THE 'FIRST COMPUTER' CONTROVERSY

The article titled "The First Electronic Computer" by Allan R. Mackintosh (March 1987, page 25) relies heavily for proof of its claim upon the decision of Judge Earl Larson and testimony given during the ENIAC patent trial. Thus it perpetuates the historical misconceptions originating in that decision. Many authorities believe those misconceptions to have been caused by the judge's lack of scientific understanding (when the trial began, for instance, Larson knew nothing about computers, including the differences between digital and analog computers, and had to receive special tutoring) and by the Sperry Rand lawyer's failure to produce evidence proving that John Mauchly's ideas for an electronic computer predated his visit with John V. Atanasoff in Ames, Iowa.

Sperry and its descendants have never found it necessary to explain why the company lawyer failed to bring out this evidence in the trial, but it seems logical that considerations other than a desire to establish the historical truth might play a part in a patent battle. Mackintosh fails to note that after Sperry sued Honeywell for patent infringement, Honeywell countersued Sperry for restraint of trade. At the same time that he declared the ENIAC patent invalid, Larson also declared Sperry not guilty of restraint of trade. Thus Sperry was victorious on the one hand and defeated on the other. One can only conclude that "despite the enormous economic interests involved" in the patent infringement part of the dispute, there must have been even more enormous economic interests involved in the restraint-of-trade part of the disagreement. The company's motives for not appealing have never been established. A competent scholar would not place so much trust in the results of an adversarial court proceeding.

Mackintosh makes much of what "might have been" had World War II not interrupted Atanasoff and Clifford E. Berry; he accuses J. Presper Eckert and Mauchly of "brilliantly exploiting" the same war to their own advantage, as if they had somehow underhandedly ripped the prize from Atanasoff's hands. Of couse such was not the case at all.

Indeed, no one denies that Atanasoff and Berry were working on their computer at the same time that Mauchly and Eckert's ideas were germinating. No one denies that Mauchly and Atanasoff, once they had met, had tremendous common interests, or that they discussed those common interests. But to make the leap from there to the statement that Mauchly and Eckert "inherited the basic ideas of electronic digital computing from Atanasoff" is an outrageous misstatement of fact, in that it glosses over Mauchly's original thinking and totally ignores the massive contributions of Eckert in making ENIAC work.

In the 25 years that passed between the completion of ENIAC and Larson's invalidation of the patent on it, there was plenty of time for the record to become muddled by academic infighting as well. Mackintosh relies heavily upon the work of Arthur Burks and Burks's wife, Alice, to bolster his point of view. Yet he does not acknowledge the bitter feelings that developed among members of the ENIAC–EDVAC team during Eckert and Mauchly's last days at the University of Pennsylvania, and in the years to follow.

In fact, Arthur Burks was one of three members of the original ENIAC team who sued at the time of the ENIAC patent trial to be recognized as coinventors of ENIAC. Larson turned him and the others down. Mackintosh does not cite this disturbing conflict of interest underlying the Burkses' subsequently published history, whose objectivity may well be questionable. It seems strange, too, that in the intervening years between the completion of ENIAC and the Honeywell–Sperry trial, Atanasoff made no charges that his ideas had been pirated, especially since Atanasoff saw ENIAC in October 1945, as the court record showed.

Physicists are not "surprisingly ignorant" of the "fact" that Atanasoff invented the first electronic computer. They have heard that line of

reasoning and discounted it. They have read of Larson's decision and been unable to find any logic in it. The ENIAC patent contained 148 claims. Honeywell challenged 17 of them on the basis of Atanasoff's work, but Larson found that Honeywell failed to prove its case in 16 of those 17 challenges. Larson also declared Eckert and Mauchly "sole inventors" of ENIAC, but then ruled the patent invalid because of "prior art" in the Atanasoff–Berry Computer. One does not need to be a scientist to see the huge contradictions in Larson's findings.

Here at Ursinus College we proudly display early prototype components of what was to become ENIAC, built by Mauchly during the years that he taught physics here (1933–41). His visit to Atanasoff occurred in the summer of 1941, after he had taught his last class at Ursinus. He was quoted later as saying Atanasoff had nothing that worked and that the trip to Ames was "a disappointment," because the ABC was not fully electronic. The speed of its electronic components was lost to the much slower speed of its mechanical rotating drums.

The March 1985 issue of the *Ursinus College Bulletin* gives a more complete picture of Mauchly's early work here.

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I was amused when I read in Allan R. Mackintosh's excellent article "The First Electronic Computer" that "this superbly important machine was invented not by an engineer . . . but by a theorist." It is later pointed out, however, that the inventor, John Vincent Atanasoff, "took his BS in electrical engineering at the University of Florida in 1925." And he continued his education to receive his MS in mathematics and his PhD in physics. Since when does one undo being an engineer by furthering his education?

In the best tradition of engineering this is clearly an example of the outstanding application of science to solve an problem!

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A foreign visitor to my lab, on seeing Allan Mackintosh's article on John V. Atanasoff's invention of the modern computer, asked, "Why has this invention, which is one of the few most important of the 20th century, not now been recognized by a Nobel Prize?" I thought, and thought, and have no good answer. Perhaps Mackintosh could reply?

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MACKINTOSH REPLIES: Evan Snyder raises, and indeed confuses, two questions. First: Did John V. Atanasoff invent the electronic digital computer? The prototype of 1939 and the Atanasoff–Berry Computer of 1942 were electronic, operated digitally and were indubitably computers. Nobody had made such a device before. By any reasonable interpretation of the words, Atanasoff was therefore the inventor of the electronic digital computer. I do not believe that there can be much controversy about this conclusion anymore. Rather, the argument is about the second question: How much influence did Atanasoff's work have on ENIAC? This is more difficult, but the patent trial elucidated the matter to an extent that is rarely attained in priority disputes. It was established that John W. Mauchly spent several days examining and discussing the ABC (not the few hours that he first claimed) and read Atanasoff's historic 1940 article, which presented an amazingly large proportion of the principles that underlie modern electronic computing.

Let us imagine that a colleague of Albert Einstein had read and discussed drafts of his 1905 papers, which had then remained unpublished for many years because Einstein had been run over by the proverbial Bern tram car. Such a person would have had great difficulty in later maintaining that he was the creator of special relativity. It was in this sense, which is recognized by all who understand the role of ideas in invention, that Mauchly and J. Presper Eckert "inherited the basic ideas of digital computing from Atanasoff." Mauchly described the work he had carried out before visiting Atanasoff in Ames with the words "My own computing devices use a different principle, more likely to fit small computing jobs." I agree with him. The irrelevance of this work to the design of ENIAC is documented in Arthur and Alice Burks's book *The First Electronic Computer* (U. of Michigan P., Ann Arbor, to be published in 1988), which I strongly recommend to anyone who is interested in this matter. (Incidentally, I find Snyder's attempts to impugn the scholarly objectivity of the Burkses distasteful; he would surely do better to limit his arguments to the facts and their interpretation.)

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My sentence "The war . . . created the opportunity that Mauchly and Eckert exploited so brilliantly" is both clearly correct and, equally clearly, was meant to be complimentary. Several times in my article I describe the creation of ENIAC, the world's first general purpose electronic computer, as a great feat of invention. Unfortunately advocates like Snyder tend to turn attention away from the true contribution of Mauchly and Eckert, leaving behind only an image of two men who attempted to claim credit for the achievement of a third, to whom they owed much of their fame and fortune.

C. Burke Swan has perhaps caught me in a small act of physicist chauvinism. Atanasoff's training as an electrical engineer was unquestionably essential to him in constructing the ABC, and in particular in what I regard as his greatest feat, the design of the first electronic logic circuits. However, the motivation for his invention and the originality of his approach both had their origins in his background in basic physics research. This is what he meant when he stated that "theoretical physics is a uniquely effective discipline" for training inventors.

Atanasoff's achievement has only recently become appreciated, and although he has received many honors, many others have passed him by. However, he is now generally recognized as the inventor of the electronic digital computer and I suspect that he would agree with me that this is honor enough for any one man.

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ARTHUR AND ALICE BURKS REPLY: Evan S. Snyder's criticisms address primarily Allan R. Mackintosh's reliance on Judge Earl Larson's decision in the ENIAC patent trial and on our 1981 article about the ENIAC.[1] But Snyder seems not even to have read that decision carefully, let alone the trial proceedings on which it was based. And his objections to our article are to *us*, not to our arguments, so that we have to wonder whether he read it, either! He finds our objectivity "questionable" because of Arthur's presumed "bitter feelings" over lack of recognition for his contributions to ENIAC. He even faults Mackintosh for not acknowledging that nonexistent bitterness!

Arthur, Kite Sharpless and Robert Shaw did informally petition the judge—they *did not* sue—for recognition as coinventors of ENIAC. But they never received a direct response. Honeywell had charged earlier not only that ENIAC was derived from Atanasoff's work but that it was a joint team effort, and that these three and others were coinventors. It was the formal Honeywell charge that Larson addressed in his trial decision, though of course his ruling effectively dispensed with the Burks–Sharpless–Shaw petition.

In section 4 of that decision,[2] Larson recognized the "group or team effort" and the "inventive contributions made by Sharpless, Burks, Shaw and others." He denied coinvention, however, for two reasons. First, Honeywell had not cited specific patent claims attributable to the other team members, as the law required. Second, too much time had passed before the claims of these others were put forward. Of course, Larson also found the ENIAC patent invalid, because of derivation from Atanasoff—whose contributions those three had not known of—so that their names would have been affixed to a worthless patent.

As to bitterness, we would observe that it is quite possible to realize one has been deprived of due recognition, even for an invention thought to be the first electronic computer, without becoming bitter, and in fact to retain respect for the achievements of the depriving parties. The only ill feeling from the ENIAC–EDVAC projects that we know of was between J. Presper Eckert and John W. Mauchly, on the one hand, and John von Neumann and Herman H. Goldstine, on the other, over the conception of the stored-program computer. We explained Arthur's interest in the ENIAC patent, along with his involvement in the design of that computer, in our ENIAC article. We also had high praise for the work of both Eckert and Mauchly.

But the main point we wish to make is that no one has ever answered the arguments we developed in that article for Larson's decision on Atanasoff. On the basis of correspondence presented in that trial and of Arthur's familiarity with ENIAC, we showed that Mauchly and Eckert derived from Atanasoff the general technology of using vacuum tubes for electronic digital computing, and moreover, that they derived from him the very idea of applying this technology to an electronic computer to do digitally what the mechanical differential analyzer did analogically.

Mauchly's widow did, in 1984, publish an article giving her own version of events, with no attempt to answer us.[3] In it she shifted blame for the outcome of the trial to the Sperry lawyers, a stance Snyder now also adopts. She also presented several devices not—in her view—adequately exploited at trial, which are presumably the self-same "early prototype components of what was to become ENIAC" that Snyder's college now proudly displays. These were actually examined thoroughly in court, and Mauchly himself claimed no such connection.

As for Snyder's treatment of Larson and his decision, we feel that he and others in the academic community have been grossly unjust. Our own study of the exhibits, testimony and arguments has shown Larson to be an able and fair jurist. His decision in a very complex case seems to us well argued, complete, prudent and correct, and we find it significant that it was not appealed. We deplore the fact that those who do not accept the decision have not consulted and dealt with the vast store of information generated by the trial.

Space does not permit a fully reply to all of Snyder's erroneous statements. Let us just answer one rather common one and counter two others briefly. Larson has often been called illogical for declaring Eckert and Mauchly the "sole inventors" of ENIAC and "then" ruling their patent invalid because of "prior art" in the ABC. In actuality, the sole-inventors finding (finding 4) *followed* the derivation-from-Atanasoff finding (finding 3) and addressed possible *co*inventors at the Moore School (see above), not *prior* inventors such as Atanasoff. The judge even, within that same finding, referred explicitly to his earlier ruling on Atanasoff as still in force.

Snyder refers to 17 claims "Honeywell challenged"; these were really claims *Sperry* offered as representative of Honeywell's infringement of the ENIAC patent! And as to Atanasoff's failure to come forward earlier, he had no way of knowing that some of the ENIAC patent's basic claims reflected his work until that patent was issued in 1964. The other side of this coin is, of course, that if Eckert and Mauchly had limited their patent claims to what the Moore School team had achieved beyond Atanasoff's work—which was indeed considerable—he would never have become an issue.

The interested reader can find more on Atanasoff and his computer in our new book *The First Electronic Computer* (U. of Michigan P., Ann Arbor, to be published in early 1988).

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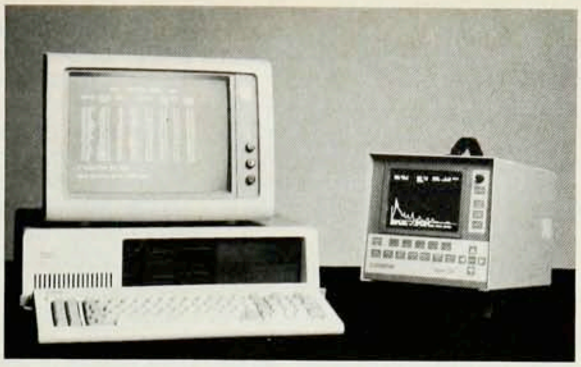

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Quarks Realistic and Naive

I enjoyed the lucid article by David Gross on asymptotic freedom (January 1987, page 39). It contains, however, one point that is not historically true, and because a sizable fraction of the article is a historical account, I believe that point should be corrected.

Referring to the birth of the parton model around 1969, Gross states: "From then on I was convinced of the reality of quarks, not just as the mnemonic devices for summarizing hadron symmetries that they were then universally regarded to be, but as physical pointlike constituents of the nucleon." The statement "they were then universally regarded to be" is incorrect.

What will be the best description of the (colored) quarks in 10 or 20 years is hard to say. But it is certain that since 1965 the most productive description has been a realistic one. It is widely known that in early 1965 I and a number of theorists started to work and calculate with a realistic nonrelativistic quark model[1,2] (later also called the naive quark model). (References can be found in the book and reprint collection by J. Kokkedee[3] and in the rapporteur talks by Richard H. Dalitz[4] in Berkeley (1966) and by myself[5] in Vienna (1968); compare also reference 6 and, for a general historical survey, chapter 4 of the book by Andrew Pickering.[7])

There is no doubt that the realistic description of quarks dates back to that time; indeed, immediately after having proposed the realistic nonrelativistic quark model,[1] I was so struck by the agreement of many of its predictions (magnetic moments, $V \rightarrow P\gamma$ transitions, E2 $\Delta \rightarrow P\gamma$ forbiddenness, reproduction of the Cabibbo theory of the semileptonic decays with a good F/D ratio, spectra of the levels of mesons and baryons, and so on) with the facts that I started a long experiment on the search for real free quarks (see reference 8 for a survey).

It must be added that in the period from 1965 to 1970 there was in fact a split between physicists doing current algebra, who mostly treated quarks as mathematical objects, and those working with the realistic quark model. That Gross belonged to the first school of thought does not imply that the second did not exist. Clearly at that time the current algebraists were more powerful: When I was invited to give a rapporteur talk in Vienna in 1968 on the nonrelativistic quark model, somehow at the last minute the title of my talk in the program was changed into "Current Algebra" (the first few lines of my report[5] mention this "accident"). However, even Murray Gell-Mann—who certainly did not like a realistic interpretation of his quarks—did not ignore (on the contrary, he tried to explain) the success of the realistic model. In a paper with Roger Dashen,[9] referring to the possibility of constructing some unitary transformation in the frame of current algebra, he wrote: "Apart from the interpretation, the irrelevance of the existence of real quarks, and the crucial operator *U*, this case represents the concrete quark model of R. H. Dalitz (*Proc. of the Oxford Conf. 1965*) and G. Morpurgo (*Physics, N. Y.* 2, 95, 1965)."

To summarize: The view that quarks were only mnemonic devices to deal with hadron symmetries was certainly not a "universal" view long before the success of the parton model. Indeed, one can see the parton model—the success of which convinced Gross of the reality of quarks—as a confirmation of this "realistic" interpretation; it provided, in fact, a demonstration that the three constitutent quarks in a proton should (as stated in references 1 and 5) be thought of as dressed quarks each composed of a cloud of bare (point) quarks.

It must be said that Gross is not alone in this misrepresentation of the evolution of the subject. Indeed, many physicists, probably working in more formal areas, took note of the "reality" of quarks later than Gross, who arrived at it via the parton model; they realized it either after the so-called November 1974 revolution and the use of the nonrelativistic model to understand charmonium, or after the paper by Alvaro De Rujula, Howard Georgi and Sheldon Glashow,[10] which used a QCD-"inspired" potential between quarks, and the subsequent applications of the QCD-inspired description by Nathan Isgur and Gabriel Karl (though I feel that the (needed) "inspiration" of the second- and higher-order QCD implies a direct link with the Lord).

Before concluding let me add a comment on Gross's very optimistic attitude toward QCD as the compelling theory of the strong interactions. Maybe he is right, but I do not share fully his faith. What disturbs me very much is the question of the long-range van der Waals forces between nucleons. The absence of these forces can be explained if the gluons somehow get a mass, perhaps through spontaneous symmetry breaking (a point that Gross mentions in his article, but not in this connection). But the original QCD is then altered profoundly, and such alterations have so far not been clarified sufficiently.

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GROSS REPLIES: I agree with Giacomo Morpurgo that I was too sweeping in my characterization of the universality of the opinion, in the mid-1960s, that quarks were to be regarded as mnemonic devices. There is no doubt, however, that this view of how quarks were to be used was very widely held. It was best summarized in the recipe made famous by Murray Gell-Mann: A piece of pheasant meat is cooked between two slices of veal, which are then discarded.

The trouble that I and others had with the nonrelativistic quark model was the apparent inconsistencies contained within it—a nonrelativistic treatment of quarks that had to be massless to yield approximate chiral symmetry, potentials that couldn't be derived from any known field theory, and confinement imposed as a boundary condition. It was only much later (with the development of the bag model, the perturbative treatment of relativistic potential theory and, recently, the skyrmion model of nu-