Talk for AiML Conference, 2026

I am very pleased and honored to participate in the present event, the 30th anniversary of the International Conference on Advances in Modal Logic. I still have fond memories of the only other conference of AiML that I attended, in 1998 at Uppsala; and it is great to be back after such a long period of time.

Many, if not most, of you would not even have begun your academic careers before the Conference's founding in 1996. But my own serious work on modal logic preceded that date by at least 10 years, beginning with my thesis 'For some Proposition and so many Possible Worlds', completed in 1969, and ending with my paper, 'Logics Containing K4, Part II', published in 1985 - a period of roughly 15 years. So when I was asked to given an address to the Conference, I thought it might be of interest to talk about this earlier, quite significant, period in the history of the subject from someone who had experienced it at firsthand. And I hope it will not be regarded as too self-indulgent of me if I approach the topic in autobiographical mode, focusing on my own working during that period and on the people with whom I had had contact.

I attended Cheltenham Grammar School for Boys in the UK from 1956-63; and it was during the last three years there that I became interested in logic. My interest had three main sources, none of which came directly from the teachers. The first was a book on logic by Susan Stebbing, which I had found in the school library. Upon sleuthing the internet, I worked out that this must have been her book, 'A Modern Introduction to Logic', first published in 1930. It could not have been so modern, since I recall it contained an extensive discussion of Aristotle's syllogistic. The second source, also from the school library, was Russell's book 'Introduction to Mathematical Philosophy'. I had always been puzzled by the explanation of the derivative given in our calculus classes; and this book helped put my puzzlement to rest. The third source was the periodical, 'Mathematical Gazette', which my father received at regular intervals through the mail. Most of it was concerned with what one might call regular mathematics, but one of the articles was written by the mathematical logician, Louis Goodstein, who was also one of its editors. The article dealt with the question of when one truth-function was definable in terms of others and I recall that, at the end of the article, he posed the problem of providing a general criterion for when a truth-function in n-valued logic would be Sheffer complete, i.e. such that all other truth-functions could be defined from it. I spent many hours, without avail, working on the problem.

So, to that date, my principal exposure was to Aristotelian syllogistic and truth-functional logic. All this changed when I went up to Oxford. While in my second year as an undergraduate at Balliol, Arthur Prior was appointed to a fellowship at the college. I was assigned to him as a tutee; and we quickly formed a strong professional and personal bond, spending many hours talking over technical problems in modal and tense logic that were of interest to him at the time. It was largely as a result of these encounters that I became interested in modal logic; and I have sometimes wondered, as perhaps may some of you, what if I had first been exposed to some other branch of logic - recursive function theory, say, or set theory? Would I then have become a recursive function theorist or a set theorist? It is hard to imagine that this would have been so, though this is perhaps akin to its being hard to imagine having had different children in place of the ones you actually have.

Arthur Prior played not only a pivotal intellectual role in developing the subject but also in bringing together the different people who were working within it - what would now be called 'networking', though without the benefit of the internet. He and his wife, Mary, would hold receptions at their home in Oxford for visiting modal logicians. I recall meeting Max Cresswell, Hans Kamp, Krister Segerberg, Rich Thomason and many others at such gatherings.

He was also helpful to me personally in a number of other, more particular, ways. Arthur had previously visited UCLA in the summer of 1965 and was very enthusiastic over the level of interest -in modal logic and the level of sophistication of their work in that area. He had, I believe, mentioned my name to Richard Montague and, during my last year as an undergraduate, Montague wrote me a very nice note, offering me a TA-ship to help finance my further studies at UCLA. I had, however, already been offered a lectureship at the University of Warwick and took that up in preference to the TA-ship. I never got to see Montague and I regret to this day that I had never had the opportunity to meet or to work with him.

In my last year as an undergraduate at Balliol, I received what was then called a Coolidge Award for travel the United States; and Arthur gave me a list of names of a number of people working in modal logic whom I might want to look up and meet. So, in between traveling by Greyhound bus from state to state, I would look up some of the people on the list. I can still recall the look of surprise on Nino Cochiarella's face when this fresh–faced undergraduate from the UK stood on his doorstep to ask him questions about his interpretation of modal logic. Looking back on my peripatetic life-style at that time I cannot help but bring to mind when Bach walked all the way to Northern Germany to study with the celebrated organist and composer, Buxtehude. We were both the same tender age of 20 at the time and willing to go far in search of a master. But I am no Bach; and nor did I possess his athletic vigor, for whereas he had to trudge 250 miles by foot to reach his goal, I had the convenience of being able to travel by Greyhound Bus.

Arthur was in constant communication with other modal logicians and would often share the contents of their letters with me. I recall, for example, his reading out a letter he had received earlier from Kripke - then a young man of 18 - about how tense logic, with its assumption of the linearity of time, would need to be modified in the light of the theory of Special Relativity. A topic on which a great deal of philosophical and technical work has subsequently been done.

Saul also wrote to me, somewhat later, about my incompleteness results. I was, of course, touched to have received a communication from the great man. But I was also bemused. For the letter was not addressed, in typical cordial American fashion, to 'Kit', or even to 'Kit Fine', -but to plain 'Fine'. Some of you may not know this, but there is a tradition in English public schools, taken up by my grammar school, of addressing the students by their last name. And so it was hard not to feel that he was the teacher and that I, just as in my schoolboy days, was once again the student.

I got to meet Saul in person somewhat later, in 1974. I was visiting the Stanford philosophy department as a temporary replacement for Dov Gabbay; and Kripke had come to the department to give a talk. It was on the surprise test, work subsequently published in the first volume of 'Philosophical Troubles' (Kripke [2011]). We got on very well and I was amazed by the ease with which we were able to communicate. When two philosophers talk, there is nearly always some kind of intellectual friction. 'What do you mean?', one of them will say. Or, 'isn't there the following problem with your view?'. Or, 'what of this alternative position?'. Our conversation, by contrast, seemed to be more of less free of such friction; and it made me think that it may not always be necessary for the purposes of having a fruitful philosophical discussion.

The day after his talk, he, myself, my then wife, Danny, and our young child went for a picnic. Two episodes stand out. At the talk, Georg Kreisel, who was then a member of the philosophy department, asked a question that Saul was at a loss to answer. While we were on the picnic, Saul asked me if I understood the question; and I had to confess that I had not been able to work out what Kreisel was saying. We both then felt somewhat reassured that the problem may have lain with him rather than with us.

The other episode related to the picnic itself. My wife, Danny, struggling with our small child and the picnic things, placed a hamper into Saul's arms, as if to say 'please carry this for us'. Saul looked puzzled and immediately put the hamper down, and carried on walking and talking. Perhaps it's still there. I have subsequently met and talked to Saul on many other occasions, but it was this, our first meeting, that was by far the most memorable of my encounters with him.

Looking back on those early years, I am mindful of how sociable and personable we all were. During my time at Oxford, Arthur Prior invited me to join him, his wife and their two children on a week long trip through the canals of Oxford and its environs. Mary was an expert on the canals and also an excellent cook; and so, while she informed us of the history of this or that section of the canal or cooked our dinner, Arthur and I could engage in the masculine activity of steering the barge, working the locks, and talking philosophy.

During the trip, I faced a near death experience. Mary had cooked a mousse for dessert but, as a result of the barge swaying from side to side, it fell to the ground and the glass bowl in which it was contained shattered into pieces. I was not to be denied my mousse; and so I went ahead and ate some of it, despite the protestations from Arthur and Mary. I think that they were worried that I was going to die that night from internal hemorrhage. But the gods were smiling upon me; for as they could see the next morning and as you can now see for yourselves, I lived to tell the tale.

Danny and I also did our fair share of hospitality. I recall that David Lewis and his wife, Steffie, joined us for dinner at our small house in Observatory Street in Oxford. I said, 'joined us for dinner', though I should have said, 'sat down with us for dinner'. For no sooner had we sat down, that David announced, in the sternest terms, 'we have to leave!', which they abruptly did. Steffie explained the next day that David was worried about having a diabetic attack.

But it was not all dinner parties or non-dinner parties; it was during their stay in Oxford, I recall, that David talked with me about his proof of the finite model property for propositional modal logics without iterative axioms, work that was subsequently published in 1974 (Lewis [1974]). As far as I am aware, this was the only technical work on propositional modal logics that he ever published.

There was another social occasion which also did not pass without incident. A couple of years later, Richard Routley stayed with us at our apartment in Edinburgh to discuss the recent semantics for relevance logic that he, Meyer and myself had recently developed (Routley & Meyer [1973], Fine [1974c]). Richard had been hiking and so wanted to take a bath. This he did, but he left his dirty socks in the tub, no doubt assuming that Danny would take care of them. When combined with the incidents with Saul and David, I can easily imagine how she might have come to have such a dim view of the social skills of my logician colleagues.

The easy sociability was typical at the time but not something that is so likely to happen today. In inviting me on the canal trip with his family, Arthur would have been thought guilty of favoritism. Why me, rather than any of his other students? And, in turning up unannounced at Nino Cochiarelli's doorstep, I myself was hardly the model of polite behavior. The profession has since become more professional; and, of course, there no longer exist the wives to cook our food, launder our socks, carry the picnic hampers, or explain why their husband left so suddenly from the dinner table.

The contact with other logicians, whether or not it was facilitated through the long-suffering wives, was very important to my intellectual development - and perhaps to many others

- at that time. I have already mentioned Max Cresswell, Hans Kamp, Krister Segerberg, Rich Thomason, Saul Kripke, David Lewis, Richard Routley and Bob Meyer. But I should also mention Dana Scott, Steve Kuhn, David Kaplan, Dov Gabbay, Steve Thomason, Alasdair Urquhart, Nuel Belnap and Johan van Benthem - all of whom exerted a strong intellectual influence on me in one way or another.

Let me say a few words about my encounters with some of these individuals, though there is no time, alas, to discuss them all. Dana Scott, along with William Kneale, were the examiners for my thesis; and I had several helpful, though nerve-wracking, discussions with him about its connection with Boolean algebra. He very kindly put on a party for me after the viva exam. However, no one had told me whether or not I had passed the exam. Of course, it would have been very cruel to hold a party for me if in fact I had failed. But, just to be on the safe side, I nervously asked Mary Prior just before we entered his apartment whether or not I had passed. She assured me that I had; and so I was able to relax.

I first met Nuel Belnap when he gave a talk at the logic series at the Oxford Mathematical Institute around 1970. He there mentioned the problem of finding a semantics for the system R of relevance logic; and it was this talk of his that got me thinking about the problem. When I sent a manuscript copy of my proposed semantics to him, he put me in touch with Richard Routley, who informed me that he and Meyer had previously obtained the result but who, all the same, wrote a very kind and supportive letter in return.

Steve Kuhn was a graduate student of mine when I was a visiting professor at Stanford in 1974. I had produced some notes for a course on the foundations of modal logic that I put on at Stanford; and we later worked on turning them into a textbook. It soon became clear that we should first write a textbook on the foundations of classical predicate logic. But once we had embarked on this other project, it became clear that we should first write a textbook on the foundations of classical propositional logic. This we have now done, fifty years or so after the project began; and the first volume of the manuscript, I am glad to say, is now with the publisher. We hope to produce the second volume, on the foundations of classical quantified logic, within the next few years. But I fear that the final installment on the foundations of modal logic must wait our afterlife.

Looking back on these various encounters, I am aware of the extent to which I learned about the subject through personal contact. It was from Nuel Belnap and Alasdair Urquhart that I learned about the open problems in relevance logic; it was from conversations with Steve Thomason, a mathematician at Simon Fraser, that I first learned about propositional dynamic logic and the provability interpretation of modal logic; and it was Prior, of course, who introduced me to the subject. This kind of personal contact is always helpful, of course, but it was especially helpful in a period when remote communication was difficult and unpublished work not readily available.

Let me get back to the thesis. It was on two related topics, propositional quantification and what are now called graded modalities, which indicate how often a proposition is true in some possible world. This work arose from the reductive project that Prior had been pursuing. For he wanted to think of a possible world as a world proposition, one true in exactly one possible world; and so he added to the standard modal language an operator Q with the meaning ‘it is true in one possible world that’. He also wanted to quantify over these propositions in order to simulate quantification over possible worlds. Hence the interest in propositional quantification. There is also a deeper connection between the two, since, in suitable contexts, the propositional quantifiers can be eliminated in favor of the graded modalities (Kaplan [1970], Fine [1970]).

My thesis work resulted in my first publication in 1970 (Fine [1970]), on propositional quantification, and in two subsequent publications in 1972 (Fine [1970a,b]), one on graded modalities and the other on graded quantifiers (which indicate how often a first-order condition is met). However, the work did not fall within the mainstream and, consequently, did not have much of an immediate impact. Oddly though, it did later have a significant impact on the development of the Web Ontology Language (OWL) and on other related areas of computer science. There has also, very recently, been a revival of interest in propositional quantification, as evidenced most notably in the work of Peter Fritz [2024]. But this further work arose not so much from an interest in Prior's reductive project as from a more general interest in higher order modal metaphysics, since the logical behavior of propositions plays a key role in determining the logical behavior of other higher order entities.

I almost did not publish the paper on graded quantifiers. It struck me as mathematically trivial, and I only published the work because it solved a problem that had been posed some time before by Tarski in his introduction to logic (Tarski [1946], §20). There is no knowing what unintended applications one's work might have. However, I am not sure that one's ignorance on the matter should be treated as a general justification to publish whatever work one has done no matter how trivial it might seem at the time.

In the early 1970's, I turned to more mainstream questions; of completeness, decidability, axiomatizability and the like for normal propositional modal logics. There were two foundational documents in the area, which together held out the promise of providing a firm and versatile foundation for further work on these questions. The first was Kripke's paper, 'Semantical Considerations on Modal Logic' (Kripke [1963]), which provided a framework in which a model theory for a modal language could be developed and in which the notion of validity could be varied according to the conditions imposed upon the accessibility relation. The other was the Lemmon-Scott notes (later published in Lemmon [1977]), which introduced the idea of establishing completeness of a logic via the associated canonical model, in which the possible worlds were taken to be the maximally consistent sets of formulas. Kripke had proofs of completeness for particular modal systems using semantic tableaux, but his methods were clunky and laborious and not readily open to variation or generalization. The Lemmon-Scott notes held out the promise of more general and versatile method for establishing completeness.

I recall working from a mimeographed copy of the notes, and nor am I sure how I got hold of them. Perhaps from Prior, who had been in constant contact with Lemmon until his untimely death in 1966; or perhaps from Dana Scott, who had been one of the examiners of my thesis. Remember, this was not the age of the internet; and so it was not so easy to get one's hands on unpublished material. In any case, these notes formed the common ground for much of the work that was subsequently done on normal modal logics.

The key question at the time was the question of completeness: is every normal modal logic complete, i.e., is there, for any normal modal logic, a constraint on the accessibility relation under which theoremhood in the logic will coincide with the corresponding notion of validity? It was anyone's guess. The method of proving completeness via the canonical model had been applied to a wide range of logics in the Lemmon-Scott notes; and it might have seemed from the work of Sahlqvist (see Sahlqvist [1975] and van Benthem [1983]) that the method had an even wider range of application than had originally been envisaged. Perhaps every logic, or every complete logic, could be proved complete via the canonical model or, if not via the canonical model itself, then via some suitable variation of the canonical model. On the other hand, the condition corresponding to the validity of a a modal axiom is in general second order. The standard formulations of second order logic are not complete; and so perhaps the same is true of some modal logics, despite their huge relative loss in expressive power.

My own approach to the problem was to work on both fronts: attempt to establish completeness by the most powerful methods I could come up with; and attempt to find a logic for which such methods would not work. This led to both positive and negative results. In analogy to Tarski's notion of essential undecidability (Tarski [1966]), let us say that a normal modal logic is *essentially complete*, or that it is *essentially fmp* (essentially has the finite model property) if every normal modal logic extending the given logic is complete (has fmp). From result of Scroggs [1951], we knew that S5 was essentially complete - indeed, was essentially fmp. Bull [1966] had an algebraic proof, and I a corresponding semantic proof, that S4.3, the logic of reflexive linear order, was essentially fmp. A natural thought, then, was to bring these results downwards to the extensions of even weaker logics.

This I was to some extent able to do in the first part of the paper, 'Logics containing K4' (Fine [1974]). Let a *finite width logic* be one containing a theorem to the effect that there are most n incomparable worlds accessible from a given world, for some number n > 0. Then I was able to show that any finite width logic was complete (although the corresponding result for fmp can be shown to fail). Oddly enough, the method of proof was somewhat akin to Kripke's original method of semantic tableaux. A semantic tableau is super-selective in the worlds it chooses to witness a possibility statement. The canonical model, by contrast, is super-indiscriminate. My method of proof was based on a slimmed down version of the canonical model, but was akin to the method of semantic tableaux in that unnecessary witnesses to a possibility statement were removed. It was selective by eliminating what was unnecessary rather than by incorporating what was necessary. One can see Dov Gabbay's method of selective filtration (Gabbay [1972]) as being based upon a somewhat similar idea.

I submitted the paper to the Journal of Symbolic Logic at about the same time as the paper on models of entailment; and the editor to whom I submitted both papers was Dagfinn Follesdal. The paper on K4 was accepted, the paper on models of entailment was not. I suspect that there was something of a prejudice against relevance logic in those days, even among those who were already sympathetic to modal logic. In any case, I had held a grudge against Dagfinn on account of this rejection until I met him and was completely disarmed by his grace and charm.

On the negative side, I was able to produce an example of an incomplete logic containing S4 (Fine [1974a]). This was discovered through trying to see how the method of proof that I had employed in establishing completeness for the finite width logics containing S4 might break down in application to logics that were not of finite width. I later adopted a similar two-pronged approach in tackling the much more difficult question of whether the system QR of quantified relevance logic was complete under the semantics that Routley, Meyer and I had developed. I first tried to prove completeness by a method which I thought had to succeed if the logic was complete. The place where the attempted proof broke down then pointed to where and how the logic might be incomplete.

My work on incompleteness and the related work of Steve Thomason [1974] led to a much deeper understanding of the incompleteness phenomenon. For all we knew back then, incompleteness was an isolated phenomenon. But we now know from the work of Blok [1978] that a logic is either not incomplete at all or else is incomplete to the highest degree (with $2^{\aleph_0}$ logics determinative of the same class of frames). His paper, which blended semantic and algebraic elements, was published only 4 years after the original papers; and it was emblematic of the increased mathematical sophistication of work in the area.

Around 1974, I became interested in classical model theory. I had been briefly acquainted before with Abraham Robinson's 'Introduction to Model Theory' and, for admission to the D. Phil. program at Oxford, which I never took up, I had submitted a new method for establishing completeness for first-order predicate logic, akin to Kalmar's constructive proof of completeness for propositional logic. For some reason, I am not sure why, I returned to the subject and studied two more recent texts: 'Models and Ultraproducts: An Introduction' by Bell and Slomson, published in 1969; and 'Model Theory' by Chang and Keisler, first published in 1973. I remember teaching an extra-curricular course on the Chang and Keisler at the University of Toronto, where I was then a visitor, as a way of familiarizing myself with the text, though I do not think that I got many takers.

The renewed interest in classical model theory was fortunate even if fortuitous. For it had two interesting upshots for the study of modal logic. The first was to the method of proving completeness by way of the canonical model. I had realized that there was a close connection between the role of the canonical model in proving completeness and the ultraproduct construction in classical model theory; and this led me to forge some connections between being canonical, i.e. complete for the canonical frame, and being elementary, i.e. expressive of an elementary class of frames.

The other upshot was to the development of what one might call *modal* as opposed to *classical* model theory. Given the great success of the one, it is natural to wonder to what extent a similar sort of success might be attainable for the other. There were, to my mind, two main ways in which classical model theory had been so successful. The first was at the object-level, to the first-order formalizations of particular theories, such as set theory or first-order arithmetic; the other was at the meta-level, to the development of the general theory of first-order theories. Although the existence of the first-order formalization of these various theories is often taken for granted, I believe that it constitutes one of the great achievements of logic in the twentieth century. After all, it is only once we have the formalizations at hand that the more formal study of them can proceed.

I therefore embarked on the project of developing modal model theory along each of these lines. In regard to the first line of enquiry, I attempted to provide first-order formalizations of the modal theories of sets, of propositions, and of facts. In regard to the second line of enquiry, I largely focused on preservation theorems. Thus, just as we might wish to find a model-theoretic characterization of when a classical first-order sentence is equivalent to a universal sentence, we might wish to find a model-theoretic characterization of when a modal first-order sentence is equivalent to a de dicto sentence, one in which there is no quantification into modal contexts. I also worked on establishing the interpolation lemma for first-order modal logic and discovered, again by looking at how a proof of the lemma might fail, that the lemma did not in fact hold (Fine [1979a]). Thus, even though there exists classical interpolants for the pair of classical formulas corresponding to two modal formulas, the peculiar expressive limitations of the modal language prevent there from being a modal formula corresponding to any of the classical interpolants.

An enormous amount of subsequent work has been done on both of these fronts. Much work has been done, for example, in developing first-order modal theories of sets under various different motivations and various different ways of understanding the modal operators. But let me mention, in particular, Kripke's review of my paper on the failure of the interpolation lemma in quantified modal logic and of his subsequent review of my three-part paper on model theory for modal logic (Kripke [1983], [1985]). These reviews contained a great deal of interesting material and commentary, some of which has not, as far as know, been taken up. Thus, in his review of the third of the papers on the model theory for modal logic, he raised the following question. Let L be the logic of quantified S5 with varying domain, but with no restriction on when an atomic predication can be true in a world, just as in Kripke's original semantics; and let L^+ be L with the assumption that atomic predications can only be true of existents. Look at the formulas of L^+ that are schematically valid, i.e. valid under any substitution for their atomic predicates. Are the valid formulas of L then the same as the schematically valid formulas of L^+. In other words, can we gain the effect of having no restriction on atomic predication by relaxing the requirement that the predications should be atomic? I remember once posing this problem to Dov Gabbay, who, in his characteristically confident manner, thought that it should be straightforward. But, to this day, I am still waiting for answer.

After writing the papers on modal model theory, I returned to the subject of modal propositional logic which, for reasons I cannot now recall, I had put on hold, and published the second part of the paper on logics containing K4 (Fine [1985]), in which I introduced the notion of a subframe logic and showed how all subframe logics had the finite model property. The framework of subframe logics was subsequently extended and applied by Zakharyaschev [1992] and a number of other authors. Although I still had a lot of modal logic in me and, indeed, had even contemplated a third part to the papers on logics containing K4, dealing with the decidability of logics that lacked the finite model property, I decided to move on to other things; and so this paper turned out to be the last of my serious technical contributions to modal logic.

In those years from 1967 to 1985, in which I had been most active, the subject had radically changed. When I started out, most of the technical work had a strong philosophical or extra-mathematical motivation. One can see Prior's interest in graded modality and propositional quantification, for example, in the context of his attempting to reduce talk of possible worlds to modal discourse, or Kripke's early work on the semantics for quantified modal logic as a reaction to Quine's doubts about the intelligibility of de re modal discourse, or Montague's development of higher order intensional logic as deriving from his attempt to provide a formal semantics for natural language. Relatedly, most of the people working in modal logic were from philosophy departments - Saul Kripke, David Lewis, David Kaplan, Richard Montague, Hans Kamp, and Bob Stalnaker, to name just a few.

This is no longer true. Most of the people working in the technical areas of modal logic no longer come from philosophy departments; and there has been steady increase in the degree to which work in modal logic is more directly tied to a mathematical rather than to a philosophical or extra-mathematical motivation. One asks questions that a mathematician is more likely to ask than a philosopher, say, or a linguist. What is the lattice-theoretic structure of the field of normal modal logics? How common is the incompleteness phenomenon? Is a normal modal logic canonical in one infinite power canonical in all infinite powers? I do not say that this mathematical orientation of the subject is a bad thing. Indeed, I may myself have been somewhat responsible for it. However, there is always a danger - in this area as in logic as a whole - of losing sight of the more foundational concerns that provided the subject with its original impetus.

Another change, again perhaps beginning in the 1980's, was a shift in the geographical center of gravity - away from the States and towards Europe. Much of the early work in the 1960's and 1970's and many of its central figures were from the States. But this has gradually changed; and one has only to look at the list of invited speakers at the conferences of AiML from 1996 onwards to see that this is so.1

The year of 1985 also coincided with the date at which my last student in modal logic, Roy Benton, completed his thesis (Benton [1985]). This was on the incompleteness phenomenon; and it was concerned with extending and refining the previous results of Blok. In what may have been the result of a subconscious desire to repay Prior's hospitality to me when I myself was working on a thesis, I invited Roy to join me on a week--long hiking trip in the Sierras. The muddy waters of the Oxford canal system were to be replaced with the pristine grandeur of snow-capped mountains. And yet once again, in an uncanny twist of fate, I faced another near death experience.

We were walking with snow-shoes on some very soft snow when, all of a sudden, I fell down to my neck in the snow. About ten feet below me was a raging torrent of water, and it seemed evident to me that if I were to fall then I would be carried far away, deep under the snow, never to be seen again. In a panic, I beseeched Roy to help me get out since any movement on my part might lead to my immediate demise in the icy raging waters below. But Roy was so amused by my predicament that he first took some photos of my head rising above the snow; and only then did he help me out. I am grateful to Roy - and to the gods - for enabling me, yet again, to escape from the clutches of death and to continue with my work.

When I first worked with Prior, the mathematical study of modal logic was in its infancy and it was not then clear how far it could go. There was still a tendency to think that the modal operators were quantifiers in disguise and that whatever was done modally was best done quantificationally. It is now clear that this attitude was not justified; and the success of AiML is both an examplar and a tribute to how far the subject has progressed from those early days. Even if I did not myself continue with the subject, I am happy to have been among a small group of people who helped make possible its subsequent success.

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[1]When I asked AI how many modal logicians were active in modal logic in the US and in Europe, it gave the estimate of 150-250 for the US and 300-500 for Europe.

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