paper did not seem very substantial, one of them being merely that we had failed to refer to his work on Wittgenstein. On this slender basis the editor r concluded that he had “no choice” but to reject the paper. Bad news from Nous indeed! Bill became absolutely furious at this, calling the Editor “craven” and “pusillanimous” in bowing to the opinion of the second referee, who Bill was convinced was a hatchet-man of the all-powerful Wittgenstein industry whom the Editor did not wish to offend. He may well have been right.

The affair did have a happy ending, however. We decided finally to submit the paper to the Notre Dame Journal of Formal Logic where it was quickly accepted for publication.

Our collaboration was great fun, and I have not experienced its like since.

It is with both affection and sadness that I recall my colleague Rob Clifton (1964- 2002). As a young up-and-coming philosopher of physics he joined the department at Western in 1993 and we quickly became friends. He and his wife Marilyn, along with their two young children, moved into the house immediately opposite ours and the friendship between my family and his was facilitated by our proximity. He was a gentle giant of a man, and positively brimmed with enthusiasm. We had numerous discussions about physics, which led eventually to our collaborating on a joint paper, *QuasiBoolean Algebras and Simultaneously Definite Properties in Quantum Mechanics*. He was also a committed Christian, active in the Anglican church. He saw no conflict between his religious faith and his devotion to science, a position I respected despite my confirmed nonbelief in established religion. Rob’s tolerant attitude to those who did not share his religious beliefs was attested by his amusement at my referring to myself in our conversations as “your friendly neighbourhood atheist”. In 1997 he was offered, and accepted, a position as Associate Professor of Philosophy at the University of Pittsburgh. I was sorry to see him go, but happy that his move represented a significant step forward in his academic career. It came as a terrible shock when, after five short years at

Pittsburgh, he died of cancer at the age of 38. When I was informed of this I did not derive much solace from the ancient aphorism *Those whom the gods love die young*, but I was later consoled to learn that Rob's religious faith had helped him to face his impending death with equanimity.

I conclude with a reminiscence of the outstanding philosopher of science Hilary Putnam.

John Mayberry once remarked to me that good mathematicians occasionally have failures of insight. He had spotted an amusing example of this in Hilary Putnam's well-known paper *Mathematics without Foundations*. In that paper Putnam states:

If the whole physical universe is thoroughly finite, both in the large and in the small, then the statement " $10^{100} + 1$ " is a prime number" may be one whose truth value we can never know.

John pointed out, with considerable glee, that it's easy to see, and Putnam should have seen it, that simple algebra shows that $10^{100} + 1$ (googol plus 1) is not prime. For from the algebraic fact that $x^5 + 1$ is divisible by $x + 1$ and the fact that $10^{100} + 1 = (10^{20})^5 + 1$, it follows that $10^{100} + 1$ is divisible by $10^{20} + 1$. John wondered whether this fact had later occurred to Putnam, or whether it had been pointed to him by any of his colleagues. It only struck me years later that John's glee at Putnam's slip was probably also fueled by the very title of the offending paper, *Mathematics without Foundations*. For John, mathematics without foundations was a kind of absurdity, a notion not to be taken seriously.

I had never met Putnam, but I had long admired him. As a mathematical logician I was aware of his contributions to the (negative) resolution of Hilbert's 10th Problem. I had also been amused and impressed by his appearance on the 1978 BBC television program *Men of Ideas* in which various philosophers (including Marcuse, Chomsky, Quine, Ayer as well as the honorary "man", the novelist and philosopher Iris Murdoch) engaged in conversation with Bryan Magee, a principled but somewhat pedantic English philosopher. To create an atmosphere of informality, the participants were seated at the ends of a long sofa. In striking contrast with Bryan Magee's earnestness, I found Putnam's easy, laid-back style agreeable, entirely compatible with the comfortable appearance of the sofa on which he relaxed so casually.

Years later it happened that Putnam was invited to speak at a conference organized in honor of my colleague Bill Demopoulos to be held at Western. I had been invited to the reception arranged for the speakers, and Bill, who knew Putnam quite well, was happy to introduce me to him. I figured that our impending meeting presented an ideal opportunity for me to ask Putnam whether he had recognized that googol plus one is actually composite. Reflecting on how I should put this question to him, it occurred to me to try to identify some smaller factors of googol plus one. John had already observed that it is divisible by $10^{20} + 1$. Then it struck me that since $10^{20} + 1 = (10^4)^5 + 1$, $10^{20} + 1$ is itself divisible by $10^4 + 1$, i.e. 10001. Lacking the numerical insight of a Ramanujan, I could not immediately tell whether 10001 was composite, but I soon found that it is in fact the product of 73 and 137. I recalled with delight that 137 was the English astrophysicist Eddington's magic number, which figures prominently in the neo-Pythagorean cosmology expounded in his *Fundamental Theory* and that Eddington's obsession with it is satirized in Bertrand Russell's *The Mathematician's Nightmare*.

Armed with this revelation, I felt that when I finally met Putnam, I would not merely be posing John Mayberry's original question but would have something novel to say. After Putnam and I were introduced

at the reception, I soon launched into the matter. I first asked him whether he recalled his remark about googol plus one in his old paper, to which he responded in the affirmative, and then whether he had later become aware that the number was actually composite. Somewhat surprised, probably, at the interrogational turn our conversation was taking (and also likely wondering whether he had been introduced to a lunatic), he replied that, no, he hadn't. I then went on to present my *pièce de résistance*, the procedure showing that googol plus one is divisible by Eddington's magic number. I shall never forget Putnam's witty exit line, *Well, you can't win 'em all!*

References

- [1] Bell, J. L. *Perpetual Motion: The Making of a Mathematical Logician*.
<http://publish.uwo.ca/~jbell/perpmonew4.pdf>
- [2] . Bell, J.L, and Slomson, A,B. *Models and Ultraproducts: An Introduction*. North-Holland, Amsterdam, 1969. Reprinted by Dover, 2006.
- [3] Bell, J. L. and Jellett, F. *On the Relationship between the Boolean Prime Ideal Theorem and Two Principles of Functional Analysis*, Bull. de l'Acad. Pol. des Sci., XIX, No.3. , (1971) 191-194.
- [4] Bell, J.L. and Fremlin, D. H. *A Geometric Form of the Axiom of Choice*, *Fund. Math.* 77, (1972). 167-170.
- [5] Bell, J.L. and Fremlin, D. H. *The Maximal Ideal Theorem for Lattices of Sets*, *Bull. London Math. Soc.*, 4, (1972), 1-2.
- [6] Bell, J.L. *Uncountable Standard Models of ZFC + V = L*, in *Set Theory and Hierarchy Theory, a Memorial Tribute to Andrzej Mostowski*, Springer Lecture Notes in Math. 537,(1976) , 29-36.
- [7]. Bell, J.L. *Boolean-Valued Models and Independence Proofs in Set Theory*. Clarendon Press, Oxford, 1977. 2nd edition, 1985. 3rd edition, 2005. Paperback edition, 2011.
- [8] Bell, J.L. and Machover, M. *A Course in Mathematical Logic*. North-Holland, Amsterdam, 1977. 4th printing, 2003.
- [9] Bell, J.L., Cole, J., Priest, G. and Slomson, A. (eds.) *Proceedings of the Bertrand Russell Memorial logic Conference*, Uldum, Denmark, 1971, published in Leeds, 1972.
- [10] Crossley, J. N (ed.) *Sets, Models and Recursion Theory, 1965 Summer School and Colloquium in Mathematical Logic*, Amsterdam, North-Holland, 1967.
- [11] Hodges, W. (ed.), *Conference in Mathematical Logic, London 70*, Lecture Notes in Mathematics, vol. 255, Springer-Verlag (1970)
- [12] Kreisel, G. *Choice of infinitary languages by definability criteria: generalized recursion theory*. In Barwise, J. (ed.) *The Syntax and Semantics of Infinitary Languages*, Springer Lecture Notes in Mathematics, vo. 72, Spinger, Berlin, Heidelberg, 1968.

[13] Myhill, J. and Shepherdson, J.C. *Effective operations on partial recursive functions*. Z. f. Math. Logik u. Grund. d. Math. 1 (4) (1955), 3120-317.

[14] Quine, W. v. O. *The Time of My Life*, M.I.T. Press, Cambridge, Mass. 1985