

## ACS Bulletin, November 1978, Page 4-6

### The ALGOL 68 Story

A personal account by a member of the design team

*Professor John E. Peck, University of British Columbia, Vancouver, Canada*

#### Early Years

My early years were spent in South Africa. I got my B.Sc. at the University of Natal in mathematics and physics and soon after that my M.Sc. in mathematics. My first job was a daily analysis of superphosphate at a chemical factory near Durban belonging to the ICI group, and then I taught mathematics to engineers at the University of Natal.

In 1946 I took up a scholarship and went to Yale University for graduate studies, where I obtained a Ph.D. in 1950 with a thesis on topological semigroups. I taught a year at Brown University and then for three years back at the University of Natal. We then decided to emigrate to Canada where I spent a year at the University of New Brunswick, four years at McGill University, ten years at Calgary and the remaining years at the University of British Columbia.

Up to the year 1959 I had thought little about computing, my professional activities being in the realm of pure mathematics where the activity of greatest importance was the proving of useless theorems. In the spring of 1959 an invitation was circulated in the Mathematics department at McGill, where I was working, to learn computer programming on the Datatron. My colleagues all looked down their noses at the thought of such a mundane exercise, for wasn't it clear that all that needed to be known about computing was the mathematical theory of switching circuits. Despite this I became interested, so my first program was for the Datatron.

As a result of this spark of interest, I attended a summer school at the University of Oklahoma in 1959 where I learned to program the IBM 650, another drum machine, whose greatest glory at that time seemed to be an optimizing assembler called SOAP, which knew how to place the next instruction on the drum in such a position that it would be picked up conveniently after the previous instruction had completed execution. At that time there was talk of a new language called FORTRAN, but we had little time to study it, except to know that its translation was from Fortran to IT to SOAP to object code, where IT stood for Internal Translator. My programming at that time was, of course, in assembler code.

In the fall of 1959, when I returned to McGill, their own IBM 650 had just arrived, largely for the purpose of weather forecasting calculations. I was one of the few who could program it. That year I left McGill for what appeared to be greener pastures in Calgary where a fledgling university was in the making.

In Calgary I soon found that the only computer in town was a Royal McBee LGP 30, another drum machine. This was in a service bureau, and the computer was kindly put at my disposal in the evenings. On this machine I learned the high level language called ACT I. I studied the construction of its compiler, which fortunately was available, and was soon able to modify it to add extensions of my own. It was some time before I came to find out that others were calling such extensions list processing.

Around 1961 the University of Calgary acquired an IBM 1620. I became the so-called computing centre director in addition to my duties as head of the mathematics department. On this machine I again explored my list processing techniques and then used these to write what I thought was an ALGOL 60 compiler, a language that had recently interested me.

In 1962 the IFIP congress was held in Munich. At that meeting Kelly Gottlieb of Toronto saw me and suggested that there were some interesting meetings concerning ALGOL 60 and that perhaps I would like to attend them as Canadian representative. I am not sure whether what I attended was a meeting of WG 2.1 or of TC2, probably the former, but I can remember how well Bill van der Poel of Delft handled the meeting. The committee appeared to be making some decisions concerning revisions of ALGOL 60. I can remember Brian Randell talking about "thunks" and my being mystified by the concept. This was his proposed method of implementing the call by name concept.

#### ALGOL 68

In 1966 I had the first sabbatical leave of my career and chose to go to the Mathematisch Centrum in Amsterdam. There I found a compatriot from Edmonton, Barry Mailloux, as a graduate student. At that time he was busy

helping Aad van Wijngaarden in the preparation of a draft of the language ALGOL X, to be presented at the forthcoming meeting of WG 2.1 in Warsaw, a mandate which van Wijngaarden had received at the previous meeting of WG 2.1 in St. Pierre de Chartreuse in October of 1965.

I sought, and got permission to attend the Warsaw meeting and found the deliberations fascinating but the technical content mystifying, for here were the beginnings of the description of a new language using a strange new grammatical technique. What I can remember of that meeting was the insistence of John McCarthy, that any new language should have what he called overloading of operators. This would eventually become the operation declarations of ALGOL 68. His insistence that boolean expressions should be evaluated from left to right was considered but later rejected. I can remember Laszlo Kalmar making the significant comment that any programming language should incorporate lambda expressions. I can also remember an undercurrent of unease with respect to Tony Hoare, whose recent inability to agree with van Wijngaarden had begun to exclude him from consideration in the new design. This smouldering conflict was to break out later, almost in disastrous form for the Working Group.

Back in Amsterdam, I was asked by van Wijngaarden whether I would consider writing an introductory text for the new language, and, expressing interest, I was then included in the ongoing design sessions. I soon discovered that the language was far from its final formulation, and that no introductory text could be contemplated at that time. The design sessions were interesting and I volunteered to do the retyping of sheets as the document changed. Before long I found that I was putting in my own ideas and having some of them rejected but others accepted. This was encouraging.

In the spring of 1967, WG 2.1 was due to meet at Zandvoort on the Dutch Coast. At that meeting a document called "A draft proposal for the Algorithmic language ALGOL 67" was presented by van Wijngaarden, Mailloux and myself and was well enough received that the authors were commissioned to continue with the design. What I can remember of that meeting was that Nicklaus Wirth was present and presented a paper on the principles of languages design, but his words were not heeded.

In the summer of 1967 I returned to Calgary but kept in touch with the design team in Amsterdam. In the spring of 1968, WG 2.1 met again in Tirrenia near Pisa in Italy, where another document MR95 called "A working document on the algorithmic language ALGOL 68" was presented. This time there were four authors, the fourth being Kees Koster, whose special duties lay with the transport section. At that meeting I can remember long discussions on the suitability of the new two level grammar as a descriptive method and the insistence of Fraser Duncan that a method of his own devising, somewhat closer to BNF, was more suitable. Fraser Duncan did not get his way in this and at the end of the meeting the authors were again given a mandate to continue with the design and to bring it to a speedy conclusion.

Another meeting was scheduled for the fall of 1968 at North Berwick in Scotland. At that meeting we presented a document, MR99, called the "Penultimate draft report ...". Many design decisions were taken at that meeting by voting of the delegates. I can remember meeting Charles Lindsey there for the first time, and hearing him say complimentary things about the language but describe the Report as abominable. Despite his outrage, he was soon elected as a member of the group, in recognition of his remarkable document "ALGOL 68 with fewer tears", which proved beyond doubt that the ALGOL 68 defining document could be read and understood by those who were not involved with its design.

The final meeting of WG 2.1 on ALGOL 68 was scheduled for Munich just before Christmas of 1968. One of the reasons advanced for having the meeting before the end of the year was that if we did not, then the name would have to be changed again to ALGOL 69.

The meeting in Munich was dramatic. I flew over to Amsterdam a few days earlier to help with the final preparations, and was somewhat appalled to discover that some parts of the "... final draft report ..." had still to be written. For two nights before flying to Munich, we worked through the night writing, typing and Xeroxing. I can remember what a crisis it was when the Xerox machine broke down in the early hours of the morning. The day before the meeting we rushed to the plane with a suitcase full of documents, on which there was extra to pay, about fifty copies of MR100, which was the "... final draft report ...". These documents represented the culmination of three years of effort and they were to be subjected to the Working Group's critical judgement.

The Algol Working Group, WG 2.1, is notorious for trying to avoid technical discussion or the making of a technical decision. Its members seem to take great delight in interrupting on points of order or trying to show that the chairman has been inconsistent or arbitrary (which, of course, he was not).

One becomes accustomed to this sort of thing and is therefore prepared to make meaningful decisions in committee over lunch or dinner or even late at night. Perhaps this is the way most international committees work. Certainly the procedural wrangling could be interminable and every meeting was a "cliff hanger" whereby nobody knew what decisions were likely to be made until the final voting on the last day just before the delegates' planes were scheduled to depart.

The meeting in Munich was no exception. It began well enough, but soon there was a movement of discontent in which it appeared that Edsger Dijkstra, Tony Hoare, Doug Ross, Fraser Duncan, Mike Woodger and others were about to make up their minds to vote against the final acceptance of the document. They were troubled, of course, because many of them had voted in favour at the earlier meetings and they had no specific technical complaints. I can remember, at that meeting, the quiet measured tones of Dijkstra who warned that the salvation of the computer science lay in the investigation of programming technology rather than in the design of new languages. I can remember the slow peripatetic speech of Randell, who expounded on this same theme and hinted that there was a group of dissenters who were not prepared to accept the Report. These words had their effect on others and it was not long before there was a dissident group who put together a statement suggesting that the new ALGOL 68 was a mistake.

I began thinking of the several years work about to go down the drain and felt very keenly for Aad van Wijngaarden upon whose shoulders the main responsibility lay. It seemed that he had staked his career on the success of ALGOL 68 and allowed his name, and that of the institution of which he was head to be associated with it. A negative vote at that stage would be a cruel blow particularly since there was little fault to find from the point of view of technical excellence. It soon appeared that a final vote might only go in favour of the Report if it could be accompanied by a minority report from the dissenters. With this delicate compromise the voting eventually took place. The final outcome was that the Report was to be handed to TC2 with a recommendation for acceptance, but with it would be attached a minority report from the dissenting group.

In the spring of 1969, TC2 met in England to discuss the ALGOL 68 Report, together with the minority report. It was the duty of TC2 to decide whether to make a recommendation to the General Assembly of IFIP, that ALGOL 68 should be accepted and proclaimed as an international language supported by IFIP. At that time I was also the Canadian representative on TC2, so I was present at that meeting. It was a tense meeting, and in the final count there was only one dissenting vote cast by Fraser Duncan representing the United Kingdom. When a year later I asked Ian Currie, of the Royal Radar Establishment why the UK was the only country to vote against the ALGOL 68 Report, yet was the first country to implement the new language, his reply was "Well, they never asked me about it". Soon after this the ALGOL 68 report was published in Numerische Mathematik, and was later translated to Russian, German, French and Bulgarian.

## **The Revised Report**

After the events of which I have spoken above, the hectic days of international travel and careful editing were over for a while. In September of 1969 I hosted a meeting of WG 2.1 in Banff, Canada, and then in the summer of 1970 the working conference on ALGOL 68 implementation was held in Munich. I believe that those in Munich had chosen to host this conference because they were busy with an ALGOL 68 implementation and expected it to be ready. But when the time of the conference arrived, delegates were surprised to discover that the first implementation was already completed at the Royal Radar Establishment in Malvern England. The implementers were present in Munich and were able to tell us that they already had a happy band of users. This was on the ICL 1900 series machines. This compiler is now in widespread use especially in England.

I was chosen to be editor of the proceedings of the Munich conference, a job which took up much of my spare time for some months. Unfortunately, when the book came out, it was noticed that a printers devil had been at work, for the well known text by Randell and Russell, on ALGOL 60 implementation, which appeared as one of the references in a paper, was given in such a manner as to suggest that Randell, a well known opponent of ALGOL 68, had written a book about its implementation. I hope that Brian Randell has forgiven me for this.

Working Group 2.1 met between 1970 and 1972 in Vienna, in Novosibirsk, in Manchester, in Fontainebleau and in Belgium. At each of these places it deliberated on the various imperfections in the language, as defined, considered the complaints of the implementers, and came up with some new ideas which would make the language neater for the user and easier to implement. As a result of this introspection, it was resolved that a revision of the Report should be called for. The original four authors were asked to cooperate with Michel Sintzoff, Charles Lindsey, Lambert Meertens and later Richard Fisker, to make a panel of eight editors for the revision, two from Canada, three from the Netherlands, two from England and one from Belgium.

At that time I applied for funds from NATO to bring these scientists together, and my application must have impressed the NATO committee with its aspect of international cooperation, for the money was granted. With the help of these funds, Michel Sintzoff, Charles Lindsey, Barry Mailloux and I spent several weeks of the summer of 1972 at UBC in Vancouver, where the foundation of the Revised Report was laid.

We made an early decision that we would take full advantage of computer technology in word processing to help us with the clerical work in developing the new Report, with the eventual hope that it could be typeset directly from a computer file, thus avoiding the long and tedious process of proof reading. Accordingly all our editing work was done using the MTS operating system running at the USC Computing Centre. This system enabled us to produce most of the indexes and to format the grammar and the other parts of the text automatically.

It was during this time that Michel Sintzoff, after closeting himself in his study for several days, showed us the germ of his remarkable NEST syntax, a device whereby he was able to express the difficult context dependent part of the language definition by a few syntactic rules. This was a significant piece of work, for although we had known for some time that two-level grammars are powerful, nobody as yet had harnessed that power to the purpose of making the identification problem a practicable proposition. Observing this breakthrough, we were then resolved that the Revised Report should make use of it fully. In language that the user might understand, this new device enabled us to describe the revised ALGOL 68 in such a way that all errors that might occur at compile time are syntactic errors. In other words, it became possible that the action of the compiler itself, including the lookup of identifier tables, could be expressed as part of the grammar.

That summer Helmut Goldie of Seattle invited us to his beach cottage on the west coast of Washington State. A few days before this little outing, we had been wrestling with the vexed question of how to deal with infinite modes, i.e., those which are expressed recursively, and how to tackle the problem of equivalence of such modes. This was an area in which the original Report had allowed some mathematical inconsistencies. Charles Lindsey and Barry Mailloux were walking up and down the beach and they returned with the exciting news that it could perhaps be done. It was several months before this very difficult piece of syntax was eventually worked out to our satisfaction, and the outcome will be found in the Revised Report in that section dealing with the equivalencing of modes.

The editors met again for a short time in England in the winter of 1972 and three of us returned to Canada, on the same plane, just before Christmas. I can remember that we landed in Montreal, rather than in Toronto, in a heavy snowstorm and were snowed in there for a day. It was at that time, while waiting in the airport, that Michel Sintzoff explained to Mailloux and myself how he thought he could solve the very difficult problem of the identification of operators in ALGOL 68. This was the last outstanding syntactic problem of importance. With the help of his solution, we were then able to complete the syntactic description in the manner in which we had hoped.

The summer of 1973 was spent in Edmonton where six of the editors: Lindsey, Sintzoff, Mailloux, Meertens, Fisker and I were getting all the final details worked out, again using the word processing facilities of the MTS operating system. Our target day was for August of that year, when WG 2.1 was due to meet in Los Angeles, to deliver a final verdict on the Revised Report. That summer I can remember many heated arguments when voices rose higher and higher in support of some point of view. At that time someone would suggest it was time to eat, and we often found that a good meal would lead to a calmer appraisal of our difficulties.

In Los Angeles there was again a final vote, but this time without bitterness. The Revised Report was eventually published in Acta Informatica in 1975, but directly from the computer files that we had generated.

## **Conclusion**

The Chairmanship of WG 2.1 was in the hands of Bill van der Poel up to the dramatic meeting of 1968. At that time Manfred Paul of Munich became chairman, and it was under his guidance that the Revised Report was generated.

In 1974 the group needed a new chairman, and I was chosen as perhaps the least controversial of several possibilities. Now the chairmanship has passed to Robert Dewar of New York University.

Working Group 2.1 has met in Breukelen (The Netherlands), Munich, Oxford and in Poland, but at the present time there does not appear to be a new product on' the horizon.

One may speculate on the suitability of an international committee for the design of programming languages. It is true that the design of anything by committee is difficult. The fact that the committee is international does not, I believe, create any special difficulty. Of course, it is important that there should be a small editorial group, one member of which is responsible for all decisions made.

I believe that the design and revision of ALGOL 68 was an important piece of work which has made a significant contribution to the advance of computer science. The fact that it has not caught on as a practical programming language as well as one might have liked is a matter for regret, but is not altogether unexpected. I believe that in any future design of a programming language by such a committee the following guidelines should be followed .

- a. For each well known programming language currently in existence, there should be at least three members of the committee who have used it and know it well.
- b. That the language should be kept as simple as possible, and there should be great resistance to the temptation of introducing new but as yet untested ideas.
- c. That a portable implementation of the language should go hand in hand with its design, and that each member of the committee should have such an implementation available so that he can experiment with it. It might turn out, of course, that rigid adherence to these principles could make the design project impossible to complete, so some compromise may have to be made.

In conclusion, I should add that this is far from the complete story of ALGOL 68. I have described events from my own point of view, and dwelt on only a few incidents which were of human interest. I hope that you have found some of these anecdotes of interest.