

# A Brief History of Compilers

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# Preface

The history of compilers is rich and deeply connected to the broader history of computing, however, I believe that no comprehensive work has tied together the threads of this history with a specific focus on compilers. There are wonderful retellings of the very first compilers on Konrad Zuse's Z4 computer at the ETH Zurich and Grace Hopper's pioneering work on the A-series compilers for the UNIVAC I, and John Backus' work on the first commercial compiler for FORTRAN at IBM, but these are often isolated stories. When they are woven together, they are often not connected to the next developments in compiler technology at Bell Labs: Aho and Ullman's *Principles of Compiler Design*, the development of Lex and Yacc, the C programming language, Bjarne Stroustrup's first C++ compiler front (inspired by Alan Kay's vision of object-oriented programming). The subsequent decades of open-source compiler development, advances in optimization techniques, and the explosion of new programming languages and compilation paradigms (e. g. just-in-time compilation) are then followed by the rise and necessity of hardware-software code-sign and domain-specific languages seen most evidently in projects based on MLIR and LLVM. The threads between these points in history offer a deeper understanding of each individual piece and context that motivates modern compiler development.

There have been several monuments works on the history of computing and a few on the history of programming languages, but in the two and a half decades since the turn of the century there have been ample developments in compiler technology that no comprehensive work has covered thus far. This book aims to fill that gap to a small degree; it is not exhaustive, but contextualizes many of the most important recent developments in the larger narrative of compiler history. The introduction chapter contains a brief version of the entire book; the reader is encouraged to read it first. This work intends to weave these threads together.

Compiler technology has been handed down and built upon by generations of computer scientists and engineers; while prior works have primarily focused on individuals or companies or have covered computer history more generally, this book follows the individuals as they develop compiler technology, and follows it to the next generation, from the genesis of the first compilers to the present day. My hope is that each chapter stands on its own, but that reading them in order will give a more complete picture. The reader ought to be able to read a particular chapter that suits their needs at the time. This book does not assume the reader is deeply familiar with compiler engineering or computer science. The book is structured as a chronological narrative of the history of compilers, intending to keep the focus on compiler technologies and the people behind them without focusing on any particular company or product.

# 1

# Introduction

While the modern programmer may consider the term *compiler* to be a specific one, it is still often misunderstood. Moreover, in the beginning, the term was more ambiguous than it is today. Brian Kernighan described compilers in this seemingly general way [56]:

A compiler is a program that translates something written in one language into something semantically equivalent in another language. For example, compilers for high-level languages like C and Fortran might translate into assembly language for a particular kind of computer; some compilers translate from other languages such as Ratfor into Fortran.

However, this is not sufficiently general. Two counter examples are  $\text{\TeX}$  and METAFONT, which are compilers that transform text into PDF documents and renderable fonts, respectively. Transforming text into an executable program is not the same operation as transforming text into a document or font, yet both are considered compilation. When students are first introduced to compilers, the first sort of program they are contrasted with is interpreters. This distinction is not necessarily meaningful. The conventional notion of an interpreter is a program that performs the actions specified by the source code as chunks of the code are consumed; perhaps this was the case with early BASIC interpreters and it may be a useful mental model when introducing interpreters to new programmers, but one is hard-pressed to find a modern interpreter that does not perform a compilation step. The most popular interpreters for today's most popular interpreted languages, Python and Javascript, both use relatively sophisticated compilation techniques. There are even Python *libraries* that perform just-in-time (JIT) compilation, targeting CPUs, GPUs, and other specialized hardware[13][77][100][116].

To capture the full spectrum of compiler technologies, the definition we will use in this book is intentionally broad:

*A compiler analyzes, transforms, and produces code, based on source code.*

In the case of  $\text{\TeX}$  and METAFONT, the source code may not entail a *program* in the conventional sense;  $\text{\TeX}$  source code describes a document rather than a sequence of instructions to be performed. The code being produced by the compiler may be the encoded PDF format, for instance. So too are interpreters which produce code in some form during the interpretation process. The CPython interpreter produces bytecode before executing the program, meaning it is a compiler by our definition. Perhaps CPython is a compiler the consumes Python code and produces CPython bytecode, which also happens to ship a CPython bytecode virtual machine which typically executes the bytecode as soon as it is produced.

While teaching a course on compilers at Columbia University, one of Alfred Aho's students wrote a compiler called Upbeat which produces music based on input data; given input data in

some format, Upbeat produces output code in the form of music[3]. The student's final presentation was to set the input data to the ticker for some symbol from the New York Stock Exchange, playing happy and upbeat music whenever the ticker went up, and sad depressed music when it went down. Another Bell Labs creation was the connection of two programs: a program that translated numbers into words (e.g. 123 to "one hundred twenty three"), and a program that turned text into data representing sound waves. Connecting these two compilers (on that compiled text with numbers into text with words sounding out the numbers, and one that compiled text into sound waves) allowed Bell Labs employees to send messages that would be turned into sound waves and broadcast on a speaker in the computer room. The point of this section is not pedantry, but to establish a broad definition of compiler technology before embarking on the details of its development. We will primarily focus on compilers that have run on a machine. We largely exclude theoretical works like Ada King Lovelace's notes on calculating Bournoulli numbers and the development of automata theory, for instance. Each step in the development of compiler technology builds on the previous steps, and while the prior steps are important, our focus is on compiler programs and not the theoretical works that preceded them, except where especially relevant.

## 2

# Dawn, 1940-1960

### 2.1 Where Does it Start?

There is significant debate about who created the first compiler, in no small part due to ambiguous nomenclature. The term *software* came into use sometime between 1959 and 1962, as Rojas and Hashagen note:

[Expressions] such as "hardware", "software", "machine language", "compiler", "architecture" and the like... were unknown in 1950. They only arrived a decade later, but the underlying concepts were quite familiar to us. [106]

This Honeywell advertisement *A Few Quick Facts on Software* sought to clarify these terms as well:

Software is a new and important addition to the jargon of computer users and builders. It refers to the automatic programming aids that simplify the task of telling the computer 'hardware' how to do its job...[56, ch.5]

At this time, hardware was the only piece that mattered to customers. Software was an afterthought, if a thought at all. The instruction set of the machine was important, because that was the user's interface to the machine. It should come as no surprise, then, that the origins of our modern understanding of the term *compiler* are similarly murky, especially considering the fact that *compiler* already carried meaning in English, and was repurposed for computing. John Backus even pointed out how the ambiguity around the term *compiler* makes computing history ca 1950s especially difficult to untangle:

There is an obstacle to understanding, now, developments in programming in the early 1950s. There was a rapid change in the meaning of some important terms during the 1950s. We tend to assume that the modern meaning of a word is the same one it had in an early paper, but this is sometimes not the case. Let me illustrate this point with examples concerning the word "compiler." [21]

One could argue that any of these efforts constituted the first compiler:

- Konrad Zuse's run-programs for the Z4 at the ETH Zurich in 1950
- Grace Hopper's A-0 and A-1 compilers for the UNIVAC I at Remington Rand in 1951
- Laning and Zierler's algebraic compiler for the Whirlwind at MIT in 1950

- John Backus's FORTRAN I compiler at IBM

I attempt to discuss these in order, however their efforts overlap significantly in time. I try to tell their stories as a whole, though each story contains references to the others; if you find yourself confused by names introduced out of context, please finish the chapter or search for the content within this chapter before giving up.

## 2.2 Development of the Z4

Konrad Zuse, a German civil engineer, began work on the Z4 during World War II. Funded partially by his family and partially by the Nazi government, his prior works demonstrated significant creativity and ingenuity, and they were leveraged to build precursors to modern cruise missiles and guided bombs. Most of Zuse's machines prior to the Z4 were destroyed during the war. [TODO: Zuse's Z4 was a strange machine with bespoke memory and instruction set. This affected how the compilers for it were designed.] In a turn of events Konrad Zuse was made aware of Aiken's Mark I through his daughter:

Konrad Zuse told me an amusing anecdote about how he first encountered the work of Aiken. The occasion of our conversation was a luncheon in Zuse's honor, hosted by Ralph Gomory at the Watson Research Laboratory of IBM before a lecture given by Zuse to the staff of the lab. When Zuse learned that I was gathering materials for a book on Aiken, he told me that he had come across Aiken and Mark I in an indirect manner, through the daughter of his bookkeeper. She was working for the German Secret Service (Geheimdienst) and knew through her father of Zuse's work on a large scale calculator. According to Zuse, the young woman never learned any details about his machine, which was shrouded in war-time secrecy. But she knew enough about Zuse's machine to recognize that the material filed in a certain drawer related to advice that seemed somewhat like Zuse's. She reported this event to her father, giving the file number of the drawer, and the father at once informed Zuse of her discovery. Zuse, of course, could not go to the Secret Service and ask for the document since that would give away the illegal source of his information. Zuse was well connected, however, and was able to send two of his assistants to the Secret Service, armed with an official demand for information from the Air Ministry, requesting any information that might be in the files concerning a device or machine in any way similar to Zuse's. Zuse's assistants were at first informed that no such material existed in the files, but they persisted and eventually got to the right drawer. There they found a newspaper clipping (most likely from a Swiss newspaper), containing a picture of Mark I and a brief description about Aiken and the new machine. But there was not enough technical information to enable Zuse to learn the machine's architecture. [44]

## 2.3 The ETH's Acquisition of the Z4

There were several early efforts to create programs that produced punch cards which contained machine code instructions, which could then be fed back into the machine as input punch-

cards. The programs produced by these early compilers were called *run-programs*, and the process of using them was called *automatic programming*, a term later coined by Grace Hopper. The first of these programs was run on a machine called the Z4, designed by Konrad Zuse in Germany. Professor Eduard Stiefel, shortly after establishing the Institute of Applied Mathematics to study numerical analysis at the Swiss Federal Institute of Technology (ETH) in Zurich, began searching for a computer for the institute. He learned of the computing advancements in the United States, Great Britain, and Germany, but no machines were readily available at the time. He sent his assistants Heinz Rutishauser and Ambros P. Speiser to the US to study the latest developments in computing; they spent most of 1949 with Howard Aiken at Harvard and John von Neumann at Princeton.

Before we returned, that is, in the middle of 1949, Stiefel was informed about the existence of Konrad Zuse's Z4. At that time Zuse was living in Hopferau, a German village near the Swiss border. Stiefel was told that the machine might be for sale. He visited Zuse, inspected the device, and reviewed the specifications. Despite the fact that the Z4 was only barely operational, he decided that the idea of transferring it to Zurich should by all means be considered. Stiefel wrote a letter to Rutishauser and me (we were at Harvard at the time), describing the situation and asking us to get Aiken's opinion. Aiken's reply was very critical - the future belonged to electronics and, rather than spending time on a relay calculator, we should now concentrate our efforts on building a computer of our own. [111]

The Z4 was a bespoke machine with unique components; the computational logic was wired together with telephone relays and the memory was entirely mechanical.

The Z4 could be used as a kind of manually triggered calculator: the operator could enter decimal numbers through the decimal keyboard, these were transformed into the floating point representation of the Z4, and were loaded to the CPU registers, first to OR-I, then to ORII. Then, it was possible to start an operation using the "operations keyboard" (an addition, for example). The result was held in OR-I and the user could continue loading numbers and computing. The result in OR-I could be made visible in decimal notation by transferring it to a decimal lamp array (at the push of a button). It could also be printed using an electric typewriter. [105]

It notably featured instructions for conditional branching and subroutine calling, which both proved essential for the compiler development that would follow at the ETH. Stiefel was undeterred by Aiken's criticism, and convinced the ETH to purchase the Z4. In 1950, Heinz Rutishauser at Switzerland's ETH obtains a Z4.

We also made some hardware changes. Rutishauser, who was exceptionally creative, devised a way of letting the Z4 run as a compiler, a mode of operation which Zuse had never intended. For this purpose, the necessary instructions were interpreted as numbers and stored in the memory. Then, a compiler program calculated the program and punched it out on a tape. All this required certain hardware changes. Rutishauser compiled a program with as many as 4000 instructions. Zuse was quite impressed when we showed him this achievement. [111]

Thus were the first run-programs produced. This is what we will consider **the first compiler**, though it was not called that at the time. Shortly after Stiefel's assistants' stints in the US and correspondence with Aiken, one of Aiken's engineers would find considerably more success exploring related ideas.

## 2.4 Grace Hopper

While Grace Hopper may not have been the first to create a program that punched out another program as its output, she pioneered the field of compilation to the extent that many consider her the inventor of compilers. Her innovations were also more readily adopted than those at the ETH. Consider Figure 2.8, and the pace of development in Hopper's time compared to the years prior. Note that while we have ample data on *how* Hopper's compiler worked and how she and her team developed it, the intuition behind those developments is foggy at best. We have recollections from Hopper and her contemporaries, but only from long after the fact. It was not understood at the time how important her work was, so we have only to speculate and piece together oral histories.

Originally a mathematics professor at Vassar College, Hopper obtained waivers for her age and weight and joined the U.S. Navy in 1943, eventually graduating first in her class from Midshipmen's School. She was assigned, somewhat unexpectedly, to Commander Howard Aiken's Harvard Computation Laboratory in 1944 as the third programmer of the Automatic Sequence Controlled Calculator (Mark I), the world's first operational computer. Although it was significantly slower than the ENIAC, it was *programmable*; the ENIAC had to be physically rewired to change its program. To write a compiler for the ENIAC, one would need to plug the phone lines in the back of the machine together to create the compiler, feed in the input program as data, and a human operator would have to take the punchcards it produced and manually rewire the machine to run that program.

Aiken built the Mark I in collaboration with IBM, though it is unclear how much either side contributed in its development. The proportion of credit given to either party would be disputed in numerous documents and press releases in the following years, including the *Manual of Operation for the Automatic Sequence Controlled Calculator*, the technical manual for the Mark I. In the fall of 1944, Aiken decided that his team needed to produce a book documenting the technical developments at the Harvard Computation Laboratory and how the Mark I was intended to be used. For this task, he chose Hopper; though she protested, her reputation as a clear and thoughtful communicator and writer had already earned her the job. Hopper was known for her writing ability, and it was a point of emphasis in her teaching career. She would assign her mathematics students onerous writing tasks to emphasize that "it was no use trying to learn math unless they could communicate with other people." [30, interview on 5 July, 1972]

Aiken and Hopper both understood that, for the Mark I to be a success, it would be used and understood by a large and diverse audience, and for that to happen, they needed a detailed, compelling, and accessible manual.[96]:

### 2.4.1 Hopper and the Mark I's Manual

Here we drift into some general computing history, mostly because it was so formative for Hopper, who was in turn so formative in the development of compilers. Her manual for the Mark I began with a detailed and dramatic retelling of computing history, opening with the following quote from Charles Babbage and culminating with the Mark I:

If, unwarmed by my example, any man shall undertake and shall succeed in really constructing an engine embodying in itself the whole of the executive department of mathematical analysis upon different principles or by simpler mechanical means, I have no fear of leaving my reputation in his charge, for he alone will be fully able to appreciate the nature of my efforts and the value of their results.

Her history went on to cover:

- Blaise Pascal's counting machines, "foundation on which nearly all mechanical calculating machines since have been constructed."
- Leibniz; stepped wheels system for mul/divs.
- Charles Babbage; most significant part of the manual dedicated to him. Difference engine, idea for computing machine. Invented punch card system to feed in information, made after textile looms. G H emphasized the machine would take 2 decks of cards, one for data, one for instructions (not von neumann).
- Ada King, Countess of Lovelace; series of essays on Babbage's machine. described possibly the first computer program. This could never run and would have to wait for the Mark I before the dream could come true.
- Aiken's Mark I

At one point, all new hires into Aiken's lab were required to read Charles Babbage's autobiography. Hopper was first exposed to Ada King in this text: "she wrote the first loop. I will never forget; none of us ever will." Their coworkers in the Harvard lab would jokingly cast Aiken as Babbage and Hopper as Ada King. Aiken ran a rigidly hierarchical and meritocratic lab, which allowed Hopper to produce quality work and placed her on more equal footing with her male coworkers; Aiken openly disliked being assigned a female officer, but Hopper's competence outweighed any such sentiments. Her competence did not, however, shield her from the pressures of the environment. She leveraged her computing knowledge to assist the war effort, which included the bombing of Nagasaki. The stresses of the war effort and Aiken's overbearing management drove her to substance abuse in this time period. In 1946, Commander Edmund Berkeley wrote a report on the conditions at the Harvard Computation Laboratory, which perhaps contextualizes her incapacity to cope.

In his report, Berkeley systematically detailed the unfavorable conditions at the Computation Laboratory, including the length of the work day and the isolation of the staff from similar projects at MIT and the University of Pennsylvania. He named eleven talented people who had left or been dismissed by Aiken between August 1945

and May 1946, noting that all were "very bitter over the conditions on the project." The root of the problem, according to Berkeley, was that "in the Computation Laboratory there is no provision for appealing any decision or ruling whatsoever made by the project manager." He was amazed that no one at Harvard and no one in the Navy seemed to have jurisdiction over the rogue director, so that Aiken was able to rule with near absolute authority.

#### 2.4.2 Postwar Collaboration

Hopper was relieved from active service in 1946, but she joined the Aiken's lab to continue working on Aiken's Mark II (a paper-tape sequenced calculator) and Mark III (an electronic computer with magnetic drum storage). As the war effort wound down, Aiken and his laboratory found themselves growing in stature. He had the authority to move military personnel to his Harvard laboratory at his discretion, which he did. He also expanded the reach of his lab's influence by opening up the computing community. During the war, research and development of computers and programming was closely guarded, but after the war ended, cross-organization collaboration was possible. Aiken started the *Symposium on Large Scale Digital Computing Machinery* in 1947 to foster this collaboration. By this time there were numerous other organizations with computing projects underway in the United States, which Aiken was now permitted to collaborate with:

- Eckert-Mauchly Computer Corporation (EMCC), BINAC and UNIVAC
- Harvard, Mark II and Mark III
- IBM, SSEC
- MIT, Whirlwind
- Institute for Advanced Study, MANIAC
- Engineering Research Associates (ERA)

We'd all been isolated during the war, you see, classified contracts and everything under the sun. It was time to get together and exchange information on the state of the art, so that we could all go on from there.

Hopper's postwar fellowship with the laboratory ended in 1949; after a brief stint of unemployment, she joined a startup called the Eckert-Mauchly Computer Corporation (EMCC) where she found a more congenial environment to continue her work on compilers.

According to her friend and former Harvard colleague Edmund Berkeley, Hopper turned to alcohol during this period as a way to deal with the compounding pressures at the Harvard Computation Laboratory. She had dedicated herself fully to the overwhelming task of bringing Howard Aiken's machines to life. She used the machines to solve critical military problems, including one that resulted in an explosion over Nagasaki. As the psychological strains became increasingly pronounced, alcohol seemed to serve as an effective outlet, freeing Hopper to express emotions

and to temporarily forget obstacles real and imagined. According to Berkeley, the expiration of Hoppers Harvard research contract was the best thing that could have happened to her, although in the short term unemployment added to the stress. During the last week of May 1949, the 43-year-old programmer packed up her belongings, headed to Philadelphia, and bet her future on two younger men who believed they could create the first commercial computer company. [30]

Once it was obvious to everyone in the industry that Hopper was done at Harvard, she had a flurry of offers, but she chose EMCC because of her impression of John Mauchly:

In 1949 when people knew I had run out the time at Harvard, (and I guess everyone in the industry knew it) practically everyone asked me to come for interviews, including IBM. I went to the IBM headquarters and they gave me a huge [offer]. I was one of the very few people who did not work for IBM. I went for interviews with practically every computer manufacturer that there was at the time. Honeywell, RCA was thinking about it, Burroughs was in it. But it was John Mauchly I just couldn't miss. Working for him was obviously going to be a great pleasure. He was a wonderful guy, one of the best that ever lived. [64]

### 2.4.3 Hopper at the EMCC

The company Hopper joined was one of the earliest pure computer-focused ventures, founded by J. Presper Eckert Jr. and John Mauchly (designers of the ENIAC, or the Electronic Numerical Integrator and Computer). This startup environment contrasted sharply with the academic rigor of Harvard and the industrial scale of IBM. There she found an open-minded and welcoming environment to develop her ideas; Mauchly, who was to become Hopper's boss, was characterized as "very broadminded, very gentle, very alive, very interested, very forward looking," [30] creating a tolerant, flexible company atmosphere in contrast to the pressure she experienced at Harvard. A majority of their programming staff consisted of mathematically inclined women who had served as ENIAC operators at the Moore School. When Hopper arrived in 1949, EMCC had two major projects underway: the BINAC (Binary Automatic Computer), which was close to completion, and the UNIVAC I (Universal Automatic Computer), which would be running within a year. The work environment and upcoming UNIVAC project excited Hopper and enticed her to join the company after walking out of an interview with IBM; she felt that IBM was too close to Aiken's lab. While the organization was grounds for fruitful and innovative research and development team, EMCC was under financial strain; they depended on partial payments for UNIVAC-I orders to stay afloat. The unexpected death of EMCC's chairman Henry Straus forced Eckert and Mauchly to seek a buyer, which they found in 1950 with Remington Rand, a typewriter and office equipment manufacturer. In 1955, Remington Rand merged with Sperry Corporation to form Sperry Rand.

Another Hopper programmer, Adele Mildred Koss, was assigned to Commonwealth Edison when the utility approached the Chicago sales office concerning a potential purchase of a UNIVAC for billing and payroll. At the time, Koss was 7 months pregnant and working part time. Since her pregnancy precluded travel, Commonwealth Edison management was forced to come to Philadelphia in order to discuss

their billing needs. In the end, the utility did not buy a UNIVAC, but instead purchased an IBM701 when it became available. Koss recalled: "I remember GraceHopper's memo to management saying 'This is a multi-million dollar client and you are not treating them like one. You have only assigned a part time programmer to work with.'" [30, Adele Mildred Koss, interviewed by Kathy Kleiman]

Rand's team did not have nearly the programming expertise nor the personnel to support their customers. Compounding with these challenges was the resistance Hopper's team faced from the new Remington Rand management. Remington Rand was a typewriter company and their management was far more familiar with the mechanical punchcard technology of the previous generation of computers than the UNIVAC's magnetic-tape memory. Even Thomas Watson Sr. had similar inclinations about magnetic-tape memory:

Having built his career on punch cards, Dad distrusted magnetic tape instinctively. On a punch card, you had a piece of information that was permanent. You could see it and hold it in your hand. Even the enormous files the insurance companies kept could always be sampled and hand-checked by clerks. But with magnetic tape, your data were stored invisibly on a medium that was designed to be erased and reused.[30]

Hopper and her team at Remington Rand developed three "compilers" in rapid succession, the A-0, A-1, and A-2, for the UNIVAC I. I quote "compilers" because the A-0 and A-1 were not compilers in the modern sense. Her work was grounded in intellectual openness, collaboration, and accessibility; she pioneered the debuggability of programming languages, compiler error reporting, and new ways to share code and collaborate, for example.

Hopper's recollections point to motivations ranging from an altruistic desire to allow "plain, ordinary people" to program to dealing with her own laziness. Naturally one must be skeptical of such claims, for they were made years after the fact. In 1951 it was difficult for even a visionary like Hopper to imagine the eventual ubiquity of computer technology, and one can be pretty confident that Hopper was not a lazy person. [30]

Shortly after the fiasco with the utility company, Hopper's team was tasked with supporting UNIVAC I customers at the US Census Bureau, a task she thought no one in the company was prepared for. She began work on the A-0 in October 1951 in her spare time in order to address this mounting crisis facing Remington Rand: they were unable to fully support their customers, and their sales teams were, to put it kindly, incompetent with respect to their product, and the sales team supported their customers as well as one might expect. Management was as probably as receptive to her ideas about compilation as they were to the UNIVAC I's magnetic-tape memory:

Inspired by Holberton's Sort-Merge Generator, Hopper conceived the idea of writing a program to create a program, or in modern day terms, building a compiler. The idea was to get commonly used subroutines automatically inserted into another program based on calculated offsets. Most people at the time considered this impossible. [55]

As Hopper later recalled:

The Establishment promptly told us, at least they told me, quite frequently that a computer could not write a program; it was totally impossible; that all computers could do was arithmetic, and that it couldn't write programs. [62]

Hopper was not the only member of the programming group with ideas about programs generating other programs:

[Betty Holberton]'s retired. I think she's still part time at the National Bureau of Standards. Everybody's forgotten that she wrote the first program that wrote a program. She wrote that sort-merge generator, and what she did was feed in the specs for the data you were handling and the keys and that sort of thing, and then it generated the sort program for that specific data. That's the first time to my knowledge that anyone used the computer to write a program. Betty did that. I don't think she's ever fully received the credit for what she did in that case...

I'm not sure that I would necessarily have gotten done what I did get done if she hadn't been ahead of me, so to speak. Knowing that she had used a program to generate a program, I had a good deal more nerve to go ahead and build the first A-O compiler.

Hopper and the team had decided that the atom of a programming language ought to be the command imperative; verbs and nouns. All programmers, no matter their native language, should be able to understand the verbs acting on nouns. Thus they began working on their first mnemonics, the inputs to the A-0 compiler.

#### 2.4.4 The A-0 Compiler

At this time we should note that the term *compiler* had not yet taken on its modern meaning. Hopper used the terms *automatic programming* and *compiler* to refer to programs that produce other programs, but they did not do the jobs that we associate with compilation today. Once Hopper and her team had developed an environment of collaborative programming, they ran into new problems with re-using each other's code.

On each of these routines they started with zero, which when you put them into a new program you had to add every one of the addresses to position it in the new program. Programmers could not add. There sat that beautiful big machine whose sole job was to copy things and do addition. Why not make the computer do it? That's why I sat down and wrote the first compiler. It was very stupid. What I did was watch myself put together a program and make the computer do what I did. [64]

John Backus had this to say about Hopper's A-2 compiler in 1976:

The above items give some idea of what the word "compiler" meant to one group in early 1954. It may amuse us today to find "compiler" used for such a system, but it is difficult for us to imagine the constraints and difficulties under which its authors worked [21]

Given that the A-2 was more sophisticated than the two prior iterations, this should tell us something about how far their notion of a compiler was from our present day understanding. Let us turn to the 1952 paper *The Education of a Computer* in which Hopper announced her A-0 compiler, which she presented at a Pittsburgh ACM meeting[66].

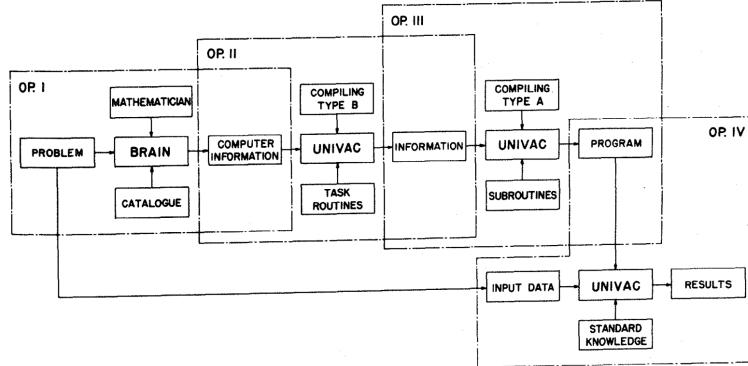


Fig. 6.- COMPILE TYPE B AND TASK ROUTINES

Page 245

Figure 2.1: Depiction of *type-B compilation* from Hopper's *The Education of a Computer*

In this paper, she dubbed her A-0 as a compiler because it was compiling subroutines from a library into a program, adjusting offsets as necessary. This is far closer to our modern notion of a linker; its role was to copy machine code from different locations into a single output program based on an input program *that was still mostly in machine code*, save for the references to library subroutines. Her notion of compilation was perhaps closer to the regular English meaning of compilation, like that of compiling research papers into a book. Her hope was that "the programmer may return to being a mathematician," though her A-0 did not lift the programmer away from machine code to nearly the same extent as her subsequent efforts would [66]. One may also consider this effort to be the first *standard library*, which would become a major feature of later compilers and programming languages. This set the stage for compilers and programming languages providing a default set of useful routines for programmers to pull from.

The cost of renting a computer remained high relative to the labor cost of hiring a programmer, thus it was not always economical for computing centers to use compilers at first. Richard Ridgway and several members of Hopper's team began testing the A-0 against hand-written programs in the summer of 1952 [103]. For this study, Richard compared the computer and labor time spent to calculate a table of values for the equation:

$$y = e^{-x^2} \sin\left(\frac{x}{2}\right) \quad \text{for } |x| < 1, \Delta x = 0.01.$$

After an analysis of the same program written by hand and by the A-0, he concluded that the A-0 (already considered "antique" by 1952, not more than two years after Hopper began working on it) was cost-efficient to use for most workloads. Richard writes in *Compiling Routines*[103]:

Thus, while more UNIVAC time may be required for the numerical solution of a problem as programmed by UNIVAC, more UNIVAC time, in toto, is consumed by

the conventional method. This remains true until the entire problem including this self-contained repetitions is to be repeated, in this case at least eighteen times.

If the total preparation time is considered, the problem must be repeated some 800 times before the conventional programming method overtakes the compiler method. In this case, the compiler used was the "antique," or A-0, the first to be constructed and the most inefficient.

#### 2.4.5 The A-1 and A-2 Compilers

As Hopper and her team began work on the subsequent A-1 and A-2 compilers, their motivations shifted away from reducing the tedium of programming to the economic costs of programming (as is seen in Richard's report). While computing time was initially far more costly than human time, as the proportion of computing costs dedicated to human labor increased over time, the importance of reducing human labor increased as well. Her team began working on the A-1 and A-2 in 1953, and the A-2 was available to UNIVAC customers by the end of the year. Hopper recruited Herbert Mitchell and Richard Woltman from Aiken's lab to lead the development of the A-1 and A-2 compilers. Margaret Harper, Frank Delaney, Mildred Koss, and Richard Ridgway were the primary developers of these compilers.

There were a number of significant innovations between the A-0 and A-2. The A-2 performed more of the jobs we associate with compilers today. Most significantly, by 1954, the A-2 compiler accepted source code in the form of *pseudocode*, which was a (slightly) more human-friendly format than plain machine code. [TODO: Some sources reference the A-2 compiler instruction manual, but I'm unable to find this source online.] John Backus described the A-2 compiler's 1954 May update as a significant improvement because of these pseudocode instructions [19]. He placed the A-2 with Laning and Zierler's algebraic compiler and his own FORTRAN I compiler as the primary compilers of significance in the mid 1950s.

Along with pseudocode instructions, this compiler also produced twelve-character error codes to inform the user why something went wrong during compilation, which must have been tremendously helpful when users had been accustomed to the alternative. The compiler worked in two phases, first constructing an index of the program and then producing the output machine code, informing the user as it did so. The compiler should be friendly to its users was not necessarily a given at the time; modern compiler tools owe this to Hopper (to the extend that they are any better than the A-1). Another feature of pseudocode instructions is that programs written in such a manner may be translated to run on different machines, provided the compiler and standard library are available on those machines. As far as we know, Hopper never attempted this, however.

By winter of 1953, the A-2 compiler was already being used heavily by several institutions, including the US Census Bureau and Lawrence Livermore National Laboratory. Several of these provided feedback, bug reports, and occasionally features back to the Remington Rand team. Nora Moser of the Army Map Service sent Hopper a collection of compiler improvements, library enhancements, and altogether new libraries in the winter of 1954. One cannot overlook the fact that Hopper was a woman in a male dominated industry, and a number of other women in the industry were her early collaborators.

Hopper presented on the A-2 at the Pentagon in 1953 [50], facilitating communication with and amongst her users. This was not the only time she sought to present her team's work to a

wider audience. Non-UNIVAC customers also expressed interest; more government agencies and potential customers (with existing IBM machines) came to learn about their developments. This open collaboration and sharing of information would set a precedent for intellectual property in the budding software industry. She had built the compilers as a way to make programming accessible and facilitate the sharing of code, and this philosophy extended to all sorts of information. She had been successful in a more restricted environment at Harvard, and she was able to see clearly the merits of a more open industry.

#### 2.4.6 After the A-2: A-3, AT-3, MATH-MATIC

[TODO: Should make some mention of interpreters in here. Hopper made some observations that might be useful later.]

Not everyone was supportive of the advancements in compiler technology. There was a significant chunk of the computing industry that thought programming took too much creativity to be fully automated. Hopper and John Backus found themselves on the same side of the debate, both firm in their beliefs that computing should be made accessible. This is a bit ironic, because IBM would shortly mount a significant effort towards compilation and competition with Remington Rand, with Backus at the helm.

Just as freewheeling westerners developed a chauvinistic pride in their frontiersmanship and a corresponding conservatism, so many programmers of the freewheeling 1950s began to regard themselves as members of a priesthood guarding skills and mysteries far too complex for ordinary mortals. [21]

Nonetheless, compiler development marched on and the number of compiler developers continued to grow. In 1954, Remington Rand formed the Automatic Programming Department in support of Hopper's team. With Hopper's ability to teach and communicate along with her compassion for the programmer, the group thrived. [TODO: Adele Mildred Koss, interview by Kathy Kleiman, 1993, talking about how nice it was to work for her.] Just as Backus was inspired by Laning and Zierler's work at MIT, so too was Hopper. In preparation for the Office of Naval Research's 1954 Symposium, her new department was focused on extending the A-3 compiler, providing a compiler capable of generating more efficient machine code, but not much else past what the A-2 could do. Inspired by the work coming from MIT, they attempted to extend the A-3 to support source code that resembled equations (more so than the three-address pseudocode that preceded it, at least). This new compiler was called the AT-3 and formed one of the two major components of the MATH-MATIC. The AT-3 was the *Translator* and the A-3 was the *Arith-Matic Compiler* [11].

It is worth noting that *Grace Hopper and the Invention of the Information Age* [30] appears to have a mistake regarding the A-3 and AT-3:

Hopper and her colleagues explored the possibility of an equation-based programming language, and by 1956 they had modified A-3 to the point where it could support a user-friendly source code. The resultant AT-3 compiler was later named MATH-MATIC.

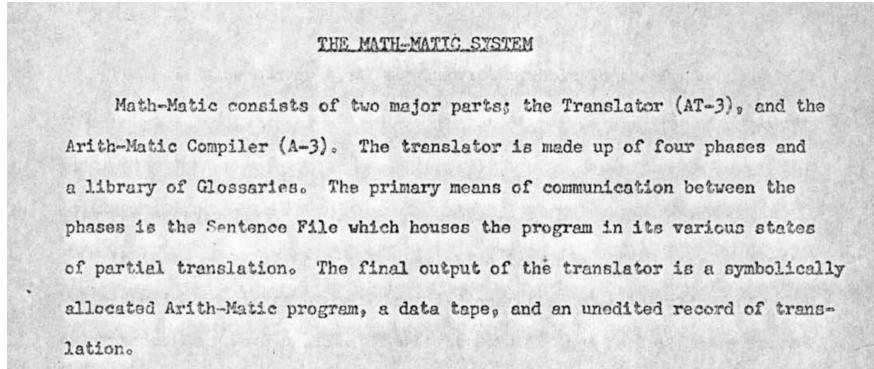


Figure 2.2: Excerpt from the MATH-MATIC User's Manual, 1957

This led me to believe that the A-3 *became* the AT-3 which was renamed to the MATH-MATIC; however, the MATH-MATIC user manual [11] specifies that the MATH-MATIC is composed of the A-3 and the AT-3, which were apparently separate programs with separate names (the Translator and the Arith-Matic Compiler, respectively). See Figure 2.2. Knuth and Pardo describe the MATH-MATIC's development as: "The language was originally called AT-3; but it received the catchier name MATH-MATIC in April, 1957, when its preliminary manual was released." [76] Perhaps the *language* A-3 was extended to become AT-3 which was renamed MATH-MATIC, while the A-3 and AT-3 compiler programs remained distinct; because I cannot find sources to clarify this nor the original programs, I cannot conclusively describe the MATH-MATIC and its precise relationship to the A-3 and AT-3 past what the user's manual describes.

Because the MATH-MATIC translated source code into A-3 pseudocode as an intermediate step, one might also consider this the first compiler to have an internal intermediate representation. The MATH-MATIC would last until about 1961, at which point UNIVAC users were already expecting FORTRAN compilers available on their UNIVAC systems, favoring IBM's language to Hopper's.

These layers of translation worked against them; the vast majority of codes at the time were dominated by floating point arithmetic, so any degradation of floating point performance would be catastrophic for the overall performance of the system. Around the same time, John Backus had convinced IBM leadership that their new machine, the IBM 704, should have index registers and floating point processing hardware, which is exactly what both UNIVAC and IBM 701 customers lacked and were spending all their time on. Now that the 704 accounted for these prior deficiencies in both machines, Backus's FORTRAN combined with the 704's hardware entirely outclassed the UNIVAC I and Hopper's MATH-MATIC.

But the MATH-MATIC programmers did not share the FORTRAN group's enthusiasm for efficient machine code; they translated MATH-MATIC source language into A-3 (an extension of A-2), and this produced extremely inefficient programs, especially considering the fact that arithmetic was all done by floating-point subroutines. The UNIVAC computer was no match for an IBM 704 even when it was expertly programmed, so MATH-MATIC was of limited utility.

Knuth and Pardo[76].

### 2.4.7 The B-0, FLOW-MATIC, and COBOL

In an interview for the Computer History Museum's Oral History series, Hopper explains her motivations for the compilers to follow the A-2 [64]:

**Pantages:** At that point did you have a feeling for what was happening, in terms of what you were contributing?

**Hopper:** No. I've always objected to doing anything over again if I had already done it once. That was building the compiler. Then I decided there were two kinds of people in the world who were trying to use these things. One was people who liked using symbols - mathematicians and people like that. There was another bunch of people who were in data processing who hated symbols, and wanted words, word-oriented people very definitely. And that was the reason I thought we needed two languages. The data processors did not like symbols, abbreviations that didn't convey anything to them. They were totally accustomed to writing things in words. So why not give them a word-oriented language? And that was part of what was behind Flow-Matic B-0, which became one of the ancestors of COBOL.

Hopper's interests in making programming accessible had not waned, however her target audience shifted. In January of 1955, she shared her more radical ideas for a new compiler in a report titled *Preliminary Definitions: Data-Processing Compiler* [65]. This compiler would take on several names: first, the data-processing compiler as outlined in the 1955 paper, then the B-0, the Procedure Translator, and finally the FLOW-MATIC.

This was what she originally called the data processing compiler in January, 1955; it was soon to be known as "B-0," later as the "Procedure Translator", and finally as FLOW-MATIC. This language used English words, somewhat as MATH-MATIC did but more so, and its operations concentrated on business applications. [67]

Always concerned with how intelligible her users would find her compilers and programming languages, she focused more on business applications and managers. Donald Knuth and Luis Trabb Pardo describe the FLOW-MATIC as "far more influential and successful, since it broke important new ground." [76] (see also Figure 2.4). Instead of catering her compilers to mathematicians and scientists by introducing mathematical symbols and notation, she sent members of her team to UNIVAC customers to learn about their *business* needs.

The business compiler B-0 was first made available to UNIVAC customers at the start of 1958. Shortly thereafter, Remington Rand merged with Sperry Gyroscope to form Sperry Rand. The marketing department of the newly formed company renamed the B-0 to FLOW-MATIC. Catering to business users, the FLOW-MATIC was programmed in English-like statements such as: IF EQUAL GO TO OPERATION 5 ; OTHERWISE GO TO OPERATION 2 . The group toyed with using abbreviations for common words, but after studying their customer's programs, they found that too many abbreviations could be mapped to different words based on the customer, so the abbreviated form was abandoned. Jean Sammet notes: "A preliminary manual for the running system was marked Company Confidential and dated July, 1957; it was available to me at that time since I was an employee of the Sperry Rand Corporation" [108]. The first generally available version

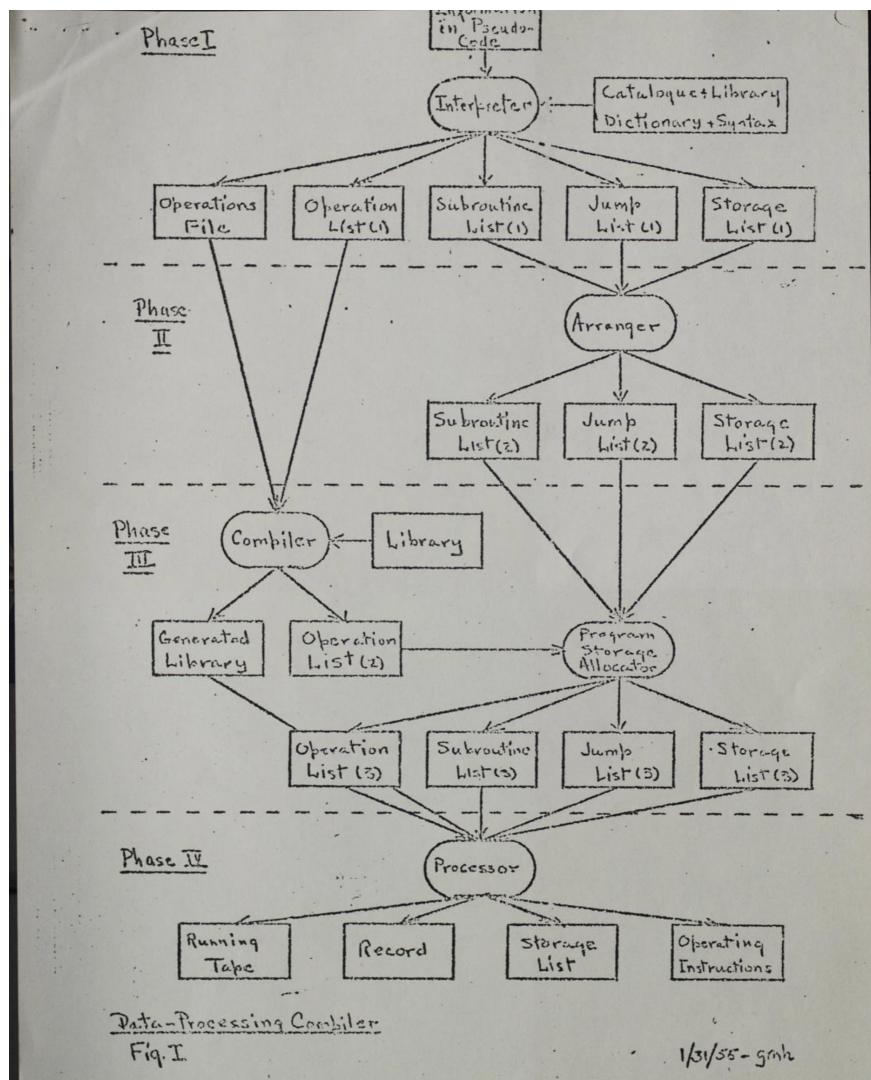


Figure 2.3: Depiction of Hopper's Data Processing Compiler[65]

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(1) COMPARE PART-NUMBER (A) TO PART-NUMBER (B) ; IF GREATER GO TO
OPERATION 13 ; IF EQUAL GO TO OPERATION 4 ; OTHERWISE GO TO
OPERATION 2 .
(2) READ-ITEM B ; IF END OF DATA GO TO OPERATION 10 .

```

Figure 2.4: Example FLOW-MATIC code from Knuth and Trabb Pardo[76]

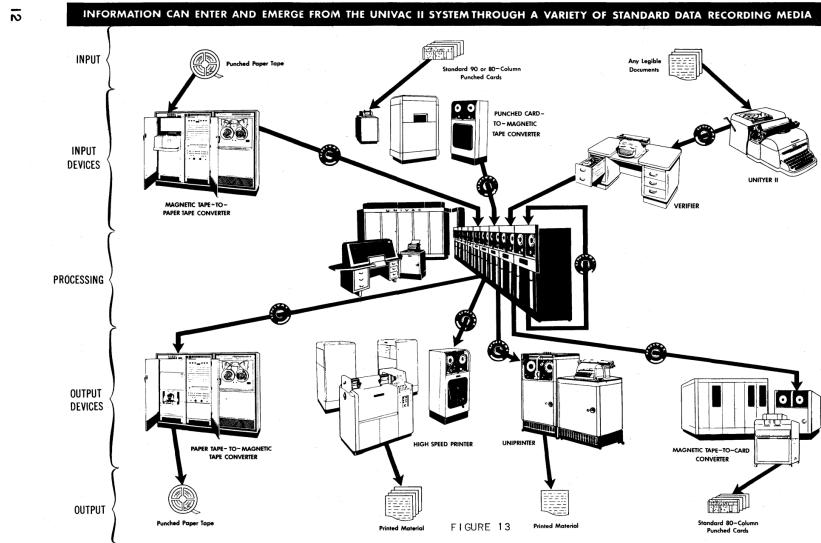


Figure 2.5: Advertisement for FLOW-MATIC and UNIVAC II

of this document was available in early 1958 in *FLOW-MATIC Programming System, U1518 Rev. 1*[112]. In this manual, one now finds advertisements for the UNIVAC II<sup>2.5</sup>.

In this document, one also finds a crude method for specifying the language, which the reader should note; Backus would propose a more formal method for specifying programming languages not long after this. For example, the CLOSE-OUT command is specified in the FLOW-MATIC manual as <sup>1</sup>:

$$\Delta \text{CLOSE-OUT} \Delta \left[ \begin{array}{c} \text{file} \\ \text{files} \end{array} \right] \Delta f_1 \Delta [f_2 \Delta f_3 \Delta \dots \Delta f_n]$$

There was no format method for specifying languages at the time. In 1959, one of the B-0 developers, Mary Hawes, contacted Sperry Rand about a meeting to discuss the direction of compilers for business applications. [TODO: In some places, Hawes is described as one of the B-0 developers which leads me to believe she worked at RR or SR, but Sammet points out that she worked for the ElectroData division of the Burroughs Corporation. Need to clarify.] Sammet states[109] that Block's 1959 *Report on Meeting held at University of Pennsylvania Computing Center* names Hawes as having called this meeting:

...meeting was the result of a request by Mary K. Hawes (ElectroData Division,

<sup>1</sup>The  $\Delta$  symbols were present in the original manual because it signified an empty space in the punchcards the FLOW-MATIC was written on

Burroughs Corporation) to plan a formal meeting involving both users and manufacturers where plans could be prepared to develop the specifications for a common business language for automatic digital computers.

I am unable to retrieve this document; we will take Jean Sammet's word for it. [TODO: there is not much about Mary Hawes available online, I can't even find the papers Hawes originally wrote. Try to find Mary Hawes, "Automatic Routines for Commercial Installations"] Through a connection at the Department of Defense (Charles Phillips), this group asked the DoD to sponsor the meetings and recommended a list of attendees, including 7 representatives from computer manufacturers and 14 from user organizations. In an ACM talk on September 1, 1959, Phillips remarked that "embarrassed that the idea for such a common language had not had its origin by that time in Defense since we would benefit so greatly from the success of such a project." [108, Phillips, as quoted by Sammet in]. There was a great deal of discussion around a common business language that could be shared across different computer systems and reduce the overall cost of programming. *Three separate committees* were formed for this purpose; a short, medium, and long-term committee. Sammet remarks that "the importance of the notion of Short and Intermediate-Range Committees is absolutely crucial to an understanding of the development of COBOL"[109]. She went on to say:

This wording is clearly ambiguous, and at the time, there was some discussion as to whether or not we were to try and create a language. It was by no means clear *then* that we were to do anything *except* try and *combine* the three known languages of the time. These were FLOW-MATIC, ...AIMACO, ...and COMTRAN

AIMACO was a language developed at the Air Force, and COMTRAN (renamed to Commercial Translator) existed only in an IBM manual whose implementation had never even started. Clearly, Hopper and Sperry Rand's FLOW-MATIC was the most mature of these languages. Indeed, Hopper pointed out that the language resulting from these committees was almost entirely FLOW-MATIC[64], though the committee notes did not say as much. Hopper's lack of an ego and her diplomatic skills led her to give credit liberally in order to drive consensus.

We'd written FLOW-MATIC before that, and if you take the FLOW-MATIC manual and compare it with COBOL 60 you'll find COBOL 60 is 95% FLOW-MATIC. So the influence of Commercial Translator in fact was extremely small. But I figured the thing to do was corral those people and when we had something to say, we'd say it was a compound of FLOW-MATIC and Commercial Translator and keep the other people happy and wouldn't try to knock us out. We'd give them some credit and they'd have to get on board with us. But if you compare the two manuals you'd find that it had hardly any influence at all. But if you gave them credit for it, why they'd go right along with you. If you didn't, they'd fight you. You can always give credit, you can always afford to.

That again is the practical. Think about the other guy and his position and his interest. You are always trying to work with people rather than against them. You've got a new idea; give the boss credit for it. It doesn't cost you anything.

Here the reader should note Sammet's observation of the differing goals of the three committees:

I am certainly convinced in my own mind that had the Short-Range Committee realized at the outset that the language it created (i.e., COBOL) was going to be in use for such a long period of time, it would have gone about the task quite differently and produced a rather different result. But I believe that most of us viewed our work as a stopgap measure—a very important stopgap indeed, but not something intended for longevity.

Most of the disdain for COBOL may indeed come from the fact that it was designed by the *short term* committee, which was solely trying to combine the existing compiler and programming language practices of the time into one language to prevent further bifurcation. If COBOL seems like a mishmash of different ideas designed by committee without proper foresight or thoughtful design, well, it more or less was. Yet another committee was formed on top of these three, the "Committee on Data Systems Languages" or CODASYL, which would be the executive committee steering the direction of the other three.

#### 2.4.8 COBOL Comes Into Focus

Throughout September and October of 1959, the short-term committee met regularly to build consensus around this new language. There were a number of similar names considered before settling on COBOL:

BUSY	Business System
BUSYL	Business System Language
INFOSYL	Information System Language
DATASYL	Data System Language
COSYL	Common Systems Language
COCONSYL	Common Computer Systems Language

On September 18th of that year, the committee settled on COBOL, according to Jean Sammet's personal notes[109]. I spare the reader of the full details of the committee's meetings, and encourage interested readers to consult Sammet's detailed notes on the proceedings. Perhaps the most important conclusion from these meetings was that there existed a *Basic COBOL*, and that no computer manufacturer could claim to support COBOL unless they supported this core subset of the language. This set the stage for numerous modern programming languages that contain a standard language specification that all compilers must support to claim to be a compiler for that language, key examples being C and C++.

The first correct and complete compilation of a COBOL program was performed in August of 1960 on an RCA 501. In December of that year Sperry Rand and RCA each wrote COBOL programs, ran them on their own machines, exchanged programs, and verified that they ran on the other manufacturer's machine as well (a UNIVAC II and an RCA 501). Thus was the first programming language standardized to the point that different compilers running on different machines could compile the same program correctly.

While Hopper's legacy tends to be tied to COBOL, I believe her true legacy lies in her passion for open collaboration and making programming accessible. She thought deeply about the needs of her users; she sent employees to study them and learn what their pain points were and how best the compiler developers could serve them. She considered how to tailor her technology to

her users, how to make her compilers more friendly speakers of other languages, and how to make compilers produce understandable error message and make them more debuggable. She facilitated communication between users and developers and advocated for a liberal notion of intellectual property to facilitate the sharing of ideas and programs; this outlook would lead to a cambrian explosion of compiler technology in the open-source era decades after her work on COBOL. This is where I think Grace Hopper's true legacy lies, not solely in the language cobbled together by a potpourri of committees with a short-term view.

## 2.5 Laning and Zierler at MIT

Of the early compiler efforts, Laning and Zierler's team is perhaps the most overshadowed. Their contemporaries were very impressed by their work, and they inspired a number of innovations at Remington Rand and IBM. Backus described the pseudocode compilers of the time (such as the A-2) as merely providing an instruction set slightly different from the machine's actual code, but not providing any real abstraction; writing the pseudocode still tedious and unproductive. Laning and Zierler's work was different.

[TODO: Backus was a huge fan of thier work. Look at "The history of Fortran I, II, and III".]

[TODO: Laning and Zierler, 1954. Early 1950s. Inspiration for Backus/FORTRAN. Worked more like a modern compiler than Hopper's A-0 and A-1. ]

Backus on the Whirlwind[21]:

**5. The Priesthood versus the Laning and Zierler Algebraic Compiler** Very early in the 1950s, J. Halcombe Laning, Jr., recognized that programming using algebraic expressions would be an important improvement. As a result of that insight he and Neal Zierler had the first algebraic compiler running on WHIRLWIND at MIT in January 1954 [3]. (A private communication from the Charles Stark Draper Laboratory indicates that they had demonstrated algebraic compiling sometime in 1952!) The priesthood ignored Laning's insight for a long time. A 1954 article by Charles W. Adams and Laning (presented by Adams at the ONR symposium) devotes less than 3 out of 28 pages to Laning's algebraic system; the rest are devoted to other MIT systems. The complete description of the system's method of operation as given there is the following

In retrospect, the biggest event of the 1954 symposium on automatic programming was the announcement of a system that J. Halcombe Laning, Jr. and Niel Zierler had recently implemented for the Whirlwind computer at M.I.T. However, the significance of that announcement is not especially evident from the published proceedings [NA 524-], 97% of which are devoted to enthusiastic descriptions of assemblers, interpreters, and 1954-style "compilers". [76]

The first programming system to operate in the sense of a modern compiler was developed by J. H. Laning and N. Zierler for the Whirlwind computer at the Massachusetts Institute of Technology in the early 1950s. They described their system, which never had a name, in an elegant and terse manual entitled "A Program for

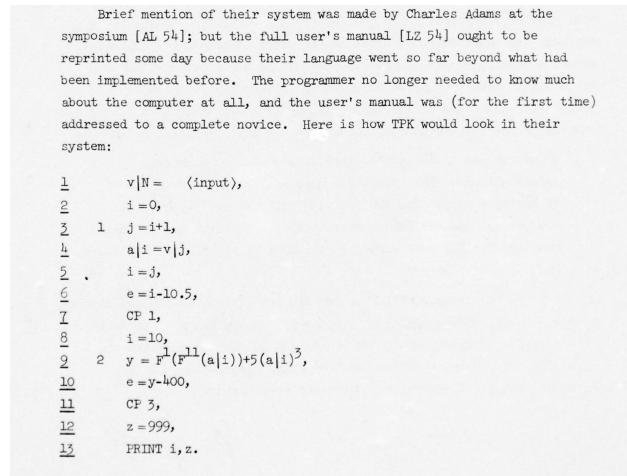


Figure 2.6: Knuth and Trabb Pardo on Laning and Zierler's Algebraic Compiler

Translation of Mathematical Equations for Whirlwind I,” distributed by MIT to about one-hundred locations in January 1954.<sup>26</sup> It was, in John Backus’s words, ”an elegant concept elegantly realized.” Unlike the UNIVAC compilers, this system worked much as modern compilers work; that is, it took as its input commands entered by a user, and generated as output fresh and novel machine code, which not only executed those commands but also kept track of storage locations, handled repetitive loops, and did other housekeeping chores. Laning and Zierler’s ”Algebraic System” took commands typed in familiar algebraic form and translated them into machine codes that Whirlwind could execute.<sup>27</sup> (There was still some ambiguity as to the terminology: while Laning and Zierler used the word ”translate” in the title of their manual, in the Abstract they call it an ”interpretive program.”)<sup>28</sup> [56]

## 2.6 John Backus and Fortran

IBM did not feel that Aiken and the Harvard Computation Laboratory had given them sufficient credit for their contributions to the Mark I, which left Thomas Watson Sr. and the IBM folks bitter about the experience and eager to produce a new device entirely in-house. This device would become the Selective Sequence Electronic Calculator, or the SSEC. It was built on 57th Street in Manhattan, and it was monstrous. Roughly 50 by 100 feet with a giant console and hundreds of toggle switches and tape units and relays behind glass panels; there were giant windows that allowed passersby to see the machine in action.

One such passerby was John Backus, a recent Masters graduate from Columbia University. He was intrigued by the machine, which he mentioned to his tour guide, who suggested he go upstairs and talk to the boss about it. Robert ”Rex” Seeber gave him a puzzle, which he solved, and he was hired on the spot [20].

In 1942, Backus majored in Chemistry at the University of Virginia where he struggled academically. He was expelled due to poor attendance within the first year before being drafted into the US Army. He commanded an antiaircraft battery at Fort Stewart, Georgia, remaining in the

US for the remainder of WWII.

While he did not at first find success in academia, he got very good marks on military aptitude tests. He was directed to the University of Pittsburgh's engineering program and later to a premedical program at Haverford College near Philadelphia (which is where he grew up). In 1945 he attended the Flower and Fifth Avenue Medical School in NYC, but he was still struggling with the academy. He was uninterested in medicine, feeling that it was all about memorization. He dropped out after less than a year.

He entered a radio technician school and became interested in math, which led him to enroll in the math program at Columbia University. The SSEC that would intrigue him at the IBM computing center was designed at the Watson Scientific Computing Laboratory at Columbia.

### 2.6.1 IBM Mathematical FORmula TRANslating System

At IBM, Backus worked on the SSEC and later the IBM 701 and 704. The main use of the SSEC was aerospace calculations; programming calculations to predict the position of the moon was one of the first tasks he was given at IBM. He would continue writing programs for these machines in spite of their poor usability. His team's techniques would be used in the lunar missions of the 1960s.

The pain of writing programs for these early machines entirely in machine code drove him to explore new ways to program. The first of these was a symbolic notation for floating point arithmetic and address expression calculation called Speedcoding[20]:

**Grady Booch:** So then from your experience with the SSEC, you then went on to produce Speedcoding, the Speedcoder...What were sort of the things that influenced you to create that in the first place?

**John Backus:** Well, programming in machine code was a pretty lousy business to engage in, trying to figure out how to do stuff. I mean, all that was available was a sort of a very crude assembly program. So I figured, well, let's make it a little easier. I mean it was a rotten design, if I may say so, but it was better than coding in machine language.

The IBM 701 did not have an index register, so calculating addresses for array operations was tedious and error-prone. Speedcoding provided a way to express these calculations symbolically.

The 704 was the first machine to have such a register; it also had floating point instructions and core memory, more or less obviating the need for Speedcoding: "we were moving to the 704, which had built in floating point, built in index registers, which was all that Speedcoding was supposed to supply. So what the hell?" [20] He credits himself with getting index registers and floating point into the 704.

Backus did not think all that highly of Speedcoding in retrospect, though it gained traction in large part due to IBM's marketing power and the number of users of the 701 relative to the size of the computer market at the time [21]. It is unclear whether Backus's assessment of his own code is accurate or if it's born of humility.

The success of some programming systems depended on the number of machines they would run on. Thus, an elegant system for a one-of-a-kind machine might remain obscure while a less-than-elegant one for a production computer achieved popularity. This point is illustrated by two papers at the 1954 ONR symposium One, by

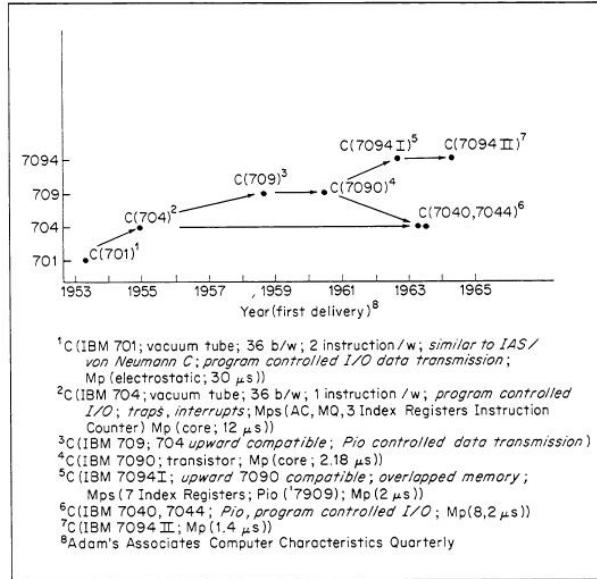


Figure 2.7: Excerpt from *The IBM 701-7094 II Sequence: A Family by Evolution* [58], illustrating the instruction structure for summing quantities.

### Examples Involving Address Modifications

An example of the use of the above instructions is given below.

Assume that it is necessary to compute the sum of 34 quantities stored in locations 688 through 721 and to store the sum in cell 945. Assume further that cell 1013 contains the number zero and that instruction 416 is the next instruction to be executed. The sequence of instructions shown below could be used for this purpose:

LOC	OP <sub>1</sub>	R	A	B	C	OP <sub>2</sub>	D
0416	ADD	0	1013	1013	0945	SETRA	0000
0417	ADD	4	0688	0945	0945	SKRA	0033
0418	NOOP	0	0000	0000	0000	TIA	0417

Figure 2.8: Excerpt from *IBM Speedcoding for the Type 701 Electronic Data Processing Machine* [68]

David E. Muller, describes a floating point interpretive system for the ILLIAC designed by D. J. Wheeler. The other, by Harlan Herrick and myself, describes a similar kind of system for the IBM 701 called Speedcoding. Even today Wheeler's 1954 design looks spare, elegant, and powerful, whereas the design of Speedcoding now appears to be a curious jumble of compromises. Nevertheless, Wheeler's elegant system remained relatively obscure (since only ILLIAC users could use it) while Speedcoding provided enough conveniences, however clumsily, to achieve rather widespread use in many of the eighteen 701 installations.

In 1953, based on his experience with Speedcoding on the 701, Backus proposed yet another

language to elevate the programming experience on the 704. IBM management supported the proposal. He formed a ten-person team of his own choosing based out of IBM's Manhattan headquarters, including Irving Ziller, [TODO: list other members]. They released *Preliminary Report, Specifications for the IBM Mathematical FORMula TRANslating System, FORTRAN*[69] after about one year of working together. Roughly two years after its first conception, FORTRAN was released for the first time. It would go on to ship with every IBM 704 and become the primary means of programming in the scientific community. Backus could not stand how slow programming was without a higher-level language, and machines were expensive; leasing a machine and spending time programming in machine code wasted money compared to a compiler capable of generating reasonable machine code (though at the time it usually underperformed hand-written code). Backus and his team would continue to develop and stabilize this compiler for several years, though.

FORTRAN did not really grow out of some brainstorm about the beauty of programming in mathematical notation; instead it began with the recognition of a basic problem of economics: programming and debugging costs already exceeded the cost of running a program, and as computers became faster and cheaper this imbalance would become more and more intolerable. This prosaic economic insight, plus experience with the drudgery of coding, plus an unusually lazy nature led to my continuing interest in making programming easier. This interest led directly to work on Speedcoding for the 701 and to efforts to have floating point as well as indexing built into the 704. [21]

When Backus was forming his team in January of 1954, he was moved from the Pure Science department at IBM into the Applied Science department because his boss Rex Seeber wanted nothing to do with the project. There he found Irving Ziller, who became his first teammate. By April, they had been joined by Harlan Herrick who co-authored the Speedcoding paper with Backus at the ONR symposium "IBM 701 Speedcoding and Other Automatic Programming Systems" in which they observe:

The question is, can a machine translate a sufficiently rich mathematical language into a sufficiently economical program at a sufficiently low cost to make the whole affair feasible? consider the advantages of being able to state the calculations...for a problem solution in a concise, fairly natural mathematical language.

The reader should note that it is often incorrectly asserted (at times even by Backus himself[21]) that this came *after* Backus and Ziller had been given a demonstration of Laning and Zierler's algebraic compiler for the Whirlwind at MIT at the ONR symposium of 1954. When they received this demonstration, there were already four members of the FORTRAN team, Irving Ziller, Robert Nelson, Harlan Herrick, and Backus himself. In Backus's words[21]:

The article and the letter therefore show that, much to my surprise, the FORTRAN effort was well under way before the ONR symposium and that, independently of Laning (but later), we had already formulated more ambitious plans for algebraic notation (e.g., Gail bjk) than we were later to find in Laning and Zierler's report and see demonstrated at MIT. It is therefore unclear what we learned from seeing their

pioneering work, despite my mistaken assumption over the years that we had gotten our basic ideas from them

Indeed, even Grace Hopper at a 1956 symposium made the same assertion:

A description of Laming and Zierler's system of algebraic pseudocoding for the Whirlwind computer led to the development of Boeing's BACAIC for the 701, FORTRAN for the 704, AT-3 for the Univac, and the Purdue System for the Datotron and indicated the need for far more effort in the area of algebraic translators. [76]

I am not sure who to believe either!

With the support of his new boss Cuthbert Hurd, his family, friends, and his team, the first report of FORTRAN was released externally to 704 users. This brought interest from a variety of users, many of whom offered up members of their teams to help.

## 2.6.2 FORTRAN Optimization and the First Users

Setting aside the members of the "priesthood" of computing who were hostile to the idea of compilers and making programming more accessible, the external report on FORTRAN brought these contributors into the fold:

- Walter Ramshaw at United Aircraft allowed Roy Nutt to work with the team; he eventually designed and implemented parts of the runtime and I/O features.
- Charles W. Adams allowed Sheldon Best to join the team on leave from MIT.
- Sidney Fernback at the Livermore Radiation Laboratory loaned out Bob Hughes.
- Harry Cantrell at G.E. was enthusiastic about the project.

The team was composed of the following 9 members: David Sayre, Harlan Herrick, John Backus, Lois Haibt, Robert Nelson, Roy Nutt, Sheldon Best, Richard Goldberg, and Peter Sheridan.

Most of the skepticism about FORTRAN came from the "priesthood," Backus's facetious term for the "elite" programmers who thought programming needn't be made accessible and compilers would never achieve the performance of hand-written code. This made the optimization of FORTRAN programs a high priority for the team; to achieve the widest possible adoption, they would need to convince even the skeptics that FORTRAN programs could be efficient. In Backus's 1976 retrospective on programming in the 1950s[21], he highlights the optimization efforts of three members of the original team:

- Nelson and Ziller optimized array indexing expressions and loop analyses. Backus specifies that they could "move computations from the object program to the compiler" which appears to be the first instance of constant-folding in a compiler.
- Best optimized the use of index registers based on the expected hotness of the execution path. "As of 1970 there were no known provably optimal algorithms for the problem he dealt with; his methods were the basis of many subsequent storage-allocation algorithms and produced code that is very difficult to improve. (For more details of Best's methods see [4, pp. 510-515].)"

This is, as far as I can tell, the first concerted effort to produce an optimizing compiler.

### 2.6.3 Language Specification and Backus-Naur Form

[TODO: explain BNF and lang specs, tie in with GH algol work] [21]:

The notation for syntax description known as BNF offers another example of a development which began with a prosaic recognition of a need. After involvement with two language design efforts-FORTRAN and IAL (ALGOL 58)-it became clear, as I was trying to describe IAL in 1959, that difficulties were occurring due to the absence of precise language definitions. In a recent course on computability given by Martin Davis, I had been exposed to the work of the logician Emil Post and his notion of a "production." As soon as the need for precise description was noted, it became obvious that Post's productions were well suited for that purpose. I hastily adapted them for use in describing the syntax of IAL. The resulting paper [5] was received with a silence that made it seem that precise syntax description was an idea whose time had not yet come. As far as I know that paper had only one reader, Peter Naur. Fortunately, he had independently recognized the need for precision; he improved the notation (replacing  $oi:$  by  $I$  and  $:=$  by  $: =$ ), improved its readability by not abbreviating the names of metavariables, and then used it to describe the syntax of ALGOL 60 in the definitive paper on that language. He thus proved the usefulness of the idea in a widely read paper I and it was accepted.

[TODO: Backus wasn't really that involved in fortran II and III and IV. The rest of the team took over. He never wrote much fortran and doesn't have many thoughts about it today other than the "we should be making things higher level."] [TODO: Backus moved into functional programming. Was never fruitful. He became a lab fellow at IBM after Watson Jr started the fellowship program, so he could kinda play with functional programming and not really get much done.] [TODO: He didn't like Hopper's ideas for COBOL, thought they were crazy and too complex. Didn't like algol either. Really just wanted to make things simpler and higher level and easier to use. Pushed back against the priesthood.] [TODO: the priesthood didn't like fortran because it made things easy. Backus resisted this urge at every point in his career. Like GH, he wanted programming to be easy and accessible (tho he didn't like her direction)]

Though the FORTRAN operator's manual was completed by the fall of 1956, the compiler itself was not distributed to IBM 704 installations until April 1957. Within a year after distribution, half of the IBM 704 installations were using FORTRAN to solve more than half of all mathematical problems.<sup>16</sup> Subsequently, compilers were produced for the IBM 705 and the IBM 650, quickly making FORTRAN the most widely used automatic program of its day. By 1961, UNIVAC users demanded a compatible FORTRAN compiler and abandoned Hopper's MATH-MATIC. [30]

It describes the system which will be made available during late 1956, and is intended to permit planning and fortran coding in advance of that time [p. 1], Object programs produced by fortran will be nearly as efficient as those written by good programmers [p. 2], "Late 1956" was, of course, a euphemism for April 1957. Here is how Saul Rosen described fortran's debut [RO 64, p. 4]: Like most of the early hardware and software systems, fortran was late in delivery, and didn't really work when it was delivered. [67]

It is not my intention to give a complete description of either; hence this section will describe only the main highlights of FORTRAN development. The earliest significant document that seems to exist is one marked "PRELIMINARY REPORT, Specifications for the IBM Mathematical FORMula TRANslating System, FORTRAN", dated November 10, 1954 and issued by the Programming Research Group, Applied Science Division, of IBM. [108]

Laning and Zierler's algebraic compiler served as evidence that prestigious institutions such as MIT were taking automatic programming seriously, prompting Backus to write Laning a letter shortly after the May symposium. In the letter, Backus informed Laning that his team at IBM was working on a similar compiler, but that they had not yet done any programming or even any detailed planning.<sup>12</sup> To help formulate the specifications for their proposed language, Backus requested a demonstration of the algebraic compiler, which he and Ziller received in the summer of 1954. Much to their dismay, the two experienced firsthand the efficiency dilemma of compiler-based language design. The MIT source code was commendable, but the compiler slowed down the Whirlwind computer by a factor of 10. Since computer time was so dear a commodity, Backus realized that only a compiler that maximized efficiency could hope to compete with human programmers. Despite this initial disappointment, Laning and Zierler's work inspired Backus to attempt to build a compiler that could translate a rich mathematical language into a sufficiently economical program at a relatively low cost.<sup>13</sup> [30]

## 2.7 ALGOL

### 2.7.1 The IAL and the ALGOrithmic Language

Before Hopper, Mayes and Phillips pulled together their committees for a common business language, design-by-committee was already in the academic milieu, especially in Europe. While not commercially successful, ALGOL introduced a number of important concepts like block scopes and the declaration of the types of variables, and it would go on to be the standard language for describing algorithms in academia. Originally called IAL (International Algebraic Language), it came to be called ALGOL, or ALGOrithmic Language, and was designed by an international committee with representatives from different organizations with the goal of a truly machine-independent language.

After deliberation in numerous committees, representatives from the German Association for Applied Mathematics and Machinery (GAMM) and the ACM met in Zurich, Switzerland in the summer of 1958. They had both produced similar reports and wanted to meet and agree on a unified language. John Backus, Charles Katz, Alan Perlis and Joseph Wegstein from the ACM attended this meeting. [TODO: who were these people? Add some narrative. Maybe of Naur, then tie in with Backus.] They arrived at the following objectives [102]:

1. The new language should be as close as possible to standard mathematical notation and be readable with little further explanation.

2. It should be possible to use it for the description of computing processes in publications.
3. The new language should be mechanically translatable into machine programs.

Shortly thereafter, a large number of dialects and partial implementations sprung up around Europe and the US such as BALGOL from Burroughs Corporation in Detroit, Michigan for the Burroughs 220. Manufacturers such as Burroughs found the standard to be insufficient for their users: "BAC-220 provides additions for the ALGOL reference language which are essential to the operation of data-processing systems: input-output facilities, conventions for inclusion of segments of machine-language coding, and diagnostic features" [36]. This was intentional; the specifications of ALGOL (both the 1958 and the 1960 versions) was solely for the purposes of describing computation; no I/O or system libraries were specified. Other dialects included CLIP, JOVIAL, MAD, and NELIAC. The first issue of the *ALGOL Bulletin* was issued in March of 1959 out of Copenhagen with Peter Naur as the editor.

Jean Sammet describes the impact of ALGOL 58 [108]:

Among the more intriguing technical features of ALGOL 58 were its essential simplicity; the introduction of the concept of three levels of language, namely a reference language, a publication language, and hardware representations; the ***begin...end*** delimiters for creating a single (compound) statement from simpler ones; the flexibility of the procedure declaration and the ***do*** statement for copying procedures with data name replacement allowed; and the provision for empty parameter positions in procedure declarations. While ALGOL 58 is not an exact subset of ALGOL 60, the only items of significance which are in the former but not the latter are the ***do*** which was removed as a concept (although the word was used for something else) and the empty parameter positions. Because of this major carry-over, specific technical description of ALGOL 58 is not necessary.

## 2.7.2 ALGOL 60

[TODO: backstory of Peter Naur]

The International Conference on Information Processing was held in Paris in June, 1959, where there were several key developments in ALGOL. Firstly, John Backus presented the now-famous paper on Backus-Naur Form for formal specification of programming languages[23]. Although other accounts describe this paper as having garnered significant attention in the IAL proceedings, Backus would recount that his paper "was received with a silence that made it seem that precise syntax description was an idea whose time had not yet come. As far as I know that paper had only one reader, Peter Naur" [21]. Backus was involved in two programming languages at the time, FORTRAN and IAL, having already moved on from Speedcoding. Backus had been made aware of the logician Emil Post and her work on computability in a course given by Martin Davis, in particular, Post's notion of a "production." It was this concept that inspired Backus to write the 1959 paper, and Naur would go on to extend it. This is part of Backus's specification of arithmetic expressions in the paper before Naur's changes[23]:

$\langle \text{factor} \rangle : \equiv \langle \text{number} \rangle \text{ or } \langle \text{function} \rangle \text{ or } \langle \text{variable} \rangle \text{ or } \dots$

Naur would make the notation more readable by replacing *or* with | and  $\equiv$  with  $::=$  and by fully spelling out names that Backus had abbreviated. Naur's edits and extensions would bring interest from more readers than Backus's original paper had. This form would come to be known as Backus-Naur Form, or BNF. Naur would submit this in a paper to the subsequent ALGOL meeting in 1960, titled "Report on the algorithmic language ALGOL 60" [15]. Various committees had established some shortfalls of the original design of the IAL, and the 1960 meeting set out to address them with the help of Backus and Naur's specification. This report "represents the union of the Committee's concepts and the intersection of its agreements." The IAL had been renamed ALGOL 58 and then finally ALGOL 60 in Naur's report.

The final report specified expressions to have the following syntax:

$$\langle \text{expression} \rangle ::= \langle \text{arithmetic expression} \rangle | \langle \text{Boolean expression} \rangle | \langle \text{designational expression} \rangle$$

### 2.7.3 Adoption of ALGOL

The *ACM Communications* started publishing an *Algorithms* section; ALGOL 58 was initially used, but ALGOL 60 was adopted once the updated report was in wide circulation. FORTRAN was not considered acceptable for publication until 1966, and even then it did not see widespread use in the journal. Because ALGOL had been designed from scratch by committee, there were no reference compilers for people to use; the only way to verify the correctness of an algorithm described in ALGOL was to rewrite it in another language and run test against test cases.

In early 1960, the *ACM ALGOL Maintenance Group* formed: an informal working group for the purpose of discussing the implementation of an ALGOL compiler. They discussed ambiguities in the specification and potential changes to the ALGOL 60 report. They mostly corresponded by mail and reports of their discussions were synthesized in the *ALGOL Bulletin* [108]. The 14 issue of the bulletin in 1962 contained a questionnaire from Peter Naur about some ambiguities in the 1960 report, and the philosophy of some proposed changes and enhancements to the language.<sup>2</sup> This questionnaire was used as a guide for another committee meeting in Rome in April of 1962 the primary result of this meeting is detailed in Naur's "Revised report on the algorithmic language ALGOL 60".

### 2.7.4 Peculiarities of ALGOL

The reader may find it interesting that ALGOL had a number of peculiar features. Namely, the distinction between *call by name* and *call by value*. Naur's 1962 revised report explains it best, I think [16, Section 4.7.3]:

#### 4.7.3.1 Value assignment (call by value)

of the corresponding actual parameters...the effect is as though an additional block embracing the procedure body were created in which these assignments were made to variables local to this fictitious block...

#### 4.7.3.2 Name replacement (call by name)

---

<sup>2</sup>The reader may find it interesting that in this edition of the bulletin, Jean Sammet submitted a paper on *A Method of Combining ALGOL and COBOL*; [TODO: follow up on this?]

Any formal parameter not quoted in the value list is replaced, throughout the procedure body, by the corresponding actual parameter, after enclosing this latter in parentheses wherever syntactically possible.

While neither Naur's revised report nor Edgar Dijkstra's *Primer of Algol 60 Programming* point out the implications of this distinction, it is important to note the subtle implications. Call-by-name is not common in modern programming languages, and readers may find it more similar to macro expansion (though it is not, in fact, simply textual replacement). The following example illustrates the difference:

```
def get_5():
    print( "Hello!" )
    return 5

def double(x):
    return x + x

# Prints "Hello!" once with call-by-value
# and twice with call-by-name
double(get_5())
```

Most implementations of call-by-name use zero-argument thunks to delay the evaluation of arguments until they are used in the body. The arguments to function calls become zero-argument closure functions returning the value of the argument expression and capable of being *l-values*, or values in the left-hand side of an assignment expression. Then, each instance of the argument in the callee's body is replaced with an invocation of this function such that it is evaluated anew each time it appears. The prior Python example may be rewritten to emulate call-by-name as follows:

```
def get_5():
    print( "Hello!" )
    return 5

def double(x):
    # argument thunk is evaluated twice
    return x() + x()

double(lambda: get_5())
```

Call-by-value semantics demand that the argument to `double` be evaluated before entering the function, so `get_5` is called once and *the result* is passed to `double`. With call-by-name semantics, `get_5` is passed the expression itself, which is the substituted into the body of the callee *before* being evaluated.

Donald Knuth proposed the *man or boy* test in the *ALGOL Bulletin* to distinguish between conformant ALGOL compilers capable of handling recursion and non-local references using call-by-name semantics[75].

There are quite a few ALGOL60 translators in existence which have been designed to handle recursion and non-local references properly, and I thought perhaps a little test-program may be of value. Hence I have written the following simple routine, which may separate the man-compilers from the boy-compilers.

This uses nothing known to be tricky or ambiguous. My question is: What should the answer be? Unfortunately, I don't have access to a "man-compiler" myself, as I was forced to try hand calculations. My conjecture (probably wrong) is that the answer will be

$$73 - 119 - 177 + 102 = -121.$$

I'd be very glad to know the right answer.

The program is as follows:

```
begin
  real procedure A(k, x1, x2, x3, x4, x5);
  value k; integer k;
  real x1, x2, x3, x4, x5;
  begin
    real procedure B;
    begin k := k - 1;
      B := A := A(k, B, x1, x2, x3, x4)
    end;
    if k ≤ 0 then A := x4 + x5 else B
  end;
  outreal(1, A(10, 1, -1, -1, 1, 0))
end
```

There are some useful applications of call-by-name semantics as well; it is not *solely* an avenue for confusion. Jensen's Device, named in honor of Mr. J. Jensen of Regnecentralen, Copenhagen, is a well-known application of call-by-name semantics. This allows the user of a function to describe an expression to be evaluated in a loop body inside the function, permitting a very general summation function. There is not an especially elegant way to do this in a call-by-value language, but as Dijkstra points out[48], it can be implemented with this ALGOL program:

```
begin
  real procedure Sum(k, l, u, ak)
  value l, u;
  integer k, l, u;
  real ak;
  begin
    real s;
    s := 0;
    for k := l step 1 until u do
      s := s + ak;
    Sum := s
  end
```

```
end;
end
```

To simply sum the values of the array V from 1 to 100, one would call:  $\text{Sum}(i, 1, 100, V[i])$ . The real power of this device is its generality; if a user were to need to compute  $\sum_{i=1}^{100} V_i \times i$ , they could call  $\text{Sum}(i, 1, 100, V[i] * i)$ . The first and final arguments to this function are not evaluated until they are used in the body of the loop in  $\text{Sum}$ . Others have also pointed out numerous easy-to-make mistakes that arise because of this feature. For example, a naive implementation of a swap function may easily cause unintended side effects:

```
real procedure Swap(a, b)
    real a, b;
begin
    integer temp;
    temp := a;
    a := b;
    b := temp;
end;
```

While calling  $\text{Swap}(i, j)$  may work correctly, calling  $\text{Swap}(i+1, j+1)$  or  $\text{Swap}(A[i], A[j])$  would not.

Among the now-unusual argument evaluation semantics, ALGOL also lacked any I/O facilities as we have already discussed. The language also made string variables available, but provided no means of manipulating them. ALGOL permitted *own variables*, which are akin to static variables in C or SAVE variables in FORTRAN; they retain their values between entrances to the block. Dijkstra also points out nowhere is the evaluation order of subexpressions specified[48].

While the declared intent of defining a language detached from any particular manufacturer or machine was laudable (and profitable in the case of portable languages to follow), ALGOL was so far removed from the machine to be useless without supplementary libraries (for I/O, string processing, etc).

### 2.7.5 ALGOL 68 (and X, Y, W, ...)

For years after the 1962 Rome meeting, there were discussions amongst the ALGOL community about potential changes and improvements to the language. The 1960 and 1962 meetings restricted themselves to clarifying ambiguities and rectifying true errors in the prior reports, but not extensions or significant changes. In the March of 1964 as follow up to Rome meeting, they finally agreed to start on ALGOL X and ALGOL Y, drafts of the language that would consider significant changes and additions to the language. ALGOL X would be intended to solve the issues of ALGOL 60 in the short-term, compared to the "radically reconstructed future ALGOL Y" [51]. To summarize the following section: three draft reports were proposed, Adriaan Van Wijngaarden was asked to revise his draft report until the other two authors were satisfied, the draft report that all three were satisfied with became canonized as ALGOL 68, many people withdrew from the language community (formally or informally) in reaction to the new language, and the language was never widely adopted.

1964:	<b>ALGOL X</b>
1965–1969:	<b>Development</b>
	1965: Wirth, Seegmüller and Van Wijngaarden
	1966: Van Wijngaarden commissioned to make the Report
	1967: Draft [MR88]
	1968: Drafts [MR93] and [MR95]. Accepted Report [MR100]
	1969: Final Report [MR101]
1970–1974:	<b>Revision</b>
	1970: First Implementation: ALGOL68-R
	1974: Revised Report [RR]
1975–1979:	<b>Active Support</b>
	1976: Standard Hardware Representation, ALGOL 68S
	1978: Modules and Separable compilation

Figure 2.9: Timeline of ALGOL 68 development, based on[90].

In 1965, the first three drafts for ALGOL X were proposed by Niklaus Wirth (who benefitted from numerous comments from Tony Hoare), Gerhard Seegmüller, and Adriaan van Wijngaarden. Seegmüller’s proposal was not too different from Wirth and Hoare’s, which was called ALGOL W, but Wijngaarden’s proposal was dramatically different. Wijngaarden described his ALGOL X language in an entirely new formal grammar which came to be known as a *W-Grammar* (after his own namesake).

The impact of Wijngaarden’s proposal on the development of ALGOL cannot be overstated. In the spring of 1968, Adriaan van Wijngaarden released a draft report for ALGOL X (which was, mind you, supposed to be the *incremental* improvement to ALGOL 60) based on Wirth and Hoare’s ALGOL W but using his own W-Grammar. In this time period, Peter Naur and Brian Randall of IBM even suggested that the incremental ALGOL X be dropped entirely in favor of the more ambitious ALGOL Y. In nearly every step, the committee ran significantly past their schedules: “Throughout the whole project, the WG in general, and Van Wijngaarden in particular, consistently underestimated the time it would take by substantial factors. Recall that ALGOL 60 was put together in six days”[83].

Wijngaarden’s ALGOL W proposal was distinct for at least three reasons:

1. A two-level *W Grammar*.
2. ”The combination of a minimal number of language concepts in an orthogonal way” [83].
3. Expression orientation; everything is an expression and has a value.

The computer and aerospace historian Paul Ceruzzi describes the result of the ALGOL 68 report as follows:

Whereas ALGOL-60 was based on a formal structure and was very lean, ALGOL-68 was burdened by an attempt to do too much, with the effects that some features interfered with the clean implementation of others. It was hard to understand. In

an attempt to satisfy a broad range of users worldwide, the committee produced something that satisfied few.

Charles Lindsey, British computer scientist and editor of *Revised Report on the Algorithmic Language ALGOL 68*[120] (and numerous other texts on ALGOL 68) describes popular sentiment about the report[83]:

The world seems to have a rather negative perception of ALGOL 68. The language has been said to be "too big," to be defined by an "unreadable Report" produced by a committee which "broke up in disarray," to have no implementations, and to have no users.

This was not his opinion, however[83]:

I should point out that my own involvement with the project came after the basic design of the language, and of its original Report, were complete...It is only now, in the course of studying the minutes and other documents from that time, that I have come to see what the real fuss was about, and I hope that all this has enabled me to take a dispassionate view of the events. The reader of this paper will certainly see discord, but I believe he will see also how good design can win through in the end.

Lindsey was appointed Lecturer in Computer Science at Manchester University in 1967. Aside from his standardization efforts, he wrote a research implementation of ALGOL 68 for the MU5, one of the Manchester computers, and he maintained an implementation of a subset of ALGOL 68, *ALGOL 68S*. It is difficult to take Lindsey's opinions about ALGOL 68 entirely at face value because so much of his career was dedicated to the language. If we look at those involved with the design of ALGOL 68 who had programming language design and compiler experience elsewhere, we find his depiction above to be more accurate than not.

Another meeting in May 1967 in Zandvoort, Netherlands was intended to develop a direction for ALGOL Y, however nearly all the discussion was about ALGOL X. From the outset, the only language aspect the designers knew they wanted from ALGOL Y was the ability for an ALGOL Y program to modify itself. Lindsey remarks:

They even spent an afternoon on ALGOL Y, from which it emerged that no one had very concrete ideas of what it was about, and that the only role model was LISP, on account of the fact that that language could construct its own programs and then *eval* them.

The largest single outpouring of criticism of ALGOL 68 came from *Draft Report on the Algorithmic Language ALGOL 68*[119], also known as [MR93]. Several members of the committee (including Peter Naur) resigned after this report's circulation. It was first circulated to the *ALGOL Bulletin* in February 1968, and "was the cause of much shock, horror, and dissent, even (perhaps especially) among the membership of WG 2.1"[83]. Numerous papers were written in response to MR93, including Peter Naur's *scathing* critique "Successes and failures of the ALGOL effort". His report was especially critical of the International Federation for Information Processing (IFIP), which had formed a working-group (WG 2.1) responsible for specifying the language.

Also in response to MR93, Lindsey joined the language effort and distributed his “ALGOL68 with fewer tears”[85] in which he attempted to synthesize the nicer language hidden within the earlier reports <sup>3</sup>. He clarified a number of terms which appear to be specific to ALGOL, and have well-understood meanings under different names in other languages. For example, what ALGOL calls a *name* is more commonly known as a reference or pointer; numerous differences in nomenclature contributed to the difficulty of reading the original reports.

Van Wijngaarden had circulated another report[89] in October of 1968 as he had earlier promised the committee; at the same time, other members who disagreed with the direction but had not resigned were also developing several minority reports, include Dijkstra, Hoare, and Randall. Sparing the reader the full details and political history of the ensuing committee meetings, the final report “Report on the Algorithmic Language ALGOL 68” was published in *Numerische Mathematik* and *Kybernetika* in 1969, thus specifying what we know today to be ALGOL 68.

### 2.7.6 The W-Grammar

Before analyzing the technical details of ALGOL 68, we must first understand what made it so offensive and uninterpretable to so many people: its grammar.

The primary reason for this grammar’s complexity is that it encompasses far more than compiler engineers typically associate with a grammar in order to be *formally context-sensitive*. It is not merely a specification of the syntax of the language; it also specifies a great deal of the *semantics* of the language, because that is required for the language to be context-sensitive. When the grammar is described as *two-level*, it communicates that the grammar consists of two levels: the meta-grammar and the concrete grammar, and the meta-grammar produces the concrete grammar. Readers of [MR93] would need to keep track of both levels in order to understand the document; meta-rules would need to be kept conceptually distinct in the reader’s mind. The *format* of the grammar was also problematic and difficult to read, but that may not have been so problematic had they not fit so many semantic concepts into the grammar.

When Wijngaarden first proposed his grammar, nobody had seen anything like it. The authors attempted to formalize object lifetimes, variable scoping rules, and relatively complicated type systems (even by today’s standards), all without the benefit of decades of research into type theory and formal semantics which we have today. Let the reader give the authors the benefit of the doubt; keep in mind they attempted to formalize concepts for the first time, and subsequent research benefitted greatly from their ideas (and mistakes). The language specification was, effectively, a compiler front-end specification.

Lindsey argues that the grammar is relatively close to Prolog, where goals and sub-goals are specified and solved for by unification <sup>4</sup>.

Lindsey uses the following Prolog example to introduce a rule in ALGOL 68’s W-grammar, which asks whether the input program contains an assignation, and to determine this, we must test if we have a destination, followed by some language construct that becomes a symbol, followed by a source.

```
assignment( ref(MODE) ) :-
```

<sup>3</sup>For those unaware, this title is an homage to the English textbook *French without Tears*.

<sup>4</sup>The term *unification* may be loaded; Milner did not publish his work on SML and type inference until later, though his work was inspired by ALGOL 68 to some degree.

```
destination (ref(MODE)), becomes_symbol, source(MODE).
```

The corresponding rule in the [MR93] draft report is as follows [117]:

```
MODE assignation :  
    reference to MODE destination, becomes symbol, MODE source.
```

Substituting *int* for our *MODE*, we have:

```
int assignation : reference to int destination, becomes symbol, int source.
```

If you squint your eyes, maybe you can see how *i* := 5 fits the mold for this grammar, where the *MODE* is *int*, the *destination* is the location of *i* of mode *ref int*, and the source is the integer literal *5* of *MODE int*. This may feel familiar, like a looser version of the Backus-Naur syntax used by compiler-compilers. The problem is, nearly anything else is also expressible in this grammar, making it nearly impossible to write a parser based on an unrestricted W-grammar.

A more nebulous component of the original grammar from [MR93] can be found in *Section 4.4 Context Conditions*, which specified conditions for a program to be *semantically valid* based on the surrounding context. Modern readers may find this strange; we are discussing the language *syntax*, why is the report discussing the semantic validity of, for example, the use of a particular identifier and its relationship to its declaration? As we have already established, this is an example of language *semantics* being specified in the grammar.

Most modern compilers are broken into three parts: the front-end, middle-end<sup>5</sup>, and back-end. The front-end typically consumes the input program, produces data structures representing the program, and performs *semantic* analysis on those data structures to ensure the program is valid. The consumption of the input program is usually determined by the grammar specified by the language, and semantic validity is determined by the language's type system and other rules, but not typically by the grammar. Because Wijngaarden attempted to specify all of this in the grammar itself, the grammar needed to specify these semantic conditions as well.

See this excerpt on *NESTs* from [120, Section 3.0.2, Semantics]:

A "nest" is a 'NEST'. The nest "of" a construct is the "NEST" enveloped by the original of that construct, but not by any 'defining LAYER' contained in that original.

The nest of a construct carries a record of all the declarations forming the environment in which that construct is to be interpreted.

Those constructs which are contained in a range R, but not in any smaller range contained within R, may be said to comprise a "reach". All constructs in a given reach have the same nest, which is that of the immediately surrounding reach with the addition of one extra "LAYER". The syntax ensures (3.2.1.b, 3.4.1.i,j,k, 3.5.1.e, 5.4.1.1.b) that each 'PROP' (4.8.1.E) or "property" in the extra 'LAYER' is matched by a defining. indicator (4.8.1.a) contained in a definition in that reach.

Today, *NESTs* might exist concretely as data structures inside the semantic analysis phase of a compiler, but in ALGOL 68, they were specified as part of the grammar itself.

---

<sup>5</sup>Only compiler engineers could produce a term as idiosyncratic as "middle" "end".

ALGOL 68's type system was also remarkably complex and, again, fully embedded in the grammar. Union modes are full commutative and accumulative types, meaning that the following types are equivalent: ***union(int, real, bool)***, ***union(real, int, bool)***, and ***union(bool, union(real, int))***.

Lindsey pointed out a particularly tricky aspect of the type system in structural equivalence. Structure types are equivalent if their members are equivalent, as opposed to name equivalence, which would have implied two structures with the same members but different names are not equivalent. The following two types are equivalent by structural comparison:

```
mode a = struct (int val, ref a next);
mode b = struct (int val, ref struct (int val, ref b next) next);
```

## 2.7.7 Concepts of ALGOL 68

Having covered the political machinery that resulted in the final report and the grammar that caused the community so much grief, we will now look to more technical details of the language.

Firstly, ALGOL 68 is an expression-oriented language; there is fundamentally no distinction between statements and expressions, allowing for constructs like the following:

```
x := (real a = p*q;
      real b = p/q;
      if a>b then a else b fi)
      + (y := 2*z);
```

There were numerous extensions to arrays, including higher-dimensional arrays, complicated slicing mechanisms, and flexible arrays, such as the following:

```
loc [1:4, 1:5] int a45;
a45[2, ] # row 2 #
a45[, 3] # column 3 #
a45[2:3, 3] # part of column 3 #
a45[2:3, 2:4] # a little square in the middle. #

loc flex [1:0] int array; # initially empty #
array := (1, 2, 3); # now it has bounds [1:3] #
array := (4, 5, 6, 7, 8); # now it has bounds [1:5] #
```

Call-by-name as it was known in ALGOL 60 was removed in favor of call-by-value and call-by-reference; some of the other proposals (such as Seegmüller's) preserved the two cases of call-by-name under different terms, but this was not adopted. One may argue that *proceduring* is a form of call-by-name, but this feature was removed in the revised report of 1973.

Call-by-name is really two different concepts: call-by-reference (where the actual parameter is a named variable to be assigned to) or call-by-full-name (where the actual parameter is an expression to be placed in a thunk and re-evaluated each time it is used in the body of the callee). This meant that Jensen's Device and other tricks made possible by call-by-name were no longer possible in ALGOL 68. Wirth's proposal included call-by-name untouched from ALGOL 60 alongside a new parameter passing mechanism called *parameterless procedure* parameters,

meaning the parameters were thunks to be evaluated each time they were used in the body of the callee[83]. Lindsey uses an inner-product algorithm to illustrate this:

```
begin
  procedure innerproduct
    (real a, real procedure b,
     integer value k, integer p, real result y);
  begin y := 0; p := 1;
    while p<=k do begin y := y + a * b; p := p + 1 end
  end;
  real array [1:n] x1, y1; integer j; real z;
  innerproduct(x1[j], y1[j], n, j, z);
end
```

He notes that the first and second parameters are effectively both called by-name. As far as I can tell, the only difference is that call-by-name parameters can be l-values or r-values while parameterless procedures are always r-values.

There were at least eight changes to the automatic type conversion rules between ALGOL 60 and ALGOL 68; Lindsey had this remark about the automatic type conversion features:

Although coercions had existed in previous programming languages, it was ALGOL 68 that introduced the term and endeavoured to make them a systematic feature (although it is often accused of a huge overkill in this regard).

Many of these coercion rules were uncontroversial; for example, a **real** may be assigned to a **union(real, int)**. [TODO: Discuss *rowing?* relates to rank polymorphism a bit..] One particularly problematic coercion rule was *proceduring*. This allowed users to force call-by-name semantics by coercing an expression into a parameterless procedure, like so:

```
PROC x plus 1 = INT : x + 1;
```

The right hand side of this expression is a *cast* (a term originally coined for the specification of ALGOL) of an integer expression into a procedure taking no arguments. In “ALGOL68 with fewer tears”<sup>6</sup>, Lindsey argues that references obviate the need for call-by-name semantics, but that call-by-substitution is what ALGOL 60’s call-by-name effectively was, and this was still possible with ALGOL 68 via this *proceduring* coercion:

```
begin
proc series = (int k, ref int i, proc real term) real expr
  begin
    real sum(0);
    for j to k do
      begin i := j;
        sum plus term
      end;
    sum
  end;
```

---

<sup>6</sup>This paper was, in fact, a valid ALGOL 68 program in and of itself

```

end

x := series (100, i, real expr(1/i));

# Or, via proceduring: #
x := series (100, i, 1/i);

```

This facility was abandoned in the ALGOL 68-R implementation which led to its removal in the revised report of 1973, as we will see in the next section. There were still other mechanisms to achieve similar results, as tricks like Jensen's Device had become important to the community. One such example is the extensions to procedures.

Naur and Wirth both proposed block-expressions, meaning the final value in a block is the value of the entire block in the context of an expression, and Naur's 1966 proposal (which was accepted) merged the formal parameters into the same line as the parameters themselves, meaning one need not restate the types of parameters at the beginning of the procedure body. These two features allowed for convenient use of anonymous procedures capturing parts of their surrounding context and procedures taking other procedures, relatively advanced features even for today. The revised report contains this example [120, Section 11.2, Innerproduct 1]:

```

proc innerproduct 1 = (int n, proc (int) real x, y) real:
    # the innerproduct of two vectors, each with 'n'
    # components, x(i), y(i), i = 1, ..., n, where 'x' and 'y'
    # are arbitrary mappings from integer to real number #
    begin long real s := long 0;
        for i to n do s +:= leng x(i) \times leng y
    (i) od;
        shorten s
    end

# Real-calls using innerproduct 1: #
innerproduct 1 (m, (int j) real: x1[j], (int j) real: y1[i])
innerproduct 1 (n, nsin, ncos)

```

## 2.7.8 ALGOL 68-R and the Revised Report

In 1973, the *Revised Report on the Algorithmic Language ALGOL 68* was published, codifying some of the changes and rejected features from the ALGOL 68-R team.

[TODO: look at peck's "The ALGOL 68 Story: A personal account by a member of the design team"[101]]

His work on ALGOL 68S was not an outlier amongst would-be implementers of the language; because the report was so large and complex, most implementers ended up restricting themselves to a subset of the language.

### 2.7.9 Criticism of ALGOL 68

Numerous committee members and readers of the *ALGOL Bulletin* criticized the original report for its complexity; Lindsey (our faithful defender of the language) conceded that "much of the mud slung at [the original report] is probably still sticking"[83]. Much of that complexity came from the

[TODO: Defined in Wijngaarden Grammar by Adriaan van Wijngaarden. Contains parsing and things which in other languages are called semantics.]

[TODO: '68 criticized by Hoare and Dijkstra for abandoning simplicity of '60. In 1970, ALGOL 68-R became the first working compiler for ALGOL 68.]

### 2.7.10 Legacy of ALGOL

It is difficult to overstate the impact of the design decisions of both ALGOL 60 and 68, even though neither saw widespread adoption in the industry outside depictions of algorithms in academic papers. When Dennis Ritchie extended Ken Thompson's B compiler with a type system, he drew heavy inspiration from ALGOL 68:

The scheme of type composition adopted by C owes considerable debt to Algol 68, although it did not, perhaps, emerge in a form that Algol's adherents would approve of. The central notion I captured from Algol was a type structure based on atomic types (including structures), composed into arrays, pointers (references), and functions (procedures). Algol 68's concept of unions and casts also had an influence that appeared later. [104]

Lindsey points out a few others:

The type system of ALGOL 68 has been adopted, more or less faithfully, in many subsequent languages. In particular, the structs, the unions, the pointer types, and the parameter passing of C were influenced by ALGOL 68 [Ritchie 1993], although the syntactic sugar is bizarre and C is not so strongly typed. Another language with a related type system is SML [Milner 1990], particularly with regard to its use of ref types as its means of realizing variables, and C++ has also benefitted from the reftypes [Stroustrup 1996]. "A history of ALGOL 68"

ALGOL 68 also had a notable influence in the Soviet Union, details of which can be found in Andrey Terekhov's 2014 paper "ALGOL 68 and Its Impact on the USSR and Russian Programming"[114].

[TODO: Pascal, Ada] [TODO: Lindsey: "So here are my recommendations to people who essay to design programming languages." "A history of ALGOL 68"[83]]

An interesting observation is the fact that the two-level formalism used in the description of ALGOL 68 by itself led to a number of programming languages, including PROLOG (Colmerauer)...[37]

## 2.8 Timeline

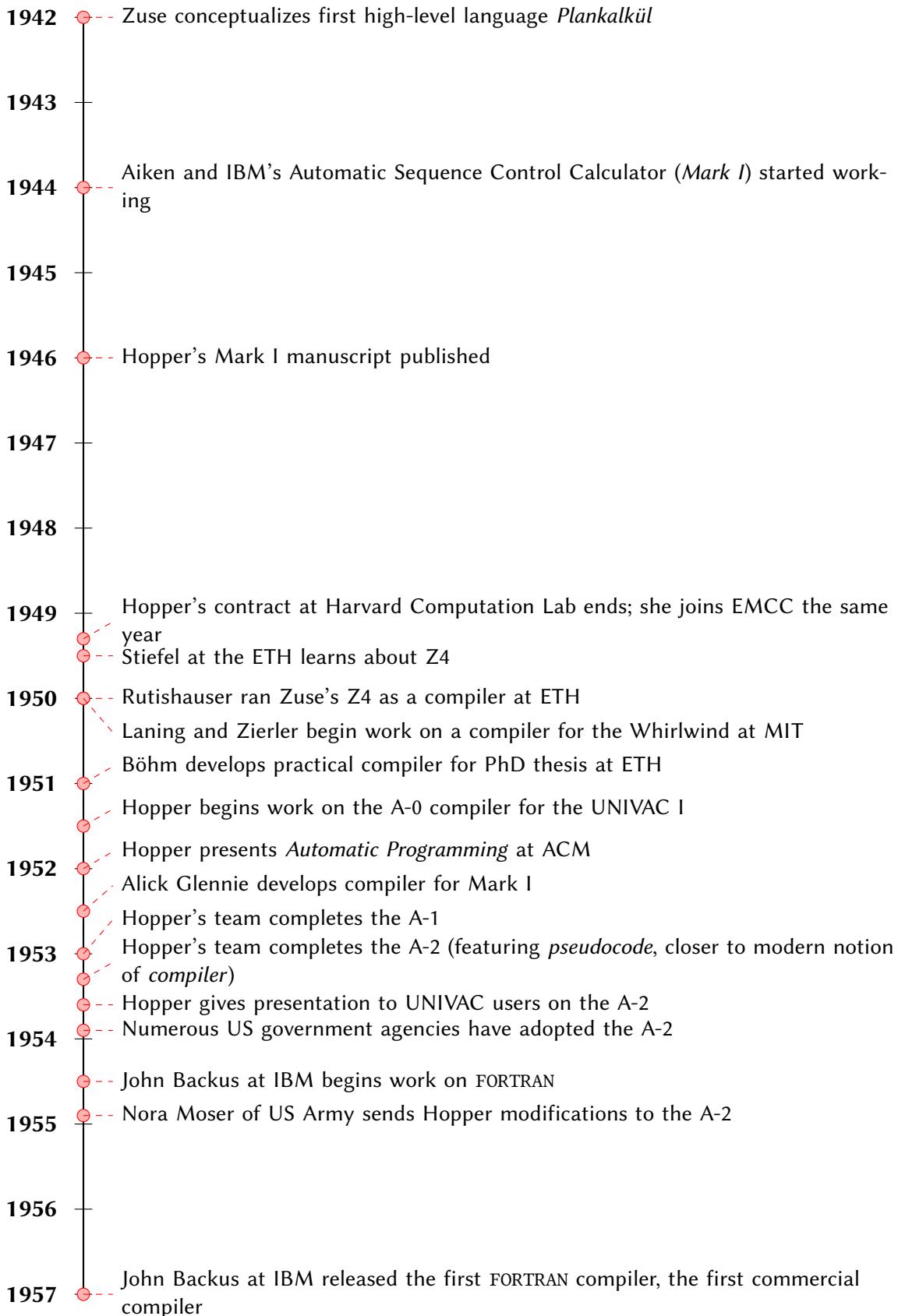


Figure 2.10: Early compiler history, 1942–1957

## 3

## Software, 1960-1980ish

Note that we will discuss some giants of computing history in this chapter who cast long shadows over the field; however, we will primarily focus on those involved in the development of compilers and programming languages. In particular, we will not do justice to key figures from Bell Labs, such as Brian Kernighan, Ken Thompson, Dennis Ritchie, Claude Shannon, and Doug McIlroy. I can recommend *The Idea Factory* for a more comprehensive account.

As Hopper and Backus had put it numerous times, programming was still exceedingly painful at this point in time, and the relative cost of programming kept increasing. As machines became cheaper and more powerful, the share of the cost of building and running a program devoted to programming increased, and thus the importance of ease of programming increased as well.

Michael Mahoney, doctor history and history of science, goes so far as to describe programming and computer design in this way as early as 1945 [106, *The Structures of Computation*]:

The kinds of computers we have designed since 1945 and the kinds of programs we have written for them reflect not the nature of the computer but the purposes and aspirations of the groups of people who made those designs and wrote those programs, and the product of their work reflects not the history of the computer but the histories of those groups, even as the computer in many cases fundamentally redirected the course of those histories.

While this description was correct about the *direction* compiler engineering and programming language design were going, the statement was far too strong to make about the year 1945. Automatic programming was still scarcely more than synthetic machine code so the programmer did not have to concern themselves with the details of the particular machine's instruction set to the same extent, but they were still very much writing programs by writing specific instructions instead of concerning themselves with the problem they were trying to solve and allowing the compiler to turn that into proper machine code.

American historian of computing and aerospace Paul Ceruzzi describes the situation in 1968:

Despite great strides in software, programming always seemed to be in a state of crisis and always seemed to play catch-up to the advances in hardware. This crisis came to a head in 1968, just as the integrated circuit and disk storage were making their impact on hardware systems. That year, the crisis was explicitly acknowledged in the academic and trade literature and was the subject of a NATO-sponsored conference that called further attention to it. Some of the solutions proposed were a new discipline of software engineering, more formal techniques of structured programming, and new programming languages that would replace the venerable but

obsolete COBOL and FORTRAN<sup>1</sup>. Although not made in response to this crisis, the decision by IBM to sell its software and services separately from its hardware probably did even more to address the problem. It led to a commercial software industry that needed to produce reliable software in order to survive. The crisis remained, however, and became a permanent aspect of computing. Software came of age in 1968; the following decades would see further changes and further adaptations to hardware advances. [42]

Backus describes this as part of the motivation for beginning work on Fortran [19, *The Economics of Programming*]:

Another factor that influenced the development of Fortran was the economics of programming in 1954. The cost of programmers associated with a computer center was usually at least as great as the cost of the computer itself...Thus, programming and debugging accounted for as much as three quarters of the cost of operating a computer; and obviously, as computers got cheaper, this situation would get worse.

Thus this chapter is about the efforts to make programming easier and more productive to address this crisis. Software rose in importance in industry, it became a field of study in academia, and true research on compilers and programming languages began.

## 3.1 The Software Crisis

[TODO: DEC PDP-8 and PDP-11; IBM System/360 and OS/360; Multics; Unix; C] As the 1960s progressed, the notion of *software* became more established, and the programs being written served the authors to a much greater extent. Prior to the 1960s, programs were often tailor-made for a specific machine. There was no hope of re-using the program on another machine. For most of the users, their organization had spent a considerable portion of their budget on their system, and they were expected to use it for a long time. Retargetable compilers did not exist.

## 3.2 Bell Lab's Computing Science Research Center

### 3.2.1 Multics and Unix at Bell Labs

There is no avoiding Multics and UNIX if we are to discuss the development of compilers at Bell Labs. We will cover the history of the American Telephone and Telegraph Company (AT&T) in very broad strokes to contextualize the development of the programming languages and compiler tools developed in this time period.

In very broad strokes, American Telephone and Telegraph Company (AT&T) was a government-sanctioned monopoly that provided telephone service to the United States. They became a monopoly by buying up smaller telephone companies across the country. Over time, they found they needed

---

<sup>1</sup>Readers may find it entertaining that Ceruzzi describes COBOL and FORTRAN as obsolete *by 1968*; both are still heavily used in some industries, and one of the very first compilers to adopt MLIR (to be discussed in a later chapter) is a Fortran compiler[113].

quite a bit of fundamental science in order to keep up with the requirements of servicing the entire country, so in 1925 they created Bell Telephone Laboratories in New York. If they did not continue providing quality service, the government could (and very often did) threaten to break up the company, so this need was existential.

During the second world war, they expanded to New Jersey, early 1960s they moved to Murray Hill, in suburban New Jersey, where they had a few thousand employees. The laboratory was responsible solely for research, and thousands of other employees worked on applying the research and developing the process knowledge needed to bring the research into the phone system. This research led to numerous fundamental discoveries and inventions in physics, materials science and many more domains; these discoveries include the transistor, lasers, fiber-optics and solar cells, to name a small subset. Patents were the laboratory's primary measure of success, and they produced many. At its peak, AT&T was the nation's largest private employer with over 1 million employees. If you are interested in an account of Bell Labs in anything close to the proper detail, I recommend *The Idea Factory* by Jon Gertner; our focus is far too narrow to do it justice here.

In the early 1960s, the labs spun off a new department, the Computing Sciences Research Center, off of the mathematics group. This group had about 25 people tasked with software and computing research, which included operating systems. In 1961, Fernando Corbató at MIT created a timesharing system called CTSS, or the Compatible Time Sharing System, with relative success. In 1965 MIT set out to make a successor system with all kinds of enhancements; they partnered with Bell Labs and General Electric to create the operating system called *Multics*. The machine was developed at GE (*Multics* required a special machine), *Multics* was designed at MIT, and Bell Labs was responsible for large parts of the software development, which was primarily done in IBM's PL/I programming language. Ken Thompson described it as "monstrously over-engineered" and "typical second-system syndrome" [115]; while they had a wonderful time-sharing system in CTSS, they got too ambitious with their sophomore project. Ultimately, this project ran long past deadlines and was slow to run and altogether not what the labs was hoping for, so in 1969 they pulled out of the project. Honeywell took over Bell Labs' share of the project.

This left a bad taste in their mouths, and Bell Labs leadership sought to avoid operating systems work in the wake of this venture. Some of the members of the *Multics* team had other ideas; they had gotten a taste for operating systems work, and now they had not operating system to work on. These included Ken Thompson, Dennis Ritchie, and Doug McIlroy.

At the time, Ken Thompson was researching file systems on an outdated and underused Digital Equipment Corporation (DEC) PDP-7 machine from the acoustics group.<sup>2</sup> He originally just used it for writing video games, until finally getting around to his research on file systems. He developed disc scheduling algorithms to maximize throughput on the PDP-7's disc drive. To adequately test the performance of his file system algorithms, he needed some test programs to stress the file system. At some point in this process, he realized he was not far off from an operating system of his own[115]:

At some point, I realized without knowing it up until that point, that I was three weeks from operating system, with three programs, one a week. I needed an editor

---

<sup>2</sup>Because acoustics research was so core to Bell Labs' and AT&T's mission, that department got just about anything they asked for in terms of computing resources, hence the outdated PDP-7 sat unused in their offices—they already had more powerful machines.

to write code, I needed an assembler to turn the code into language I could run, and I needed a kernel overlay, call it an operating system. Luckily, right at that moment, my wife went on a three-week vacation to take my one-year-old to visit her parents in California. One week, one week, one week, and we had Unix.

Thompson's assembler was not a complete toolchain; there were no libraries, linkers, or loaders. The entire assembly program had to be presented to the assembler all at once, and the output was directly executable. Thus the `a.out` naming convention for the default executable output of modern compilers was born (*assembly output*). Even after the system gained more toolchain components, the name remained.

Very early on, Thompson's system picked up very impressive users. Two at a time, Dennis Ritchie, Doug McIlroy, Morris McMahon, and Brian Kernighan all became users on Thompson's new system. They sent a proposal to get a PDP-10, which was top-of-the-line at the time, to port Unix to a more powerful machine. This was rejected as soon as Labs leadership saw it was related to operating systems, in spite of the fact that their request was well within budget. Joe Ossanna (another colleague) came up with a proposal that positioned their need for a PDP-10 as a research project to develop better typesetting software for filing patent applications (notably missing any mention of operating systems). Ossanna developed the Nroff and Troff typesetting programs to fulfill their claimed goal of helping the patent office. The patent office loved their software, and they ended up buying the computing group a PDP-11<sup>3</sup> sometime in the early 1970s. Unix came up on the PDP-11 almost immediately. Thus we have the basis for discussing the subsequent development of compilers and programming languages at Bell Labs.

### 3.2.2 Personal Histories

The following sections will feature many of the same figures; here we will introduce them so their interleaving stories may be told uninterrupted in the following sections. Readers unfamiliar with the history of Unix may find unfamiliar terms in these personal accounts; these will be discussed in the following sections after our characters have been introduced.

#### Doug McIlroy, Joined 1958

Douglas McIlroy was born in Fishkill, New York. His father worked in electrical engineering and spent time at MIT and ended his career at Cornell, having contributed to the RADAR effort in World War II. His father collected maps, and Doug grew an interest in maps himself. One day, while ill in bed with chicken pox, his father assigned him the task of drawing a dam on a USGS map, and showing which regions would be inundated by the dam [94]. His mother, too, had a master's degree in physics from the University of Rochester, which was very unusual at the time. She was forced to audit some of her classes there, because "women can't take this class! But you can sit in on it, if you want." Thus he was raised in an engineering-minded household.

Barbara, his wife, also studied mathematics and faced discouragement similar to his mother's. They ended up meeting at the laboratory, where Doug recalled that some figures at the labs sought to improve the situation for women, specifically Bernie Holbrook.

---

<sup>3</sup>While many resources will mention the PDP-11, they usually refer to the PDP-11/22; very few of original PDP-11s (which the computing center worked on) were ever produced, but the refreshed models had many users.

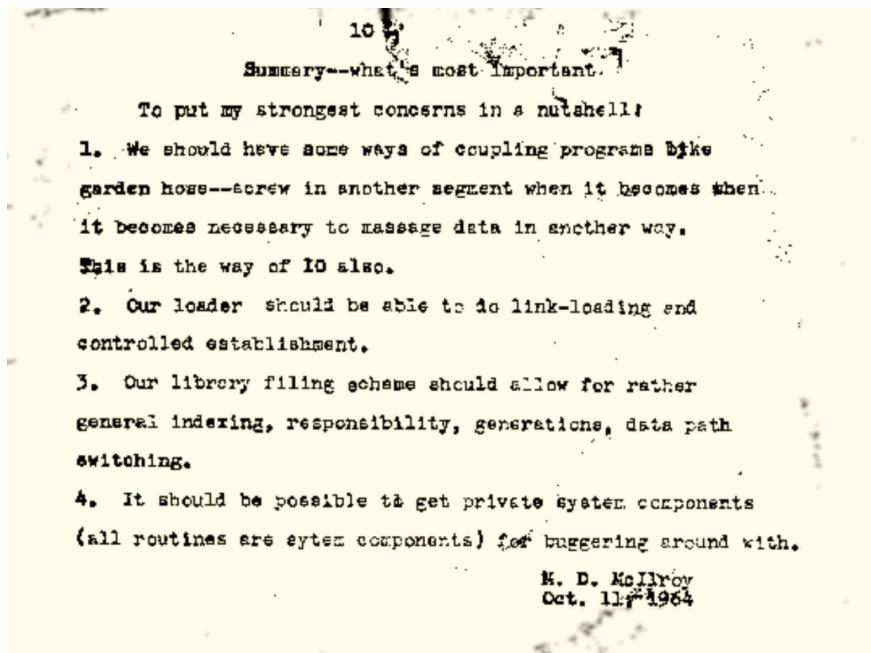


Figure 3.1: Doug McIlroy's 1964 memo proposing Unix pipes[93].

Doug joined Bell Labs in 1958 and shortly thereafter earned his PhD in applied mathematics from MIT. McIlroy became head of the Computing Techniques Research department in 1965, and he's regarded as one of the most brilliant members of the staff by key members that readers may be more familiar with.

He is described by nearly everyone at the Bell Labs Computing Research Center (who wrote or spoke about their time there) as the most brilliant member of that team that no one has heard of [115]; Ken Thompson described him as "the smartest one of all of us and the least remembered or written down of all of us."

He was relatively hands-off as a manager, preferring to peek into his employees' offices with suggestions or interesting problems and wait for the employees to let *him* know what needed to be done, rather than assigning tasks directly. His employees respected his taste in technical and personal matters, as Brian Kernighan describes[73]:

Unix might not have existed, and certainly would not have been as successful, without Doug's good taste and sound judgment of both technical matters and people.

In 1964, he circulated a memo which led to Unix pipes[93] (see 3.1) though it took him quite a while to convince Ken Thompson that he really ought to implement them. Aside from pipes, he wrote many common Unix utilites we still use today, like diff, sort, join, tr, echo, tee, and spell.

He hired Alfred Aho in 1967[3]:

I was interviewed by a department head by the name of Doug McIlroy. He was an applied mathematician from MIT. He had been at Bell Labs for a few years before me. Amongst other things, he had co-invented macros for programming languages and he's also in this class of one of the smartest people I've ever met.

**Ken Thompson, Joined 1966**

[TODO: Ken's oral history is rich and includes fun stories about how he was recruited to the labs.]

**Dennis Ritchie, Joined 1967****Jeffrey Ullman, Joined 1967**

[TODO: This section can be shorter since Ullman went back to Princeton and only consulted at the lab part-time.]

**Alfred Aho, Joined 1967**

Alfred grew up in a Finnish household, and when he showed up to Kindergarten, he couldn't speak any English. In his first report card, the teacher told his parents that "Al can't speak any English!", and in the next report card, the teacher said reported that "Al speaks *too much* English!"; so he learned it relatively late and became quite a social kid. Alfred played music all throughout his childhood, and the schools he went to as a kid had quite good musical programs. He went to University of Toronto for undergraduate school and continued playing the violin all through his years at Bell Labs. He was an only child. He had a proclivity not only for music but for mathematics and reading science fiction.

After finishing his undergraduate degree in Toronto in engineering physics, he got a masters and then PhD from Princeton University in electrical engineering and computer science, completing the latter in 1967. At Princeton took a course from John Hopcroft on computer science theory with a heavy emphasis on automata and language theory, which sparked his long-lasting interest in formal languages. His PhD thesis was on extending the theory of context-free grammars, an area of expertise that would serve him well at the Labs.

One of the fist people he met at Princeton was Jeffrey Ullman, who had also recently started there, beginning a long-term friendship and collaboration between the two [3]:

One of the first people that I met at Princeton was a Columbia graduate by the name of Jeffrey Ullman. He had just gotten his undergraduate degree from Columbia University and also had come to study digital systems in the EE department at Princeton. So, he and I became close friends. When we graduated from Princeton, we both joined the newly formed Computing Sciences Research Center at Bell Labs. There we developed a lifelong collaboration on subjects ranging from algorithms, programming languages, to the very foundations of computer science. I was very fortunate to have met some of the greatest people in the field and to have gotten to know them and work with them.

His PhD advisor was John Hopcroft, whom he would also continue to work with for a long time. He told Alfred to "find his own research problem," and he turned to his interest in programming languages and compilers. He was always very keen on precision; he wanted to use precise terms and he would press even his own friends to be very precise in their informal discussions, and this acuity for precision extended to his thesis, which was on the very precise understanding

of the *syntax* and the *semantics* of programming languages. Alfred would contribute his deep understanding of the theory of automata and programming languages to the Labs, where there were many other brilliant people who were more practical and lacked familiarity with the literature. In 1967 his good friend Jeffrey Ullman had started working at Bell Labs, and a few months after starting there, Alfred interviewed there. He was interviewed by Doug McIlroy, who hired him. For some time, he and Jeffrey worked together at the Labs, but Jeffrey had wanted to spend more time in academia than Alfred, so he returned to Princeton's electrical engineering department, but continued to consult at Bell Labs one day a week. Thus the two continued to work together. Alfred described their collaboration at Bell Labs:

He stayed at Bell Labs for a few years and went to Princeton University where he joined the faculty of the electrical engineering department, but he would come and spend one day a week consulting at Bell Labs. His consulting stint, he would come Fridays and sit in my office all day. The conversations that we'd have would range over all sorts of topics, and sometimes he'd mentioned that he was working on a problem with a colleague at Princeton, and after describing the problem, I might say, "You're kidding," and he said, "Oh, you're right. The solution is obvious, isn't it?" I don't know whether I would say dynamic programming or whatever, but several papers came out of this intense collaboration, and we got to the point where we could communicate with just a few words. We had a very large, shared symbol table. [3]

Ken Thompson joined the labs several months before the two of them to start working on Multics, and had developed a plethora of tools based on regular expressions, such as grep. This combined with the ED and QED editors that came from MIT and were shipped with Multics sparked Alfred's interest in regular expressions.

Aho is probably best known for the textbooks he co-authored with his friend Jeffrey, colloquially known as *The Dragon Book (Compilers: principles, techniques, and tools)*[5] and his book with John Hopcroft on algorithms, *The Design and Analysis of Computer Algorithms*[4].

### Brian Kernighan, Joined 1969

#### 3.2.3 The First Unix Compilers

Alfred Aho described this era of compiler history as a cambrian explosion of programming languages and compiler tools [2], and there were several reasons for that. Tools were developed that made the description of compiler front-end tools extremely simple, the computing center got access to machines with more memory, and the theory of parsing context-free grammars advanced. The Labs employees worked all of these together to produce many compiler tools.

While Doug was the department head of the Computing Sciences Research Center, he would regularly stop into the offices of his employees and drop ideas and requests to them. In one case, he mentioned to Ken how convenient it would be to have a tool for searching text files for patterns. Ken already had a rough version of such a tool in his home directory, so the next day he showed Doug his program and thus was born grep; many such tools came to be like this.

On another similar occasion, Doug walked into Ken's office to discuss the TMG language (standing for TransMoGrifier), designed by Robert McClure[91], one of his friends at the lab. McClure had gotten defensive about this language, and when he left the lab, he claimed it was

proprietary and nobody else could use it. TMG was a top-down recursive-descent compiler-compiler for context-free grammars and procedural elements similar to Yacc, a tool to help write compiler front-ends. See this example from Doug's TMG manual[92] and notice the similarities to modern parser generators and Backus-Naur Form:

This simple program defines the translation of fully parenthesized infix expressions to Polish postfix for a stack machine.

```
expr: <(>/ exp1 expr operator expr <)> = { 3 1 2 };
exp1: ident = { < LOAD > 1 };
operator:
op0: <+>/op1 = { < ADD > };
op1: <->/op2 = { < SUB > };
op2: <*>/op3 = { < MPY > };
op3: </> = { < DIV > };
```

Ken recounts[115] the story of how Doug brought a full TMG compiler *written in TMG, on paper* to Ken's office. He then decided to feed the TMG program into his TMG compiler *by hand* to produce an assembly listing of the TMG program. He then went over to Ken's keyboard and typed in the program that his TMG-TMG compiler had produced, and with astonishingly few errors, they had a working TMG compiler on the PDP-7.

Ken's logical next step was to use this new TMG compiler to write a Fortran compiler for the PDP-7 because "no computer was complete without Fortran." Now, the PDP-7 was only 8k of 18-bit words of memory and Ken's Unix system needed about half of that just to run, leaving only 4k for user programs. Once completed, the new Fortran compiler took far more memory than was available for user programs, so he had to cut down the TMG program to get a program small enough to fit on the machine but capable enough to facilitate a usable programming language. Once he finally cut down his compiler to 4k, he called that programming language *B*. His ill-fated attempt at producing a Fortran compiler ended up being a very different language, based more so on BCPL than on Fortran<sup>4</sup>. BCPL had been designed by Martin Richards at MIT in the mid-1960s, and thus had found its way onto MIT's CTSS system, which Bell Labs employees were familiar with.

### 3.2.4 BCPL and B

BCPL, B and C may differ syntactically, but semantically they are very similar and operate at a similar level of abstraction. The BCPL compiler was allowed to use more memory than Thompson had on the PDP-7, so it allowed for more complicated expression-oriented language features. Dennis Ritchie points out the following two examples as only being possible in BCPL and not in B or C due to memory constraints [104]:

---

<sup>4</sup>The origins are really unclear. Ken himself mentions Fortran as his only inspiration for B in a live interview[115], however Dennis Ritchie describes it as a "language of his own" and "BCPL squeezed into 8K bytes of memory and filtered through Thompson's brain" [104]. He describes the name as either a contraction of BCPL or (less likely) a contraction of Bon, a language Thompson developed for Multics.

```

let P1 be procedure
and P2 be procedure
and P3 be procedure

E1 := valof ( declarations ;
               commands ;
               resultis E2 ) + 1

```

Because the entire program was held in memory, the BCPL compiler could make procedure *P3* available to expressions inside procedure *P1*, but the B compiler was limited to a one-pass technique and could not provide such features. On the other hand, some BCPL features were purposefully left out of B; for example, to share data between separately compiled source files, BCPL programs had to use a global buffer. To make a procedure or variable available to other source files, the programmer had to manually associate the name with an offset into this buffer, similar to the COMMON block in Fortran programs. In more mature B compilers and later in C, the linker handles this automatically.

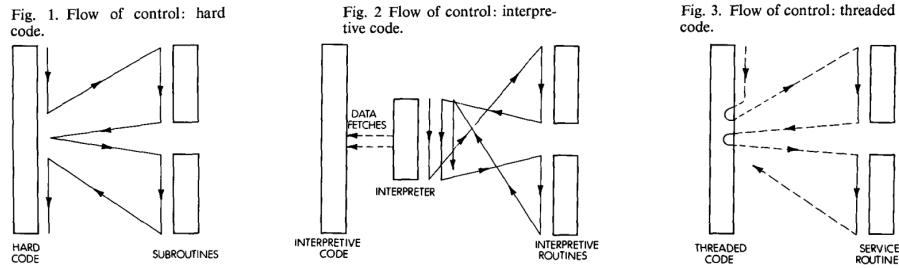


Figure 3.2: Execution of Threaded Code[27]

The B compiler did not emit machine code directly, but instead used *threaded code*[27], an early form of bytecode operating on a simple stack machine, which was then interpreted. James Bell claimed that this threaded code needed no interpreter, but really, the execution model for threaded code is just a specification for an interpreter without outside the routines responsible for executing a given bytecode instruction. Today this would be considered a bytecode interpreter, but in the day when interpreters were only known to interpret the source language directly, his ideas may have seemed distinct from other forms of interpretation. See Bell's depiction of the execution of threaded code 3.2.

Neither B nor BCPL had types; only the addressable size of data for the particular machine. Since pointers were just integers representing offsets into memory, pointer arithmetic was natural in both languages, and both had syntax sugar to facilitate pointer arithmetic and dereferencing of offsets into arrays.

```

/* Declaring an array of 10 integers */
let V = vec 10 /* BCPL */
auto V[10];      /* B */

```

```

/* Accessing element i of array V */
V! i          /* BCPL */
*(V+i)        /* B      */
V[ i ]        /* B      */

```

After Ken had the TMG version of B working, he bootstrapped the compiler by implemented it in B. Dennis Ritchie recalls[104]:

During development, he continually struggled against memory limitations: each language addition inflated the compiler so it could barely fit, but each rewrite taking advantage of the feature reduced its size. For example, B introduced generalized assignment operators, using  $x =+ y$  to add  $y$  to  $x$ . The notation came from Algol 68 via McIlroy, who had incorporated it into his version of TMG.

Dennis also recalls this about their language design and implementation decisions:

Although we entertained occasional thoughts about implementing one of the major languages of the time like Fortran, PL/I, or Algol 68, such a project seemed hopelessly large for our resources: much simpler and smaller tools were called for. All these languages influenced our work, but it was more fun to do things on our own.

By 1970, the team had been able to acquire the PDP-11, which they promptly ported Unix to, along with the B bytecode interpreter and a small assembler written in B. As the number of PDP-11 users grew, so too did their B codebase, which now included quite a few programs and subroutines, as a result of the tedium of writing assembly, which was the only alternative. B proved useful—however, the new machine revealed some serious inadequacies. The only data type in BCPL and B was the cell, roughly equivalent to the hardware's machine word. The 18-bit PDP-7<sup>5</sup> did not map cleanly to the 16-bit PDP-11. BCPL on the PDP-7 had support for floating-point operations through special operators, but this only worked because the floats could fit in a single word and this was not possible on the PDP-11.

For all these reasons, it seemed that a typing scheme was necessary to cope with characters and byte addressing, and to prepare for the coming floating-point hardware. Other issues, particularly type safety and interface checking, did not seem as important then as they became later. Aside from the problems with the language itself, the B compiler's threaded-code technique yielded programs so much slower than their assembly-language counterparts that we discounted the possibility of re-coding the operating system or its central utilities in B. [104]

In 1971, Dennis Ritchie began extending B by adding a character type and directly generating PDP-11 machine code. Thus the creation of a compiler capable of generating efficient machine code on a system with very little memory and the transition from B to C were simultaneous. Dennis renamed the B compiler to NB, or *New-B*, and then C shortly thereafter.

---

<sup>5</sup>18-bit might seem like an odd choice to the modern reader; most mainframe computers had 36-bit words, and the PDP-7 was marketed as a smaller system. Thus at 18-bits, it would be marketed as roughly half a mainframe.

### 3.2.5 C

Dennis's new language now had types and new addressing schemes, and it evolved quickly. *New-B* existed so briefly that no full description of its syntax or semantics was ever written.

The run-time overhead associated with BCPL and B's vector indexing semantics were addressed in C:

Values stored in the cells bound to array and pointer names were the machine addresses, measured in bytes, of the corresponding storage area. Therefore, indirection through a pointer implied no run-time overhead to scale the pointer from word to byte offset. On the other hand, the machine code for array subscripting and pointer arithmetic now depended on the type of the array or the pointer: to compute `iarray[i]` or `ipointer+i` implied scaling the addend `i` by the size of the object referred to. [104]

C gained a type system, overlaying semantics onto the raw bits that BCPL and B users were working with. The type system was heavily inspired by ALGOL 68, though not necessarily in a way the authors of the ALGOL language would have been happy with, as Dennis himself admits [104]. Many features were added to the language in a short time; some of the warts that persist in even modern C were because of Dennis's desire to make porting programs from B to C as simple as possible.

For example, the precedence of `&` and `==` were made equal in C, thereby requiring parentheses in the second expression below:

```
if ( a == b & c ) { /* ... */ }
if (( c & b ) == a) { /* ... */ }
/* ^ Parentheses required here */
```

While many features were added to C in 1972-1973, the preprocessor was likely the most significant. Dennis implemented it partly at the request of Alan Snyder (who had also suggested that Dennis add the `&&` and `||` to make boolean logic more explicit) at MIT in *A Portable Compiler For The Language C*, and partly inspired by the textual inclusion mechanisms in PL/I and BCPL. The first version could only `#include` other files and do simple textual substitution, though it was soon extended by Mike Lesk and John Reiser to handle macros with arguments and support conditional compilation.

In the summer of 1973, the language and compiler were mature enough that the team was able to rewrite the Unix kernel in C. Ken Thompson had tried once before to port Unix to C, but the language did not support structures at the time, making the task too onerous to continue. The compiler was also ported to the Honeywell 635 and IBM 360 and 370, and commonly used procedures coalesced into what eventually became the standard library. The first of these major library components was Mike Lesk's input/output library, which became the basis of the C *standard I/O*. Dennis and Brian Kernighan wrote the seminal *The C programming language*[74]. Brian wrote most of the exposition and Dennis wrote most of the programs. This would come to be common; Brian's gift for technical writing that was easy to understand made him a common author of documentation and publications.

The subsequent period was filled with language features focusing on type safety and portability as Unix and all the tools they had rewritten in C had to be ported to more and more systems,

and much of the codebase remained untyped, harkening back to its B and assembly heritage. In the same vein, pointers and integers were mutually assignable in C, absent of any syntactic indication that the programmer's intent had changed. This practice fell out of style, and *casts* were introduced to the language, inspired by a similar *but distinct* concept in ALGOL 68. Similarly, the member-dereference operation did not care much about the type of the pointer being dereferenced; `ptr->field` could be used regardless of the type of `ptr`, and `field` was taken to mean an offset from some pointer into a structure.<sup>6</sup>

In 1978, Steve Johnson started work on a C compiler that was easy to retarget to other machines while he, Dennis and Thompson worked on porting Unix to the Interdata 8/32. While the language continued to evolve and some older practices fell out of style, Johnson came up with the `lint` program, adapted from his *Portable C Compiler* pcc, to search a set of source files for coding practices that might need to be updated. This is one of the first (if not *the* first) static-analysis tool, its namesake lives on in today's tools; one still talks of a *linter* remarking on a dubious construct in one's code.

The third version of Unix, System III, was the first one to be primarily written in C. As Unix matured and its System III and System V versions were distributed, institutions outside AT&T developed Unix distributions (particularly at the University of California at Berkeley, yielding the Berkeley Software Distribution of Unix), and C compilers were ported to many new machines, C became a mainstay of the software world, as it remains today.

### 3.2.6 Standardization of C

[TODO: using pcc as a reference compiler became impractical. The X3J11 committee formed in 1983 to standardize C.] [TODO: includes early criticism of C.]

### 3.2.7 Regular Expressions: Grep, Yacc, Lex

Regular expressions were a large part in the explosion of new programming languages and compiler tools at the labs in the early 1970s.

One such use of regular expressions was Ken Thompson's s program ("s" for "search") to do pattern-based searching in his home directory sometime after working on Unix on the PDP-11. In one of those casual drop-ins, his boss McIlroy asked him to come up with a program for finding patterns in files and searching through directories and these things, to which Ken responded "ah, let me think about it overnight" knowing he already had the perfect program. After a night of cleaning up his program, he gave it to Doug who loved the program and moved it from Ken's home directory into the shared binary directory. It was at this time renamed grep after the command in the ed editor which performed the same function: g/re/p, for global, regular-expression, print. Many small programs like this formed over time at the Labs.

---

<sup>6</sup>Perhaps they should have preserved more of ALGOL 68's type system; these pointer rules plague optimizers working with C programs to this very day. The modern Linux kernel is *not* compiled with a flag usually spelled `-fstrict-aliasing`, which allows the optimizer to take advantage of the C language's aliasing rules. These aliasing rules specify that a pointer cannot refer to multiple incompatible types, and programs that use such behavior are undefined behavior. This flag would theoretically allow the compiler to better optimize the Linux kernel, however, strict aliasing rules are broken so often in the kernel's source that the flag results in a buggy kernel, so it cannot be enabled. Programs that make use of casting to and from `void*` or type-pun through unions are also common in C, and in general the aliasing rules are not very friendly to optimization when compared to Fortran, for example.

On another occasion, Steven Johnson came into Alfred Aho's office to ask about parsing algorithms for his Portable C Compiler. He was having trouble getting the C parser working properly, so he went to Aho for his theoretical background in parsing algorithms and automata theory and asked how he would go about constructing a parser. Aho replied that first he would construct a grammar for the language, and then apply Knuth's LAR parsing algorithm—Aho got some sheets of cardboard from the stockroom, and did the sets-of-items construction for the grammar that Johnson had given him over the weekend.

On Monday, he would give Johnson the constructed parsing table, Johnson would implement it, and find issues with Aho's cardboard construction. After a few iterations of this, Johnson asked Aho to explain the theory, which he implemented. This program went on to become the yacc program, standing for *Yet Another Compiler-Compiler*, which is used for generating parsers from a context-free grammar. Aho and Johnson continued to improve the performance of the program and it vastly decreased the effort required to create a parser for a new language. Johnson's Yacc program was especially useful because it would inform the user of shift-reduce and reduce-reduce conflicts, so they knew if they had any difficult-to-parse constructs in their grammar. Yacc became one of the most widely used tools for creating parsers, even outside Bell Labs in computer science courses. [TODO: Aho's egrep program that constructed the regexes lazily.]

In a mailing list, Johnson recalled that:

The name comes as a result of a comment by Jeff Ullman – Al was telling him about the program, and he said "What? Yet Another Parser Generator?" So I started calling it Yacc.

The first implementation was really slow. We were all sharing a single PDP 11 with Unix on it, and Yacc could take 20 minutes to generate a 50-rule grammar table. Everyone would groan when i started the program: "Ohhhh. Johnson's running Yacc again".

After Al and Johnson's optimization efforts, the group was eventually able to parse Fortran 77.

One of the fast regular expression algorithms that Aho had come up with also saw use in the lex program, which is used for generating lexical analyzers from regular expressions. Eric Schmidt, summer intern at Bell Labs but perhaps better known today as the CEO and co-founder of Google, used one of Aho's algorithms and worked with Mike Lesk of Bell Labs to produce the first version of lex. Using these tools in tandem allowed for very tight iteration cycles for designing and developing new programming languages and compilers. With Lex for the lexer and Yacc for the parser, one could quickly develop the front-end of a compiler based only on the grammar for the language.

### 3.2.8 Applications of Yacc and Lex: Eqn and AWK

In 1974, Brian Kernighan and Linda Cherry developed one such program called eqn for typesetting mathematical expressions. At the time, Nroff and Troff were the state-of-the-art typesetting tools, and they saw lots of use at Bell Labs (recall that the Labs primary output was publications).

[TODO: expand on eqn and show examples] [TODO: Yacc and Lex made with Aho's help. then everyone started making mini languages.AWK. "Kernighan and Cherry developed a little language for specifying mathematics called EQN using these tools"]

People started using the Kernighan and Lorinda Cherry EQN tool to specify mathematics in their documents and in the research papers that they were writing. They would feed the EQN specification into the typesetting program roff ...Knuth adopted the EQN language to include in the TeX typesetting system, and in LaTeX. It's basically Kernighan and Cherry's way of specifying mathematics. These software tools had a great deal of influence, and Kernighan and Cherry enjoyed the fruits of parsing theory and formal language theory in using the tools Lex and Yacc to create their EQN typesetting language. Knuth has this saying that the best theory is motivated by practice and the best practice by theory. I internalized that with my early experience in the Computing SciencesResearch Center because I found that the theory that we were developing in computer science could be applied to document preparation systems, programming languages, compilers, and so on. It was really avery productive environment. I taught courses on compiler design at local universities, and then when I went to Columbia, I would teach the course on programming languages and their translators ...I might point out that the first Fortran compiler developed by IBM in the 1950s took 18 staff years to create. In my programming languages and compilers course, I organized the students into teams of four or five. Each team had to create their own programming language, and then write a translator for it, and in all the time that I taught the course for almost 25 years at Columbia to thousands of students, never did a team failed to deliver a working compiler in the 15-week course, and I attribute that to the abstractions [3]

Aho: Okay. AWK is a programming language that was created by me, Brian Kernighan, and Peter Weinberger.

Hsu: And it's your three initials that are in.

Aho: Yes. I'm the A in AWK. Weinberger is the W in AWK and Kernighan is the K in AWK.We thought that it was just a throwaway tool for us, nobody really would be interested in it. But it's amazing how much routine data processing there is in the world.The reason the language got to be known as AWK was because when our colleagues would see the three of us in one office or another, and when they'd walk past the open door, they'd say, AWK, AWK, AWK as they were going down the corridor. So we had no choice but to call it AWK because of the good-natured ribbing we got from our colleagues, and because at some Unix conference, they passed out t-shirts that had AWK, and the error message saying "bailing out on or near line five" on them.

[TODO: Ratfor, AMPL, other Kernighan languages.] [TODO: continue with typesetting...]

### 3.2.9 The Dragon Book

Jeffrey Ullman and Alfred Aho continued working together on developing compiler theory and parsing algorithms. Other folks at the Labs would impress on Al that it was important to write about what you were working on to build your reputation in the scientific community, an idea that Jeff also bought into. They decided they ought to write a book together about their parsing techniques in light of how much interest the development of Unix and C had generated. Lots



Figure 3.3: Cover of the first edition of the Dragon Book

of people were newly interested in developing new programming languages and compilers, and they were well-positioned to write about it; thus was *The Dragon Book* born.

As with the algorithms book, what we did was we performed research on efficient algorithms for parsing and for some of the other phases of compilation, wrote papers on those and presented them at conferences. But we took the important ideas that we developed and the community had developed over several decades and codified them into what are now called the dragon books. The first dragon book was published in 1977. We did have theorems and proofs in the book, and Jeff had this brilliant idea that the book should have a cover with a fierce dragon on it representing the complexity of compiler design, and then a knight in armor with a lance. The armor and the lance were emblazoned with techniques from formal language theory and compiler theory to slay the complexity of compiler design. [3]

They published updates to the dragon book over time as well; they co-authored the second edition with Ravi Sethi, another Bell Labs researcher, in the 1980s. The second edition had grown to over 800 pages, and the third edition was published in 2007 at close to a thousand pages. At this point none of the original authors wanted to work on a fourth edition because of how much the field of compiler design had grown since then.

The first edition (with the iconic green dragon in Figure 3.3) had a very heavy focus on lexing, parsing, and semantic analysis, all but omitting optimization techniques. This makes sense because parsing algorithms were Al and Jeff's specialties, but as this book went on to be the seminal compiler textbook for a generation, many student's primary exposure to compiler design was almost entirely focused on front-end design. Of the 600 or so pages in the first edition, only about 100 were dedicated to optimization and data-flow analysis[6, Chapters 12, 13, and 14].

[TODO: Bjarne Stroustrup, C++ (1979); Dennis Ritchie, C (1972); Ken Thompson, B (1969); Brian Kernighan, AWK (1977), AMPL (1976), co-author of The C Programming Language (1978)]

### 3.3 Seymore Cray

The CDC 160 and the Origins of the Minicomputer

The Whirlwind (a computer prototype built at MIT) had a word length of only 16 bits, but the story of commercial minicomputers really begins with an inventor associated with very large computers: Seymour Cray. While at UNIVAC Cray worked on the Navy Tactical Data System (NTDS), a computer designed for navy ships and one of the first transistorized machines produced in quantity. Around 1960 Control Data, the company founded in 1957 that Cray joined, introduced its model 1604, a large computer intended for scientific customers. Shortly thereafter CDC introduced the 160, designed [41]

### 3.4 The DEC VAX and the IBM System/360

Through the 1980s the dominant mainframe architecture continues to be a descendent of the IBM System/360, while the dominant mini was the DEC VAX, which evolved as a 32 bit extension of the 16-bit PDP-11. [41]

### 3.5 Commodification

[TODO: Bill Gates and Paul Allen (Microsoft) | Microsoft BASIC (1975) | Developed the first critical piece of commercial software for personal computers, establishing the doctrine that software should be a purchased, proprietary commodity. Sun Microsystems, each part of the company needed to sell to all the others, reason why their compiler was paid; proprietary Unix;]

### **3.6 Timeline**

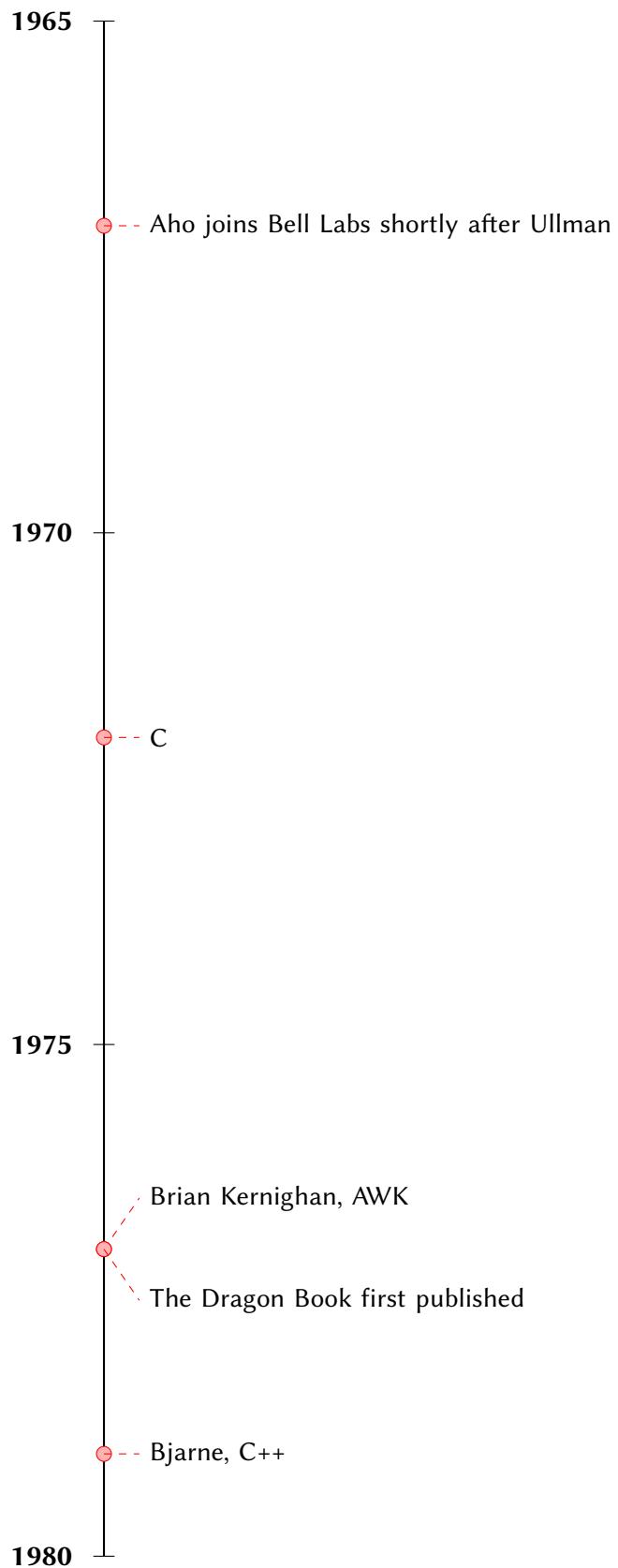


Figure 3.4: TBD, 1965–1980

# 4

# Freedom, 1980-2000

[TODO: gnu linux c llvm python; Facebook php->c++ compiler;]

[TODO: lattner tried to get llvm in the gnu project; new licensing, permissive licensing.]

The discussion of the GNU/Linux operating system and the "open source" software movement, discussed last, likewise has deep roots. Chapter 3 discussed the founding of SHARE, as well as the controversy over who was allowed to use and modify the TRAC programming language. GNU/Linux is a variant of UNIX, a system developed in the late 1960s and discussed at length in several earlier chapters of this book. UNIX was an open system almost from the start, although not quite in the ways that "open" is defined now. As with the antitrust trial against Microsoft, the open source software movement has a strong tie to the beginnings of the personal computer's invention. Early actions by Microsoft and its founders played an important role here as well. We begin with the antitrust trial.[42]

## 4.1 GNU

## 4.2 Adoption of Linux

## 4.3 Low-Level Virtual Machine

Key innovation is that LLVM is a collection of compiler *libraries* that have thin programs wrapping them. Other open-source compilers like GCC tend to be monolithic programs, which can be harder to compose. LLVM contains a collection of sub-projects that can be used independently as tools *or libraries*; key examples being the LLVM sub project of LLVM itself (which contains LLVM Intermediate representation, usually a file format or data structure used inside a compiler to represent the program being compiled (IR), the intermediate representation, the optimizer, interpreter and code generator) and Clang, a C/C++/Objective-C compiler front-end for LLVM. Any other compiler project could emit LLVM IR and fully leverage the LLVM project after the front-end. LLVM's intermediate representation has become the lingua-franca of the compiler world.

From its beginning in December 2000, LLVM was designed as a set of reusable libraries with well-defined interfaces [LA04]. At the time, open source programming language implementations were designed as special-purpose tools which usually had monolithic executables. For example, it was very difficult to reuse the parser from a static compiler (e.g., GCC) for doing static analysis or refactoring. While scripting languages often provided a way to embed the ir, runtime, and interpreter into larger

applications, this runtime was a single monolithic lump of code that was included or excluded. There was no way to reuse pieces, and very little sharing across language implementation projects.[34]

The name "LLVM" was once an acronym, but is now just a brand for the umbrella project. While LLVM provides some unique capabilities, and is known for some of its great tools (e.g., the Clang compiler<sup>2</sup>, a C/C++/Objective-C compiler which provides a number of benefits over the GCC compiler), the main thing that sets LLVM apart from other compilers is its internal architecture.[34, p. LLVM]

Lattner published his thesis on LLVM in 2002, and joined Apple in 2005.

## **4.4 Timeline**

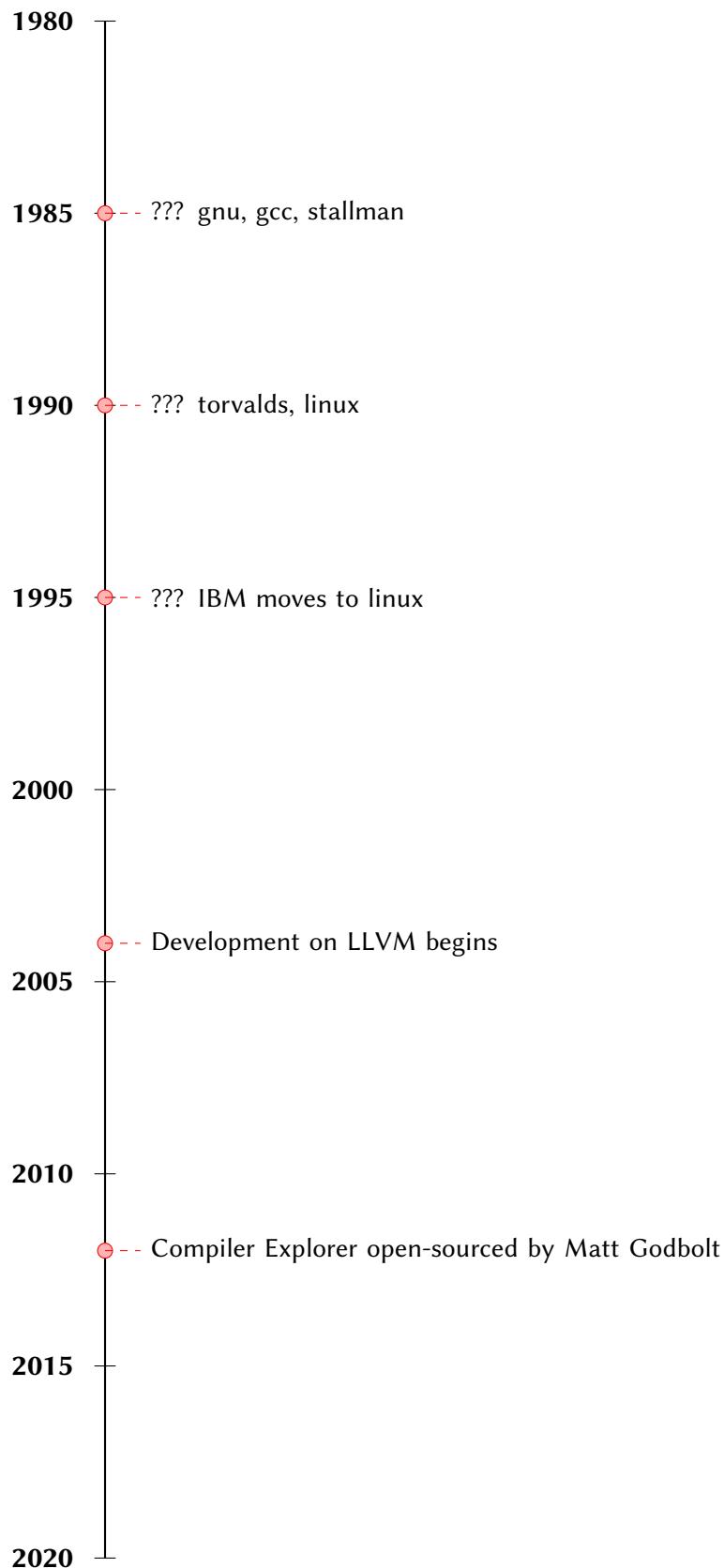


Figure 4.1: Open Source Compiler Development Timeline, 1980–2020

# 5

# Codesign

In each age, contemporaries have attempted to place their work in the overarching history of computing, and some have attempted to look forward and guess at the field's future; in either case, they are often wrong. Computing is a young, volatile industry, and it is often much easier to place a time period in history retrospectively than it is to do so in the moment. Nonetheless, this is what I will attempt to do in this chapter. I am writing about compilers here in the 2020s, the time period in which I work on compilers. Out of hubris I will attempt to contextualize today's compiler work in the context of history. Please let my future readers be forgiving.

But there are still lots of interesting open problems left and one of the most intriguing aspects of compiler design is can we use AI, machine learning, and large language models like GPT-3 to create code automatically from written or spoken specifications. That's still an unfolding story and I'm not willing to trust any program created by an AI program at this point. I wouldn't want it in my pacemaker. I wouldn't want it in my self-driving car or in my airplane. But maybe for a computer game, it's okay. This is what they're creating with these at this time. So even the area of programming language translation is undergoing new approaches and how successful they will be is yet to be determined.[3]

## 5.1 Godbolt

Huge jump in compiler accessibility[53].

## 5.2 Accelerators

Lattner talks on mojo explaining motivation for languages and hw sw codesign. FPGAs. ASICs

## 5.3 MLIR

## 5.4 Flang

## 5.5 Python DSLs

# Quotes

This section should not be included in the final copy; it contains quotes and bibliographic references that may be useful in the writing of the book.

## 5.6 *A Catalogue of Optimizing Transformations*

Allen and Cocke [9] One of the earliest publications about compiler optimizations. Mentions inlining/IPO.

The term *optimization* is a misnomer in that it is not generally clear that a particular, so called, optimizing transformation even results in an improvement to the program. A more correct term would be "amelioration."

## 5.7 “Keynote Address”

Hopper [62]

Mark I had built-in programs for sine, cosine, exponential, arctangent, . . . On the other hand, because they were wired into the machine, they had to be completely general. Any problem that we solved, we found we did not need complete generality; we always knew something about what we were doing—that was what the problem was. And the answer was—we started writing subroutines, only we thought they were pieces of coding. And if I needed a sine subroutine, angle less than  $\pi/4$ , I'd whistle at Dick and say, "Can I have your sine subroutine?" and I'd copy it out of his notebook. We soon found that we needed just a generalized format of these if we were going to copy them, and I found a generalized subroutine for Mark I. With substitution of certain numbers, it can be copied into any given program. So as early as 1944 we started putting together things which would make it easier to write more accurate programs and get them written faster. I think we've forgotten to some extent how early that started.

## 5.8 *The First Computers: History and Architectures*

Rojas and Hashagen [106]

The chief programmer of Mark I, Richard M. Bloch, kept a notebook in which he wrote out pieces of code that had been checked out and were known to be correct.

One of Bloch's routines computed sines for positive angles less than 45 degrees to only ten digits. Rather than use the slow sine unit built into the machine, Grace Hopper simply copied Dick's routine into her own program whenever she knew it would suffice. This practice ultimately allowed the programmers to dispense with the sine, logarithm, and exponential units altogether. Both Bloch and Bob Campbell had notebooks full of such pieces of code. Years later, the programmers realized that they were pioneering the art of subroutines and actually developing the possibility of building compilers.

There were sets of instructions for integers, floating-point numbers, packed decimal numbers, and character strings; operating in a variety of modes. This philosophy had evolved in an environment dominated by magnetic core memory, to which access was slow relative to processor operations. Thus it made sense to specify in great detail what one wanted to do with a piece of data before going off to memory to get it. The instruction sets also reflected the state of compiler technology. If the processor could perform a lot of arithmetic on data with only one instruction, then the compiler would have that much less work to do. A rich instruction set would reduce the "semantic gap" between the English-like commands of a high-level programming language and the primitive and tedious commands of machine code. Cheap read-only memory chips meant that the designer could create these rich instruction sets at low cost if the computer was micro-programmed.

## 5.9 *Grace Hopper and the Invention of the Information Age*

Beyer [30]

Though it is sometimes difficult to identify the motivation behind particular inventions, it appears that a dearth of talented programmers, a personal frustration with the monotony of existing programming techniques, and the lack of resources made available by senior management at Remington Rand to support computer clients led Hopper to invent the technologies and techniques, such as the compiler, that allowed the computers to, in effect, help program themselves. Interestingly enough, as her A-0 compiler evolved into the A-1 and the A-2, Hopper's reasoning in regard to the invention changed. Compilers became less about relieving programmers of the monotony of coding and more about reducing programming costs and processing time.

*Programming* was considered the action of writing machine code directly; writing code in a high-level language was not even considered programming. The motivation for writing Hopper's first compiler was to offload this task to the computer itself.

First and foremost, the central motivations for automatic programming were far more personal in the 1952 paper. With the construction of a functioning compiler, Hopper hoped, "the programmer may return to being a mathematician." Though Hopper had sincerely enjoyed the challenge of coding since first being introduced to computers 8 years earlier, she wrote, "the novelty of inventing programs wears off and degenerates into the dull labor of writing and checking programs. The duty now looms

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as animposition on the human brain." By teaching computers to program themselves, Hopper would be free to explore otherintellectual pursuits

These computing pioneers, according toHopper, created machines and methods that removed the arithmetical chore from the mathematician. This chore, however, wasreplaced by the new burden of writing code, thus turning mathematicians into programmers. Hopper's paper boldly offers thenext step in the history of computing: shifting the human-machine interface once again so as to free the mathematicianfrom this new burden, and making "the compiling routine be the programmer and perform all those services necessary to theproduction of a finished program."

He [the mathematician] is supplied with a catalogue of subroutines. Nolonger does he need to have available formulas or tables of elementaryfunctions. He does not even need to know the particular instructioncode used by the computer. He needs only to be able to use the catalogue to supply information to the computer about his problem.<sup>20</sup>The "catalog of subroutines" was a menu that listed all theinput information needed by the compiler to look up subroutines in the library, assemble them in the proper order, manageaddress assignments, allocate memory, transcribe code, and createa final program in the computer's specific machine code.<sup>21</sup> Asubroutine entry in the catalogue consisted of a subroutine"call-number" and the order in which arguments, controls, andresults were to be stated. The call-number identifi ed the type ofsubroutine (t for trigonometric, x for exponential, etc.), specifiedtransfer of control (entrance and exit points in each subroutine),and set operating and mem-ory requirements. In fact, languagesuch as "call-number" and "library" compelled Hopper to nameher program generator a "compiler," for it compiled subroutinesinto a program in much the same way that historians compilebooks into an organized bibliography.<sup>22</sup>

A program generated by a compiler could not only berun as a stand alone pro-gram whenever desired; it also "may itselfbe placed in the library as a more advanced subroutine." Thissuggested that subroutine libraries could increase in size andcom-plexity at an exponential rate, thus enabling mathematiciansto solve problems once deemed impossible or impractical.<sup>23</sup>

Hopper ends the paper by establishing a short-term road mapfor the future de-velopment of compilers. She describes a "type B" compiler, which, by means of mul-tiple passes, could supplement computer information provided by the programmer withself-generated information. Such a compiler, she imagines, wouldbe able to au-tomate the process of solving complex differentialequations. To obtain a program to compute  $f(x)$  and its first  $n$  derivatives, only  $f(x)$  and the value of  $n$  would have to be given.The formulas for the derivatives of  $f(x)$  would be derived byrepeated applica-tion of the type-B compiler.<sup>24</sup>Hopper also admits that the current version of her com-piler did not have the ability to produce efficient code.For example, if both sine and cosine were called for in a routine, [TODO: make note about me doing this in NVHPC compilers] a smart programmer would figure out how to have the programcompute

them simultaneously. Hopper's compiler would embed both a sine subroutine and a cosine subroutine in sequence, thus wasting valuable memory and processing time. Hopper states boldly that the skills of an experienced programmer could eventually be distilled and made available to the compiler. She concludes as follows:

Although the test results appear to be a smashing endorsement of the A-0 compiler, Ridgway dedicates a substantial amount of his paper to the inefficiency of run-programs. (A "run-program" was the final product of the compiler process. Today, such a program is called object or machine code.) During the 5 months since Hopper had introduced compilers, critics had pointed out that run-programs generated by compilers were less efficient than those created by seasoned programmers... Furthermore, an hour of computer time was far more costly in 1952 than an hour of programmer time.

Ridgway acknowledged that using compilers took up more computer time, both as a result of compiling a program and as a consequence of inefficient code. But "in this case," he argued, "the compiler used was the 'antique,' or A-0, the first to be constructed and the most inefficient." Ridgway was confident that Hopper and her team at the Computation Analysis Laboratory would construct new compilers that "squeezed" coding into "neat, efficient, and compact little packages of potential computation."<sup>29</sup>

A closer look at the manual for the A-2 compiler (produced by the Computation Analysis Laboratory during the summer of 1953) suggests that, despite significant improvements over the A-0 compiler, automatic programming had its limitations. Hopper's vision of intuitive, user-friendly, hardware-independent pseudo-codes generating efficient running programs was far from realization. The A-2 provided a three-address "pseudocode" specifically designed for the UNIVAC I 12-character standards. The manual defined "pseudo-code" as "computer words other than the machine (C-10) code, designed with regard to facilitating communications between programmer and computer."<sup>32</sup> Today we refer to it as source code. Since pseudo-code could not be directly executed by the UNIVAC I, the A-2 compiler included a translator routine which converted the pseudocode into machine code. (See table 8.3.) The manual states that the pseudo-code is "a new language which is easier to learn and much shorter and quicker to write."<sup>33</sup>

The most groundbreaking change was the A-2's ability to debug pseudo-code and flag errors automatically. The compiler generated twelve-character error codes that captured the nature of the error, a miraculous innovation for any programmer who had experienced the pain and monotony of debugging computer code. (See table 8.4.)

Pseudocode was actually the innovation of compilers as we know them today, which didn't wasn't part of Hopper's compilers until the A-2. Prior to that, it was really a way to link/load programs from a library of subroutines.

First, the designer of the compiler now was a linguist. That is, the compiler programmer had the ability to design the syntax of the pseudo-code.

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Not only would it be far easier to learn than machine code; its intuitive logic would help users debug their work. "I felt," Hopper recalled, "that sooner or later...our attitude should be not that people should have to learn how to code for the computer but rather the computer should learn how to respond to people because I figured we weren't going to teach the whole population of the United States how to write computer code, and that therefore there had to be an interface built that would accept things which were people-oriented and then use the computer to translate to machine code."<sup>38</sup> She mentioned that compilers could be designed to program the machine code of any computer. "A problem stated in a basic pseudo-code can thus be prepared for running on one or more computers if the corresponding compiler and subroutine library is available," she wrote. Just as the compiler freed the user from knowing how to program in machine language, pseudo-code was now liberated from a specific type of hardware. A payroll pseudo-code could run on a UNIVAC or an IBM computer, so long as the appropriate compiler was running on both. Hopper stated that as of May 1952 such a benefit was theoretical, insofar as her laboratory had tried it only once, with inconclusive results.<sup>39</sup> Though interpreters were simpler to use than programming in machine code, Hopper believed the approach was a step in the wrong direction. Compiling the A-2 pseudo-code was time consuming in the short term, but the resultant run-program eliminated these six interpretive steps and thus could run more efficiently. In her final comparison of interpreters and compilers, Hopper wrote: "In both cases, the advantage over manual programming is very great, once the basic subroutines have been tested and proved. The saving of time for a compiler is usually greater."<sup>40</sup>

Moreover, Hopper's network of invention attracted the enthusiastic participation of many women in the programming field. Nora Moser (of the Army Map Service), Betty Holberton (at the David Taylor Basin), Margaret Harper (of the Remington Rand/ Naval Aviation Supply Office), and Mildred Koss (of Remington Rand) viewed the compiler as more than just a new programming concept. Indeed, they saw it as the centerpiece of an innovative automated system of programming that they had a hand in creating.

The fact that Hopper wholeheartedly welcomed non-UNIVAC personnel to learn about the A-2 compiler sheds some light on her beliefs concerning intellectual property. Hopper did not view software as a commodity to be patented and sold. Rather, she took her cue from the mathematics community. Like most other academics, mathematicians shared information universally, in order to advance knowledge.

Reflecting on the negative reactions of some of her fellow programmers, Hopper expressed the belief that arguments focusing on "efficiency" and "creativity" covered far baser motivations: "Well, you see, someone learns a skill and works hard to learn that skill, and then if you come along and say, 'you don't need that, here's something else that's better,' they are going to be quite indignant." In fact, Hopper felt that by the mid 1950s many programmers viewed themselves as "high priests," for only they could communicate with such sophisticated machines. Hopper was not the only one

who came to this conclusion. John Backus, developer of Speedcode and later of FORTRAN, was conscious of the programming community's reaction to his contributions: "Just as freewheeling westerners developed achauvinistic pride in their frontiersmanship and a corresponding conservatism, so many programmers of the freewheeling 1950s began to regard themselves as members of a priesthood guarding skills and mysteries far too complex for ordinary mortals."<sup>54</sup> But the more the likes of Backus and Hopper preached the benefits of automatic programming, the more concerned the programming priesthood became about the spreading technology.

## ***5.10 Dealers of Lightning: Xerox PARC and the Dawn of the Computer Age***

Hiltzik [60] Vendor lock-in was tremendously powerful at the time, if only for hardware reasons (expensive, difficult to move/transition). The instruction set was only part of the problem. Compilers gave a way to break out of vendor lock-in at the ISA level by giving programmers a higher-level target, but still depended on orgs being able and willing to switch hardware.

Once IBM sold the system to United Airlines, it could rest assured that the frightful effort of rewriting software, retraining, staff, and moving tons of iron and steel cabinets around would make unit very long and hard before replacing its IBM system by one made by, say, Honeywell.

Xerox and SDS execs wanted to try and break out of scientific computing and into business computing, competing with IBM. When the Xerox folks met with potential business customers, compilers were part of the value chain. Chapter 7. The Clone.

"How good is your COBOL compiler?" they asked... On hearing the question, Bob Spinrad recognized as though for the first time the enormity of the task confronting the company. Scientific and research programmers, like those who worked for SDS and its traditional customers, would not be caught dead working in COBOL, which they considered a lame language suitable only for clerks and drones. He shifted uneasily in his chair. "It's not a question of how good our COBOL compiler is," he told the visitors. "Why not?" "Because we don't have one."

The SDS folks didn't understand software's role in value chain for users, and they weren't willing to make hardware changes.

Headquarters executives thought of software as the gobbledegook that made a machine run, like the hamster driving the wheel. They could not understand why the decision between the PDP-10 and the Sigma needed to be any more complicated than, say, choosing an albino rodent over a brown one. But from a technical point of view, the issue was hardly that casual. Software was the factor that defined the fundamental incompatibility between the Sigma and PDP machines and the superiority, for PARC's purposes, of the latter. The architectures of the two computers were so radically different that software written for the PDP would not properly fit into the

memory space the Sigma allocated for data. Although it was theoretically possible to simply "port" all the PDP software over to the Sigma, the CSL engineers calculated that such a job would mean rewriting every single line of every PDP program, a task that would take three years and cost \$4 million dollars...They had made an issue out of hardware—what machine they could buy –when their real concern was software—what programs they could run.

PARC folks replicated the PDP-10's instructions in microcode.

## 5.11 *A New History of Modern Computing*

Haigh [56]

The development of Unix shifted gradually from assembler to a new higher-level language...Instead of writing, a whole operating system, all that was needed was a C compiler able to generate code in the new machines language, and some work to tweak the Unix kernel and standard libraries to accommodate its quirks.

## 5.12 “Konrad Zuse’s Z4: architecture, programming, and modifications at the ETH Zurich”

Speiser [111]

[Eduard Stiefel] sent two of his assistants, Heinz Rutishauser and myself, to the United States with the assignment of studying the new technology in order to start a similar project at the ETH. We spent most of the year 1949 with Howard Aiken at Harvard and John von Neumann at Princeton, but we also looked at other installations, among them the ENIAC at Aberdeen and the Mark II at Dahlgren. We gratefully acknowledge the hospitality with which we were received and the openness with which we were given information. Despite the fact that the Z4 was only barely operational, he decided that the idea of transferring it to Zurich should by all means be considered.

Zuse introduced the undef / poison values in the original Z4!

In the following sections, expressions such as "hardware", "software", "machine language", "compiler", "architecture" and the like are used freely, although they were unknown in 1950. They only arrived a decade later, but the underlying concepts were quite familiar to us. Konrad Zuse must be credited with seven fundamental inventions:...4. Look-ahead execution: The program's instruction stream is read two instructions in advance, testing if memory instructions can be executed ahead of time.6. Special values

We also made some hardware changes. Rutishauser, who was exceptionally creative, devised a way of letting the Z4 run as a compiler, a mode of operation which

Zuse had never intended. For this purpose, the necessary instructions were interpreted as numbers and stored in the memory. Then, a compiler program calculated the program and punched it out on a tape. All this required certain hardware changes. Rutishauser compiled a program with as many as 4000 instructions. Zuse was quite impressed when we showed him this achievement.

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# Acronyms

**IR** Intermediate representation, usually a file format or data structure used inside a compiler to represent the program being compiled. 66

# Glossary

**bootstrap** The process of writing a compiler in the language that it compiles, such that an older version of the compiler can be used to compile a newer version of itself. 57

**bytecode** A compiler intermediate representation for the purpose of interpretation or execution instead of optimization. 4, 56, 57

**Fortran 77** The 1977 version of the Fortran programming language's standard. 60

**undefined behavior** Source code constructs that are illegal as per the language's specification. Typically, undefined behavior assumed never to happen in a well-formed program, is used by optimizers when certain compiler flags are enabled (such as `-fstrict-aliasing`). 59