C. Graduate Study-- Regular Departmental Graduate Students, ca. 1955

The first generation of graduate students interested in computers were still affiliated with regular academic departments. They met all the regular requirements for their disciplines.

However, the graduate students' advisors, themselves often returnees from the wartime computing programs, arranged for the students to get computer training somewhere or other. This did not have to be on-campus; it might equally well consist in being sent to a summer job at a government laboratory somewhere.

This course of instruction, beyond programming per se, came to consist of teaching the students how to build compilers and kindred programs, as a point of departure for linguistics research, the same point of departure that their mentors had reached while doing applied research in the employ of the military-industrial complex. Thus, this first generation replicated the precarious balance of their elders.

When computers first arrived, their influence was limited by their scarcity. Sometimes, graduate students got only very limited access to computers, computer centers, and the people who worked in them. The students got enough exposure to tantalize them, and to yield important long-term consequences, but in the short run, this was not enough to derail their graduate programs, and force institutional changes. The students got their Ph.D.'s before they started to act drastically on their new ideas about computers.

Thomas Keenan was a physics graduate student at Purdue in 1954, when the university ordered a computer. One gathers that the machine's arrival was too late for him to use it in his own research, for which he used desk calculators (he completed his doctorate in 1955). However he attended the training sessions, and became knowlegeable about computers. After graduation, he got a job at the University of Rochester, Rochester was also getting a computer, and Keenan was put in charge of the emergent computer center (OH 217,Keenan p. 3).

It took somewhat longer for computers to filter down to people who were not in recognized quantitative disciplines. In 1964, Bruce Buchanan, who had been a mathematics major as an undergraduate, was writing his dissertation in Philosophy at Michigan State University, on the subject of scientific discovery, trying to treat it in a logical way. This is known as "operationalizing" a concept, reducing it to a model which is at least verbally specific (though not necessarily specific enough to stand up to being programmed). He had written the first half, which would probably have been a literature survey. That summer, Buchanan applied for a job as a policy analyst at System Development Corporation. He didn't get the job, but his application got passed around to potentially interested parties--notably Edward Feigenbaum at The Rand Corporation. Feigenbaum, of course, was gearing up to launch the "expert systems" school of artificial intelligence, in the wake of Newell, Simon, and Shaw. He was naturally interested in anyone trying to reduce scientific work to something precise enough to be programmed.

At RAND, Buchanan not only "...learned a lot about computing"(p. 4), but was exposed to the unpublished or quasi-published works of Newell, Simon, and Shaw. He met people with more nearly kindred interests than he could find in his home department. At summer’s end, he went back to Michigan State and wrote the second half of his dissertation, along Newell-Simon-and-Shaw lines. He then applied to Feigenbaum for a letter of reference. Feigenbaum offered him a job instead, and Buchanan accepted it, shelving his plans to teach Philosophy. (OH 230, Buchanan, pp. 4-5)

At Michigan State, Computer Science was emerging as an undergraduate program, and as a branch of electrical engineering (, ref harry hedges, oh 221.). One does not know how much computer access Buchanan had before the summer of 1964, but he would have had to fight his way through all kinds of bureaucratic barriers to establish contact with the computer people on the Michigan State campus who might potentially be interested in his work.

In both cases, the incipient computer scientists were obliged to delay doing anything substantial about their new interests. This meant that potential conflicts with their original disciplines did not come out into the open.

As computers became more abundant, they were used especially by mathematics and hard science students. This, however, did not trigger conflict. Mathematicians and hard scientists were operating in the real world, not in an ideal one. Applied mathematics already existed before the computer. The effect of a shift to computer-based applied mathematics was to diminish the role of routine calculation with adding machines, which the wartime experience had shown could be done by clerks. The work that students were doing with computers involved comparatively ambitious projects, which had a comparatively high mathematics content.

Gene Golub was recruited by the University of Illinois computer center in 1953, when he had just finished his bachelors’ degree,(BAB OH 105, Golub, p. 4-5) and got his phd in 1959, in mathematics. In the meantime, the mathematics department seems to have impinged on him very little. Golub observed that mathematicians were not as enthusistic about computing as other fields,

> "...But there was none of the hostility that you would find at Stanford towards computing. I think people just realized that the computer was there but they didn’t, there was no anger in their attitude towards computing."(ibid, p. 19)

He must have taken the usual courses, examinations, etc., but they were apparently so uneventful as not to be worthy of mention. However, the major business of the computer center at Illinois seems to have been numerical analysis. This worked out to taking the mathematical unfinished business of the sixteenth to nineteenth centuries, and recasting it in terms of twentieth century mathematical orthodoxy. As such, numerical analysis is essentially conservative, like teaching mathematics, and was unlikely to attract strong animosities once the issues were properly understood.

The situation was even clearer in physics. Physicists' approach to mathematics is of course results-oriented. They are, perforce, applied mathematicians on the side, and the only real question was what kind of applied mathematics.

Joseph Traub was a good example of a physicist in the process of becoming a computer scientist. Traub's family was a family of German jewish emigres, with a long tradition of producing professional men such as rabbis and doctors. They had gotten out at about the last possible moment. Traub's formerly upper-middle-class father happened to be a bank official, one of those professions which does not travel well. He could only find marginal employment in the United States. This kind of family is sometimes called "sunken middle class," waiting for a son to grow up and get through school so that the family can resume its former status. Traub went to Bronx High School of Science, where he played chess as an extracurricular activity. He was not interested in ham radio, but he was interested in mountain climbing. In fact, his interests were substantially the normative ones of a European schoolboy in a French Lycee or German Gymnasium. (OH 70. Traub, p.3-10)

Traub went to City College on a Regent's Scholarship, living at home. Presumably the scholarship money went for his share of housekeeping expense. He majored in physics and minored in mathematics, taking advanced calculus from Emil Post. Post did not lecture, but conducted the class as a collective oral examination. This set a standard that Traub's graduate school coursework would fail to match in his eyes. Traub started graduate school in physics at Columbia in early 1954, with a teaching assistantship (ibid, p. 11-14).

Within a year or two, some time in 1955, Traub got involved in IBM's on-campus Watson Scientific Computation Laboratories. A friend told him about it, and suggested he go over, and it was apparently possible to just go in and talk to someone in authority. The Watson Laboratory had a bureaucratic alter ego as Columbia University's Committee on Applied Mathematics, on which the physics department, inter alia, was represented. In 1957, IBM gave Traub a generous fellowship, of about $2000, with unlimited computer time. He afterwards estimated that his thesis required something like a thousand hours of computer time (ibid, pp. 14-16, 17-18).

Meanwhile, the physics department per se was not engaging Traub's energies. Physics does not have comprehensive examinations in the same sense that liberal arts fields do. The system of examinations and courses is actually a qualifying examination system, designed to insure that students learn a little about all of the branches of the discipline, and, further, this is typically spread out over three years, instead of being concentrated in the first year. There is only minimal opportunity for specialization in the formal coursework and examinations. The system is seemingly contrived to compel a very bright student to spend a couple of years messing around in a laboratory, instead of completing comprehensive exams in the first year, and doing his dissertation research in the second.

Traub was distinctly underimpressed by the academic side of the physics department:

> ... my feeling is that I learned a smattering of math, a smattering of physics, a smattering of numerical methods in school. But the way I really learned something is I would get interested in it because of my research, and then I would just gobble it up, or I would create things. I think in some ways it may have been an advantage that my formal training, either because I wasn’t interested in somebody else’s agenda or because I thought the teacher was so bad, was such a smattering. That, in fact, if anything, may have been helpful. But I do not feel like I had a good education. (ibid, p. 17)

At any rate, Traub describes professors who distributed copies of their lecture notes, and then lectured from them, and gave exams in the undergraduate fashion, and turned a blind eye to class-cutting (ibid, pp. 16-17).

Traub did his messing around in the IBM Watson Laboratory. Somewhere in the process, he ceased to be a physicist, but the requirements for comprehensive examinations were apparently sufficiently low that this did not interfere with his passing them. When it came time to propose a dissertation topic, he wanted to do chess (this being immediately after Arthur Samuels). This was not allowed, of course, but he was given an equation from physics to solve by numerical methods instead. In 1959, he finished his Ph.D. (under the Committee on Applied Mathematics), and went to work at Bell Labs (ibid, pp. 24-25, 28)

M. Granger Morgan is an example of a physicist who went through an even more complicated divergence, indicative of the sheer extent to which physics departments would accomodate people diverving from the norm. In the course of his undergraduate work (at Harvard), and the early stages of graduate school (at Cornell), he discovered that he had all kinds of complicated humanistic interests. He managed to visit Latin America on the pretext of working at astronomical observatories in Peru and Puerto Rico, and then went off to do Latin American history at Berkeley. However, this didn’t suit him either. His humanistic interests were too eclectic, and too focused around science and technology. As Morgan explains:

> In those days there weren’t doctoral programs like the one here [at Carnegie-Mellon] in Engineering and Public Policy, I knew I had to have a Ph.D. in something. I looked around and figured I could get a Ph.D. in applied physics faster than I could get one in anything else (p. 4).

Morgan’s former advisor from Cornell had gone to set up a new department at U. California, San Diego. Morgan followed, becoming the first or second Ph.D student. In the nature of things, his physics skills would have been highly portable. He would have been able to pick up quickly where he had left off when he had left Cornell, and he would not have had to cope with the kinds of complex identity crises which are the norm in the liberal arts. Morgan’s new thesis advisor was in the process of becoming a computer scientist himself, by small increments, to the point that he was running the campus computer center. At any rate, Morgan, in his spare-time reflections, became convinced of the potential of computer programming as a means of social mobility. (p. 3-4)

In the last year of his doctoral work (phd 1968, per cmu department website), he launched a practical experiment. He found a group of underprivileged teenagers, employed in a federally funded make-work scheme, and arranged to teach them to program, using the computer in his laboratory. It was a roaring success. With his advisor’s encouragement, Morgan began scaling up the program, and putting it on an institutional basis. This of course involved doing the work of a school administrator. He then offered a course in "technology and public policy." This again, would have been an obviously useful thing to do, giving the physics department an interesting and probably popular course for the liberal arts undergraduates to take in order to meet the science requirement (p. 4-5).

In due course, Morgan went on to the National Science Foundation, and eventually, the Engineering and Public Policy program at Carnegie-Mellon. (p. 6-9)

Mathematics and Physics departments were willing to find common ground with the emergent computer scientists. There was practically always something that the emergence computer scientist could do, which was interesting as a computing problem, and was also desirable to the mathematics or physics department. However, there were often other departments willing to make an even better offer.

Some graduate students got involved in computing through departments where there was an understanding of carte blanche. For example, circa 1950, there was an understanding that a research masters in engineering constituted full academic qualification. Given the basic engineering value of elegant simplicity, the Ph.D. in engineering is inherently a bit contrived. It implies that the dissertation author spends a year without generating any results finished enough to publish. That may sometimes be necessary, but it is a situation to be avoided if possible. The ongoing development of abstract mathematical methods (of which computer techniques are paradoxially an extreme case) meant that there was less and less necessary knowlege for a student to learn, and correspondingly less justification for lengthening the curriculum. There is no real tradition of monographic writing in science and engineering generally-- the tradition is that of the journal article, often very short, and the fruit of a month’s or a couple of weeks’ work. The result was that an engineering department, once it decided to start offering the Ph.D., had an intellectual gap to fill up, and could therefore be very catholic indeed in what it allowed students to do for a Ph.D.

Ralph Griswold, eventual founder of the Computer Science department at the University of Ariszona, and inventor of a couple of programming languages, is an example of the type of student such a department could sponsor. Griswold's father was a civil servant (State Department). Griswold majored in physics as an undergraduate at Stanford, more or less by accident. He spent his ROTC obligated service in the navy, teaching, by rote, Nuclear Warfare, a subject he had no interest in, and decided that he did not want to be a teacher. When he went back to Stanford for graduate work, he chose the electrical engineering department, because the "... EE department looked like it was a place that would give the opportunity to get an unusually broad education."(BAB OH 256, Griswold, p. 5) His interests went as far afield as metaphysics, apart from the more mundane areas such as artificial intelligence. Once he got his degree, in 1962, Griswold moved on to Bell Labs. (ibid, pp. 3-5, 8-10)

At the opposite end of the spectrum from engineering was Education. For institutional reasons, education schools had expanded far beyond their theorectical basis in psychology. The situation was opposite from that in engineering-- children are too complicated for theories and formulae to be of any use in dealing with them. Getting an advanced degree in education was something of an exercise in "ticket punching," in which teachers got a pay increment for having a masters, and school administrators were expected to have doctorates. As James Koerner documented in <u>The Miseducation of American Teachers</u>, an Ed. D. might very well work out to sending out questionaires to school districts to ask how they used school busses. (180-192) In this climate, a graduate student who wanted to do something-- anything-- really well simply did not have to worry about orthodoxy.

In the early 1960's (1963?), Dale Lafrenz enrolled as a graduate student in the mathematics department at Minnesota, having previously gotten a bachelors degree in mathematics education and spent a couple of years teaching. He soon discovered that he was not a mathematician, but rather a mathematics educator. He transferred to the education school, and got a job as an instructor in the university's "laboratory" high school. [the account is slightly unclear, but confirm this. states that he got a math degree at marquette in the summers]. In 1963, he and his colleagues started teaching the high school students to program computers. (BAB OH 315, Lafrenz, pp. 4-5)

At the time, the usual and customary method of programming was to write programs, submit them to the computer center, and get a printout back, eventually. This did not fit very well with children's attention spans, of course. Lafrenz and his colleagues heard about John Kemeny at Dartmouth, made contact, and arranged to use his interactive computer system running BASIC. Kemeny's computer was made by General Electric, and the GE foundation came through with a grant to pay for the telephone charges to connect up from Minnesota. Eventually, Pillsbury in Minneapolis bought a copy of Dartmouth's software, and the University high school was able to use it, thus saving long-distance telephone charges. On the new terms, the education school group was able to scale up their project, and turn it into an outreach program.(ibid, pp. 6-11) Lafrenz spent two years running the outreach program, and then in 1970, he moved over to Honeywell, which had decided to get into the computer outreach business on a commercial basis.

(ibid, pp. 11-15)

The fact that graduate students were becoming interested in computers did not imply the emergence of computer science departments. Apart from anything else, the sheer scarcity of computers delayed the process for a few years. Even then, the departments containing potential recruits were able to negociate a compromise. Finally, there were always some departments whose internal imperatives led to eclecticism, instead of leading to the formulation and definition of an orthodoxy.

Wheeler 132

An Interview with DALE LAFRENZ, OH 315, Conducted by Judy E. O'Neill on 13 April 1995, Minneapolis MN

An Interview with RALPH and MADGE GRISWOLD, OH 256, Conducted by Judy E. O'Neill on 29 September 1993, Minneapolis, MN

An Interview with BRUCE G. BUCHANAN, OH 230, Conducted by Arthur L. Norberg on 11-12 June 1991, Pittsburgh, PA

An Interview with THOMAS A. KEENAN, OH 217, Conducted by William Aspray on 28 September 1990, Washington, D.C.

An Interview with JOSEPH F. TRAUB, OH 70, Conducted by William Aspray on 5 April 1984, Columbia University (New York, NY)

An Interview with GRANGER MORGAN, OH 224, Conducted by Andrew Goldstein on, 27 November 1990, Pittsburgh, PA

An Interview with GENE GOLUB, OH 105, Conducted by Pamela McCorduck on 8 June 1979, San Francisco, CA

James D. Koerner, The Miseducation of American Teachers, Pwenguin Books, Baltimore, 1965, orig. pub. 1963