What is mathematical logic? An Australian odyssey

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Abstract

John Crossley settled in Australia in 1968 having been a graduate student and later University Lecturer at Oxford. This is a brief account of his logical career. It is a revised version of a webcast talk for World Logic Day on 14 January 2022.

1 Introduction

The book '*What is mathematical logic?*' [10] came about because of Melbourne University philosophers, I should perhaps say philosophy students, wanting to know more about the subject. That was in 1970, 2 or 3 years after I first started thinking about Australia. At that time there were very few logicians in the country; I think just two. I had been in Oxford in a secure position but felt that I could not achieve any further expansion of mathematical logic there because of the entrenched interests. So I started looking elsewhere. Gordon Preston had been appointed to a chair at Monash in 1963 and before that he used to attend the same seminars as me in Oxford. In turn, he invited Peter Neumann for a term in 1967 and he gave a course on mathematical logic.

I first came to Australia in 1968 on a visit and, to cut a long story short, I was elected to a chair in Pure Mathematics then—a post I took up the following year. Just one anecdote: in the interview, where I was sitting on the ostensibly intimidating sole chair at one side of a huge table with the interviewing committee opposite, I was taken to task. Someone offered that mathematical logic was abstract and useless, so I pointed out that Alan Turing was responsible for the British computer. Subsequent events showed how useful logic is.

At Monash at that time there was Richard Routley (later Richard Sylvan) but he was in Philosophy and I did not know him.[1] In 1971 he moved to the Australian National University where he remained. In both Oxford and in Australia the philosophical and mathematical aspects of symbolic logic tended to be largely autonomous. Relations between the two camps have varied over time. Sometimes there has been hostility, at others cordial relations. Richard invited me to Canberra and there Bob Meyer was always a pleasure to be with. When I met Michael McRobbie in 2012, he had long been in Indiana University Bloomington and by that time was president. He recounted fond memories of the 1974 conference of which I shall have much to say shortly. Nevertheless, this article will concentrate on the mathematical side of logic.

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[1] For more on the philosophical side of logic in Australia, see Greg Restall's https://consequently.org/papers/logic_in_australasia.pdf (accessed 28 May 2022).

Vol. 31, No. 6, © The Author(s) 2022. Published by Oxford University Press.

Advance Access published 13 July 2022 https://doi.org/10.1093/jigpal/jzac057

2 Melbourne connections

Having arrived in Australia, and having come from a joint appointment in mathematics and philosophy, I established contacts with the philosophy department at Monash. That led me into contact with Chris Brickhill, a graduate student at Melbourne. Together we planned a series of informal lectures on mathematical logic. His professor, the kindly Douglas Gasking, was terrified we wanted money, but he was reassured that we did not want any, though he seemed to find the idea incomprehensible at first. We convinced him that we only wanted access to a lecture theatre. At that time the four branches of Mathematical Logic were Model Theory, Proof Theory, Recursive Function Theory and Set Theory. Already in the Monash mathematics department we had just the right people to cover most of the four areas. There was one exception: proof theory. Georg Kreisel who had an enormous influence on mathematical logic was a frequent visitor to Oxford when I was there, but one thing he did say, and that was that I should never do proof theory. More on that later.

I had been able to bring my graduate student, Chris Ash, with me as a Senior Tutor and he was appointed as a lecturer in 1973, while Gordon Preston had recruited Neil Williams (who later went to the University of Queensland) and John Stillwell who had managed to get interested in recursively enumerable sets and had been supported by a generous supervisor, Bruce Craven, although he was by no means a logician: Bruce's main interest is in optimization.

We first presented a historical survey of mathematical logic while the remaining lectures covered the Gödel–Henkin Completeness Theorem, Model Theory, Turing Machines and Recursive Functions, Gödel's Incompleteness Theorem and Set Theory. Although individuals were responsible for individual chapters they were considerably reviewed by all members of the team, first after their presentation at Monash and later in written draft.

Ironically, when we presented the lectures at Monash we had a tiny audience, no more than half a dozen, and that was in the afternoon. When we went to Melbourne to give the lectures on a Thursday evening, about eight o'clock I think, we started off with 59 and after 13 weeks, at the end of the course, the numbers were down to about 30. The audience was enthusiastic and stimulating and convivial. I do not remember at which stage we thought we might make a book out of the lectures but we soon approached Methuen and also Oxford University Press (OUP). Both were interested but OUP said they would put it out more cheaply so we continued dealing with them. Our aim was to popularize logic, not to make money. The resulting book was a mere 72 pages.

It appeared in January 1972 when I was visiting Anil Nerode at Cornell. Since then it has remained in print, Dover taking over the publication quite early on.

3 The 1974 Summer Institute

The next major Australian event was in January–February 1974. The Australian Mathematical Society sponsored Summer Institutes and I proposed having one at Monash. One great facilitator was a grant from the vice-chancellor. Although I had been at Monash for a few years, I had never received a honeymoon grant as new professors usually did. I asked Louis Matheson, the tall, imposing but very friendly vice-chancellor for \$10,000 and he said 'Five'. So that was settled in a matter of minutes: the equivalent of \$50,000 today, it went a long way.

The Summer Institute was to last 6 weeks, an unbelievable length nowadays. The first three works were principally on Logic and the last on Applied Maths. It was at this point that I realized that a principal concern of this report is networking. Let me explain.

The ground work for the '74 conference was laid a decade or more earlier. In 1961 I had attended my first logic conference in Cambridge, which was mainly about modal logics. Then, in 1962 I went to the Logic Colloquium in Leeds. Trading on the fact that many top American mathematical logicians were going to the International Congress of Mathematicians in Stockholm, the meeting was organized in Leeds where they might stopover. Alonzo Church, a monumental figure both in logic and physically, attended. Also, there was Ray Smullyan who did conjuring tricks, something that had earned him a crust while he was a graduate student. Arend Heyting, Georg Kreisel and Alfred Tarski all presented invited lectures. I gave my first contributed talk; my thesis was beginning to develop.

With the help and backing of Michael Dummett I argued for the meeting to be in Oxford the following year. Michael knew there was NATO funding available and we secured £900 (about £8,000 or A$14,000 today). Michael suggested various names such as Saul Kripke and Helena Rasiowa. The latter was behind the Iron Curtain but neither the Communists nor NATO objected. In the end she was unfortunately unable to attend and refunded the airfare we had sent to her. Arthur Prior, Bob Bull, Ron Harrop, John Lemmon, Mike Cresswell, Martin Löb, Hans Hermes, Saul Kripke, Mike Yates, Richard Montague, R. L. Goodstein, Kurt Schütte, Hao Wang and I all gave talks. The non-Oxford people were mainly nominated by Michael Dummett but almost everyone invited came.

This was followed by another meeting in Leicester, where Goodstein was professor, in 1965. Although I was not a local organizer, I was heavily involved. By that time I had my doctorate and, since the content of my thesis was closely related to work of Jim Dekker and Anil Nerode, I invited them. But we also had the participation of Gerry Sacks, Hartley Rogers, Jr., Gisbert Hasenjaeger, Ronald B. Jensen, Haim Gaifman, Simon Kochen, Carol Karp, Bill Lawvere, Joan Moschovakis, Gerry Sacks, Alastair Lachlan and Dana Scott. As I have written elsewhere, on meeting Nerode I took an instant dislike to him—and we have been friends ever since, not to mention working closely together for more than 30 years! Americans told me that Hartley Rogers did not fly, but he came this time, though he did not stay long. This time I think all the people who were invited accepted. Moreover, they funded themselves. In addition my first doctoral student, Peter Aczel, gave a talk.

The reason these people were invited is that their work had become known to us one way and another; in particular through the DLMPS of the IUHPS[2] meeting in Jerusalem in 1964, which I had attended and where I first learnt about models and ultraproducts: Simon Kochen talked about them and that ultimately led to John Bell and Alan Slomson's book of that name [1]. There I met and spent pleasant time with Paul J. Cohen and his wife. Cohen's invention of forcing, which he had used to prove the independence of the Axiom of Choice and the Continuum Hypothesis [3], had perplexed a distinguished gathering of logicians at the Oxford 1963 meeting where, after an hour or more's struggle, we were unable to understand what was going on. The groundwork had therefore been laid for invitations for the 1974 conference. I was greatly assisted in the preparations by two of my then research students: Liz Wachs (now Sonenberg) and Bill Gross. I was also very ably supported by the secretarial staff in the Mathematics Department, especially my own secretary, Annemarie Vandenberg. We had help from every single secretary in the department at one time or another. I was not aware at the time of just how much work I was creating for them but Liz later pointed out forcefully that her thesis would have been finished much earlier had it not been for the demands of the conference.

Everyone invited accepted—the list is in the proceedings—except Abraham Robinson. I had delighted in his avuncular company at St Catherine's before I left Oxford but now he had pancreatic cancer and was suffering a great deal of pain. It was reported to me that he was bitterly disappointed not to be able to come to Australia. In fact he died three months later. As part of the strategy to get people to talk at the Summer Institute I had arranged for Nerode to come on a visit before the institute

[2]Division of Logic, Methodology and Philosophy of Science of the International Union of History and Philosophy of Science.

started and for Mike Morley, also from Cornell to come early too. Since this was Mike and his wife Vivienne's first visit to Australia, and Nerode was keen to see some of the outback, we made a trip north getting as far as Mootwingee and Broken Hill. On the way back we got disastrously bogged near the (as we found out later, notorious) Lachlan River but, with the help of fellow travellers, we managed to push and pull all the vehicles to dry land.

The Summer Institute was wonderfully successful. There were more than 250 participants (actually 253)—the majority coming for the first 2 or 3 weeks when the logic was on. Officially, there were three parts to the pure mathematics section of the programme. The Summer School in Mathematical Logic was intended to be instructional for those who had not had the opportunity to study mathematical logic before, but on the other hand we had a Symposium on Recursive Model Theory. This last was a subject that did not exist at the time the Symposium was planned! Anil Nerode and his Greek student George Metakides had started building on the work of Fröhlich and Shepherdson developing recursive algebra[15]: algebra where, instead of being given the structure abstractly, one can explicitly compute values. (Of course this is still very abstract to non-logicians!) I had an interest in the area because of my own work on constructive order types (starting with my thesis) and especially because of later work with Nerode.

The greatest virtue of the Symposium on Recursive Model Theory was that there were no lectures, no tutorials, indeed no formal sessions at all! Absolutely nothing was scheduled. What I did do was to invite people who were interested in the area and put them in the compact environment of Mannix College where people would inevitably meet and talk. The greatest success of all was only made public when the proceedings were published as volume 450 of the Springer Lecture Notes in Mathematics [7]. On the evening of Tuesday 15 January 1974, the following met in my office to reminisce about the rise of mathematical logic: Chen-Chung Chang, Jerry Keisler, Steve Kleene, Mike and Vivienne Morley, Andrzej Mostowski, Anil Nerode and Gerald Sacks. Peter Hilton was unable to come that evening but later spoke about Alan Turing. I had arranged a supply of excellent wine and had a tape recorder ready but unannounced. After a few drinks I asked if people would mind if I tape-recorded the session. No-one objected. In particular, Mostowski went to a DLMPS/IUHPS meeting in London, Ontario, in 1974, visited Małgorzota Dubiel in Simon Fraser University in Canada and died there shortly afterwards on 22 August 1975. He was only 62. Polish logicians later told me that Mostowski had never been so open in his remarks as he was on the tape.

Here is one of the highlights and you will find the transcript of the conversation in the conference volume. That transcription, not the easiest job, was excellently done by my two graduate students, Liz Wachs and Bill Gross. So here is Steve Kleene talking about Church's thesis. You will recall this is the claim that all the computable, or to put it another way, effectively calculable, functions can be generated by Turing machines or the λ-calculus or Gödel's recursive function schemata; essentially that whatever general method you think of as giving computable functions of natural numbers yields exactly the same functions. As Church put it in his 1936 paper:

'The purpose of the present paper is to propose a definition of effective calculability [footnote] which is thought to correspond satisfactorily to the somewhat vague intuitive notion in terms of which problems of this class are often stated.' The footnote is where Church mentions the different definitions of 'effectively calculable'.

Kleene: *Gödel had this notion of general recursive function, and the questions came up: 'Does this embrace all effectively calculable functions, and is it equivalent to λ-definability?'*
As I said, for us the first idea that λ-definability was general was after the fact – after having formulated that the λ-definable functions are simply the ones for which you can find formulas in this symbolism, and discovering that everything you thought of that you wanted to prove λ-definable, you could. It was Church (I have to give the credit to Church; I can't take it myself) who asked whether

we had not really got all the effectively calculable functions.
Then Gödel arrived on the scene with another concept, and there must have been discussions between Church and Gödel. I do not know how ready Gödel was to embrace the thesis that they were all the effectively calculable functions. But Church, of course, was the one who certainly came out explicitly with this, and then it was a simple matter to prove the equivalence of the two notions. And then we had done all this work before we heard of Turing. Turing's paper [36-37] is also of 1936, like Church's one [36] with his thesis, but a little later in 1936; and my impression is that Turing did it independently of knowing anything about what we were doing in Princeton. We certainly did our work before we heard of what Turing did. Post had another version, which he did round about the same time; I guess Post probably knew of what we were doing in Princeton.
Keisler: *Was Church's thesis just a sort of off-hand remark, then?*
Kleene: *He spent some months sweating over it and saying: 'Don't you think it is so?' and I was a sceptic, and when he came out and asserted the thesis I said to myself: 'He can't be right.' So I went home and I thought I would diagonalize myself out of the class of the λ-definable functions and get another effectively calculable function that was not λ-definable. Just in one night I realised you could not do that, and from that point on I was a convert. But until Church really came out and said so, I guess I had not really believed they would be all of them.*

In advertising the 1974 conference we had sent letters (*sic*) to all two thousand or so members of the Association for Symbolic Logic. We had one response! That was from Ben Nebres in the Philippines who had done his doctorate under Sol Feferman at Stanford. He invited me to Manila and that initiated a friendship that still endures. Although the relationship helped the development of mathematics in the Philippines, it only produced one logician: my very last student Lito Cruz (who graduated in 2010). However, this led to contacts with logicians throughout southeast and east Asia. (Contacts with North Asian logicians had begun back in 1963 with Rohit Parikh and these were developed in 2005.)

4 An interesting year

The year 1975 was a particularly spectacular one in many ways. I was able to take study leave and chose to spend part of the time in Europe and part in the Philippines. There were a couple of interesting conferences in Europe. The first was the Logic Colloquium at Clermont-Ferrand in the Massif Centrale in France from 15 (Tuesday) to 25 (Friday) July 1975. Immediately before the conference I was in Munich visiting Schütte (who had been at the 1963 Oxford conference). He told me that his student Martin Wirsing was also going to Clermont-Ferrand. He drove us to France and then took the train to Clermont-Ferrand for the conference, which was a long one, taking more than a week. Partly it meant meeting old friends, partly making new ones as this was the first European conference I had attended since migrating to Australia. There must have been well over a hundred participants at the conference and it was a truly European Logic Colloquium with representatives from both East and West. I remember Małgorzota Dubiel from Poland who had been a student of Wiktor Marek who in turn was a student of Mostowski and I think they had met at the '75 conference. There was also Anne Troelstra, a Dutchman, who has never attracted me with his, to my mind, catalogue-style papers. Troelstra was being heavily promoted by Kreisel at the time in the way that Kreisel usually had his current favourite, so Troelstra was expected to be some sort of star, I think. He has certainly made a mark. Kreisel's own lecture was the one that is probably most remembered from the conference. This is not because of its content. Indeed I wonder if anyone at all can recall any of the content! It began in French, then he switched to English but he ended the lecture in German, thereby ensuring that virtually no-one understood the whole lecture.

After the conference I continued what was a very extensive tour of Europe. After a visit to Spain, including Granada, I went across Europe to Poland. I was going to the Second Conference on Set Theory and Hierarchy Theory to be held from 17 to 28 September 1975. Before that, however, I was invited to Warsaw. This was my only time behind the Iron Curtain. The people I met were splendid but the atmosphere was indeed strange. I was first taken to Warsaw briefly. I was staying right in the centre of Warsaw, very close to the Palace of Culture—a Stalinist edifice where Helena Rasiowa and other logicians had offices. The local story was that the best place from which to view Warsaw was from this Palace of Culture. Then one could not see the Palace of Culture!

From Warsaw I was taken to the conference itself which was at Bierutowice at the foot of the mountains in Silesia. The conference was delightful, the Poles wonderfully hospitable and although there were great shortages yet we were fed extremely well. After Bierutowice I returned to Warsaw briefly and stayed again in the same hotel. Inka Rauszer took me to the Mathematics Department of the Academy of Sciences and I met Helena Rasiowa for the one and only time. We talked about her not being able to come to the Oxford conference in 1963. At the age of 58 years she seemed quite old and appeared to me to be a mixture of kindness and domination. From what I saw she was clearly revered by her younger companions.

After Warsaw I was invited to Kraców by Stanisław Surma. He had started a logic journal in which Lloyd Humberstone, from the Monash Philosophy Department, and I subsequently published a paper: 'The Logic of "Actually"' [11], which I shall say more about shortly. In the strange world of pre-Solidarność Poland, with Stan I was in very good hands. He took me to the Jagiellonian University and we went and saw telescopes and apparatus that had belonged to Nicolaus Copernicus.

Sometime later Stan left Poland to work in Africa and eventually settled with his family in New Zealand. His group and all the other Poles were wonderfully warm and amazingly generous with what little they had. I remember them with great affection. After the visit to Poland I went to the Philippines for a couple of months. As I mentioned earlier, Ben Nebres had invited me after my brief visit in 1974. I gave some lectures at the Mathematics Departments of both Ben's university, the Ateneo de Manila, and also at the University of the Philippines. My visits to the Philippines continued almost every year thereafter until the pandemic struck.

Back at Monash my work with Lloyd Humberstone on the logic of 'actually' continued and eventually we produced the paper that applied the 'possible worlds' semantics of Saul Kripke (about which Hughes and Cresswell wrote a very clear book [5]) to possible worlds in which one of the worlds was the 'actual', i.e. the real, world. This is my most cited paper! Martin Davies (a student who went to Oxford and ultimately became a professor there) thought it was a trivial exercise. Perhaps simplicity is a virtue.

Academic recruiting was becoming more difficult but I was fortunate to have a steady stream of graduate students (see the Appendix).

In 1979 Jim Dekker and his wife visited from Princeton. Jim's work with John Myhill on Recursive Equivalence Types [14] had been vitally important to me, forming the basis for my thesis work on Constructive Order Types [6]. In Jim's honour we had a little conference and the papers appeared as *Aspects of Effective Algebra* [8]. In 1981 the First Asian Logic Conference was held in Singapore with Chong Chi Tat as the main force behind it. One hour invited lectures were given by J. N. Crossley, Manny Lerman, Angus McIntyre (sometime Professor of Logic at Oxford), Anil Nerode and Gerry Sacks. In addition there were two workshops, one in recursion theory (conducted by Mark Tamthai from Chulalongkorn University, Thailand) and one in model theory (conducted by Chris Ash from Monash). Rod Goldblatt from New Zealand also gave a talk as did Michael McRobbie, then at the ANU, and this was the first time I met mainland Chinese logicians including Li Xiang from Guizhou University, yet another logician who later moved into computer science.

5 Logic and computer science

The phenomenon of logicians moving into computer science is not new. From the 1960s early graduates from Cornell were quickly snapped up by newly created departments of computer science—that was easier than getting a job in a traditional mathematics department as a logician. For example, when John Staples, who had done his PhD at Bristol with John Shepherdson, was in the University of Queensland, he moved to computer science and got involved in program verification in a very practical way: verifying a program to control a heart pacemaker. Program verification, which came to be applied by such people as Geoff Dromey (Wollongong and later Griffith University, Brisbane), was one of the cases where logic was brought in to another discipline. My own move into computer science was somewhat different. It was really a matter of university politics.

In 1982, after a complaint from Chris Wallace, the head of Computer Science at Monash, about the way his department was being both overworked and starved of funds, a major, high level, inquiry chaired by two deputy vice-chancellors was launched that lasted for 2 years. During that time Chris, on leave in England, had major heart bypass surgery, I became head of Computer Science and this eventually led to me, slowly, moving totally into that department. Although I had done recursion theory, which surely is theoretical computer science, I had written only one program in my life, in Mercury Autocode. The department was most welcoming and I modified my interests. Fortuitously, Logic Programming had developed in the seventies and Melbourne developed its own variety: Constraint Logic Programming. The chief protagonists here at that time were Jean-Louis Lassez at Melbourne and Joxan Jaffar at Monash. I quickly acquired graduate students in the area.

I think it was at a conference in Canberra in 1980 that I first met Alan Robinson, the pioneer of the resolution method, and in particular unification, that underlies the logic programming language PROLOG. We just bumped into each other at the end of a session. I introduced myself as 'Crossley' and Alan did not seize on the logic but the name. 'My father used to work for a Crossley', he began. Both Alan and I come from Yorkshire, but immediately after that initial salvo we were soon talking about technical matters. As it happens, while I was a graduate student at Oxford, Hao Wang, who soon after moved to Rockefeller University, New York, had got us to study Herbrand's thesis—the source of the unification method. I still have the thesis I bought in Blackwell's in those days. Alan and I became good friends and he visited Monash where he gave a very entertaining lecture on 'Albert's cap'. The title comes from a famous Stanley Holloway monologue about a boy who gets eaten by a lion at Blackpool Zoo leaving only his cap as evidence.

"Are you sure it's your boy 'e's eaten?"
Pa said, "Am I sure? There's 'is cap!"
. . . They told what 'ad 'appened to Albert
And proved it by showing 'is cap.

I have no record of that talk but I will shortly come to a later visit by Alan.

The presence of Joxan Jaffar at Monash and Jean-Louis Lassez at Melbourne University led to great developments in constraint logic programming—combining logic programming with real number arithmetic—even to the extent of IBM giving Monash a significant amount of money for rights to the system. More about logic programming shortly. In 1984, in some ways echoing the 1974 conference, Joxan Jaffar and I ran a conference on Logic and Computation. Again, we were blessed because (so far as I can remember) all the international invitees accepted. This time the list included: Jeff Remmel (UCSD), Juris Hartmanis (Cornell), David Harel, Keith Clark (Imperial College, London), Krzysztof Apt, Anil Nerode, Zohar Manna (Stanford), Yang Dongping (Beijing University) and Yuri Gurevich (who moved to Microsoft in 1998). Unfortunately, Dana Scott did not make it because the airline would not let him on the plane: he had not applied for an Australian visa.

6 Proof theory

I continued my work with Anil Nerode and we visited each other often, especially in the '90s. Our work shifted because of the work of Jean-Yves Girard on strong normalization for second-order logic [16]. The import of that was a new way of proving the consistency of second order logic. Anil and I spent a lot of time on his thesis and this eventually led to what I believe is a more transparent way of doing the proof. John Shepherdson had been one of my thesis examiners in Oxford and a visitor to Australia on more than one occasion, and with him I reworked Girard's thesis in terms of the more usual formulation of second order logic, rather than using Girard's rather terse and cryptic formulation [12]. This was taken into a new area involving computer states by my student Iman Poernomo in his thesis that developed into our book with Martin Wirsing [20].

Academically, being in the Computer Science Department worked in both directions: I got to know more about logic in computer science and the students got more logic. The syllabus moved from basic consideration of Boolean algebra (necessary for circuit theory) and Information Theory á la Shannon to a wider consideration of logic, in particular its relation to algorithms: the work of Girard and others was leading to a new generation of theorem proving: obtaining algorithms from proofs. In a way, things had come full circle since when Hao Wang had been on leave from Oxford he used an IBM 704 computer to prove several hundred theorems in propositional logic from Whitehead and Russell's *Principia Mathematica* [22]. Incidentally, he always espoused formal methods rather than heuristic ones and his formal approach seems to have been much more effective.

Teaching logic in a computer science environment was very instructive. I had an excellent assistant, called Joel Reicher, who taught me much about the computer science students thought. That was very different from the way mathematicians thought and learnt. There were two major differences: On the one hand, mathematicians learnt all the techniques first and only later how to apply them; computer science did not learn techniques until they needed to use them. On the other, proofs were alien to computer scientists but algorithms were not. The timing of my interest in extracting programs from proofs á la Girard led me to look at the relationship between proofs and programs not just abstractly but practically also. I endeavoured, as much as possible to provide constructive proofs (which almost looked like algorithms) as much as possible, studiously avoiding proofs by contradiction.

In 1997 Alan Robinson made another visit to Monash and on 19 March he spoke about the development of Logic Programming. The whole may be seen at https://www.youtube.com/watch?v=cIM0bca3bdo and https://www.youtube.com/watch?v =gszbkhG-nOk&t=1289s. Alan described how things all came together from Herbrand's *Property A* and work of Davis and Putnam and Dag Prawitz; see [21]. For the record: Property A is (almost) buried on p. 93 of Herbrand's thesis [18].

7 The 1990s

By this time, the late '90s, I was doing less logic and more history. My last student, Lito Cruz, and I had remarkable difficulty getting his work on agents published. What had seemed to me an elegant and simple double-decker logic seemed to perplex the organizers of various conferences we approached. In the end, however, we got the best paper award at the 2009 International Conference on Agents and Artificial Intelligence in Porto, Portugal, and a fuller version of the work was published in this journal [13]. Over the years I have also had the pleasure of having a few students from other places than Melbourne; see the Appendix.

8 Philosophy and logic

So what does this all mean? How much has mathematical logic clarified our ideas of the notions of mathematics and why has it been so important for computer science? For a start, it is worth remembering how Mary Carruthers [2, 58] contrasts the practices of philosophy and history. 'With philosophy the problems keep changing, ... philosophical works are products of their time, and it is important to acknowledge their cultural matrix. Students of history have long recognized that philosophical answers are specific to time and place, but one needs also to keep in mind that philosophers have not, in fact, asked eternal questions.'

My basic premise is that logic and mathematics are human activities. The early twentieth century division of philosophy of mathematics into Platonism, Formalism and Intuitionism is, perhaps surprisingly, a manifestation of that. There seems no reason to believe that there is an ideal (platonic) world underlying the real world, because all our actions take place in the real world. This is not to deny that the systems we create have a life of their own—witness the convergence of the definitions of effectively computable that Steve Kleene talked about.

However, in order to communicate about mathematics and logic we need to engage in intersubjective activity, to use Peter Caws's phrase. If there is sufficient communication between, say, two people, then we believe that we understand each other and we can investigate phenomena together. For this it is essential to both know and agree on the rules of engagement. Imre Lakatos gave a delightful analysis of the proof of the Euler formula: Vertices plus Faces minus Edges = 2 for a three dimensional rectilinear solid [19]. He showed how, over time, the proof had changed as people made more precise the notion of 'a three dimensional rectilinear solid'. For a start it had to be convex, and the present formulation of the theorem now involves convex polytopes, though we all usually thing of 'ordinary' solids such as cube, tetrahedra, etc.

This process of refining definitions is more commonly thought of in relation to the physical sciences, but it also occurs, and is necessary, in mathematics. A prime example of a definition that needs refining is that of a set. Axiomatic formulations of set theory have never succeeded in giving a definitive idea of a set in the past and it seems unlikely they will do so in the future. Nevertheless, we can get a lot of use out of set theory.

Let me turn briefly to the other two divisions. Formalism claims that mathematics is simply a game but it has been argued that it is of far too much practical use for that to be the case. Unfortunately, that argument has been undermined; the argument of 'the unreasonable effectiveness of mathematics' collapses when we see why particular varieties of mathematics are developed; see [17]. They flourish because they are useful, though it is important to note that their usefulness may not be in the solving of, say, engineering problems but abstract problems in higher set theory. If we adopt a more human approach, then we can see that formalism in mathematics, that is to say the use of formulae, etc., is of great benefit to human endeavour and the correlations between abstract and ideas and physical phenomena work well enough together. I remember being shocked in my youth by physicists simply neglecting singularities of functions they were using. People like Dirac managed to develop theories that resolved some of those problems. The case of Intuitionism is somewhat different. Although, as conceived by Brouwer, it sounds more like a religious creed than the philosophy of mathematics, the technical approach has been immensely fruitful, not least in computer science. As I mentioned earlier, the way I now look at mathematical proofs in computer science is guided by looking at the algorithms involved and those algorithms are of their very essence constructive—conforming to Intuitionistic logic. So we now have two common kinds of logic used in mathematics and mathematical logic: classical and constructive/intuitionistic. I believe this simple development came about because of the cultural approach in Europe: the way European mathematicians thought is crucial. When one looks at Indian logic its complexity is mystifying and, in my practical experience, seems quite bewildering or confusing even to some of its practitioners. On the other hand, Western developments had led to other kinds of logic. The ways of Quantum Physics led to the development of Quantum Logic; philosophers have developed various kinds of Modal Logics. All of these are human activities and they are used to clarify what is going on in different areas of study. As I said in my paper ‘Samsara’ [9], I expect many different kinds of logics to develop in the near future. I do not expect any one logic to be definitive in the way that logicians of the early twentieth century felt that. I do expect the logics to be judged for their usefulness in selected areas.

Wittgenstein famously said that ‘the meaning is the use’. It took me a long time to parse that as an English sentence, but it seems much more accurate than his earlier claims that ‘what can be said can be said clearly’. The clarity of a concept is something that is capable of changing over time and according to the environment. And *‘What is mathematical logic?’* continues. Despite their savage 1976 review [4], Corcoran and Shapiro acknowledged ‘[t]his highly praised book’ and it does indeed seem to have stood the test of time. There are versions in Bulgarian, Italian, Japanese and Spanish. The latest translation is a Chinese one. Is it time for a second English edition?

But let me end with the words of the Audrey Azoulay, Director of UNESCO, promoting on World Logic Day in January 2022 and one final comment.

Logic is ever-present:
when you use AI software,
when you turn on your computer,
when you develop an argument.
Logic is a contemporary universal.

To me, the usefulness of mathematical logic is in helping to clarify ideas: a very necessary human activity. But very much a human activity. This approach affects the philosophy and theory of Mathematics and Mathematical Logic very much, but the practice hardly at all.

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Appendix. Graduate students of John N. Crossley

PhD graduates

1. Peter H.G. Aczel, (Recursion Theory, 1964), Professor, Mathematical Logic and Computing Science, University of Manchester.
2. Alan B. Slomson, (Model Theory, 1968), Senior Lecturer, Mathematics, University of Leeds.
3. John L. Bell, (Infinitary Languages, 1969), Emeritus Professor of Philosophy and Adjunct Professor of Mathematics, University of Western Ontario.
4. Alan G. Hamilton, (Recursion Theory, 1969), Lecturer, Computing Science and Mathematics, University of Stirling, Scotland.
5. Wilfrid A. Hodges, (Model Theory, 1970), Emeritus Professor, Queen Mary, University of London.
6. Christopher J. Ash, F.A.A. (Model Theory, 1972), Reader, Mathematics, Monash University.
7. David W. H. Gillam, (Model Theory, 1973), sometime Lecturer, Mathematics, Capricornia Institute of Technology.
8. William F. Gross, (Set Theory/Model Theory, 1975), Australian Bureau of Statistics, sometime Senior Tutor, Mathematics, University of Tasmania.
9. Elizabeth A. (Wachs) Sonenberg, (Model Theory, 1976), Professor, School of Computing and Information Systems, University of Melbourne.
10. David Lucy, (Set Theory, 1979), Lecturer, Mathematical Sciences, Swinburne University, Melbourne.
11. Rod G. Downey, (Recursion Theory, 1982), Professor of Mathematics, Mathematical and Computing Sciences, Victoria University of Wellington, New Zealand.
12. Michael F. Moses, (Recursion Theory, 1983), Associate Professor, Mathematics, George Washington University, Washington, D.C..
13. Philip J. Hingston, (Recursion Theory, 1983), IKMRG, Edith Cowan University, Western Australia.
14. Peter J. Stuckey, (Logic Programming, 1987), Professor, Computer Science and Software Engineering, Monash University.
15. Christopher J. Stuart-Ho, (Parallel Programming, 1990), Lecturer, School of Software Engineering & Data Communications, Queensland University of Technology.
16. Pierre Lim, (Interface Logic Programming, 1991), previously at European Centre for Research in Computing, Munich, Germany, now in private employment.
17. John S. Jeavons, (Logic Programming, 1992), (retired) Lecturer, Mathematics, Monash University.
18. Roland Yap, (Logic Programming, 1995), Associate Professor, National University of Singapore.
19. Luis Mandel, (Constrained Lambda Calculus, 1995), Ludwig-Maximilians-Universität, Munich.
20. Hannes Peterreins, (A natural-deduction-like calculus for structured specifications, 1996), Ludwig-Maximilians-Universität, Munich.
21. Matthias Hölzl, (Constraint-Lambda Calculi: Theory and Applications, 2001), Ludwig-Maximilians-Universität, Munich.
22. Matthew Spinks (pre-BCK-algebras, 2003).
23. Rob Rendell (Constraint logic programming in multiple domains, 2003), Research Fellow, RMIT University, Melbourne.

24. Pimpen Vejjajiva, (Program extraction from proofs, 2003), Full Professor, Chulalongkorn University, Bangkok, Thailand.
25. Iman Poernomo, (Program extraction from proofs, 2004), Postdoctoral Fellow, Monash University, Clayton, Australia. Mollie Holman Medal for thesis. Lecturer King's Collge, London. Subsequently, JP Morgan and currently Chief Data Officer, Preqin.
26. Lito P Cruz (Logics for agents in context, 2010), computer consultant and lecturer.

MSc graduates

1. Frank P. Jackson (Lambda-combinatorial operators, 1975).
2. Alan R, Woods (1977), Adjunct Associate Professor, Mathematics, University of Western Australia.
3. David Leigh-Lancaster, (History of logic: Boole, 1986), Manager, Mathematics, Victorian Curriculum and Assessment Authority.

Received 28 May 2022