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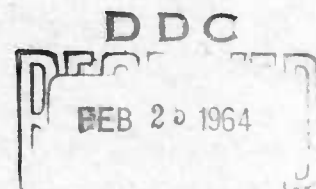
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BIGGER COMPUTERS AND BETTER MATHEMATICIANS

Richard Bellman

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# BIGGER COMPUTERS AND BETTER MATHEMATICIANS

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According to Lefschetz, Einstein constantly complained that mathematics was basically simple; it was the mathematicians who made it seem difficult.  I think that most mathematicians would agree with this dictum, which is certainly well supported by the history of the subject.  Occasionally, we hear a related comment, "Mathematicians need bigger and faster computers to solve by brute force problems which they are not bright enough to solve otherwise."

Most mathematicians would disagree with this comment, which has two defects.  First of all, it has enough truth in it to appeal to prejudice seeking rationalization; secondly, it omits any discussion of why mathematicians want to solve some problems by brute force, and what they expect to do with the solutions obtained in this fashion.

To begin with, let us point out that certain current problems possess a high priority in the sense that any type of solution now is far preferable to the most elegant analytic solution ten years from now. Into this category fall a large number of problems from the engineering, economic and military spheres. Indeed, the rapid change in technology would make solutions ten years from now of no interest whatsoever in many of the most important situations. Furthermore, in the biomedical area, it is difficult to calculate what the present worth of an efficient drug—screening technique is, as compared to an optimal technique ten years from now.

I do not believe that anyone should be ashamed of, or con—temptuous of, brute force solutions to problems of the type indicated above. Actually, as far as most significant problems are concerned, we require all of our mathematical sophistication and ingenuity even to apply what the uninitiated might call "brute force" methods at the present time.

The principal point is that bigger and faster computers permit those of us somewhere between the ape and da Vinci to treat signi—ficant questions now.

From the purely theoretical point of view, it should be remembered that the origins of mathematics are in experiment. Who can count how many triangles and circles were drawn in the sand before the time of Euclid, or how many beads and apples and

ranges were arranged and rearranged before even elementary number theory arose. The prime number theorem was discovered by Gauss and Legendre by means of an examination of numerical tables; Euler regularly used computers (people—computers that is) to construct tables of arithmetical functions; Artin conjectured the Riemann hypothesis for finite fields on the basis of an examination of several hundred cases; and so on and so on.

The present emphasis, and certainly overemphasis, upon axiomatics and armchair mathematics has obscured these well—known facts. In entering new domains of mathematics and science, we need specific examples before our eyes. First Brahe, then Kepler, then Newton, that is the order of scientific progression. Gauss understood this perfectly, and so did Wiener and von Neumann, two of the most important pioneers in the development of digital computers.

In entering new fields, such as turbulence, or adaptive control, or the many body problem, or scheduling, it is essential to solve as many particular problems as possible in the hope that patterns and solution and structure will emerge. Following initial clues, we solve further problems and so, hopefully and at least historically, converge to a theory.

Once upon a time, this preliminary problem—solving could be done with pencil and paper, or with a few hired hands. This is no

longer the case. Eventually, when we fully understand some of the phenomena we are investigating, we will know what simplifying approximations to make so that we can easily and rapidly perform the calculations leading to a solution. At the present, we are struggling to understand many different types of scientific phenomena never previously studied, or even conceived of as worthy of study.

As luck would have it, the digital computers currently available are about one order of magnitude in speed and rapid access storage below what we could use to tackle vast classes of realistic and signi—ficant processes. Many examples of this can be given. Let me merely refer to some of my own work for discussions of these questions, [1], [2], [3], [4].

What is rather sad to contemplate is that the computers we have now have certainly not been used in any scientifically efficient or idealistic fashion. Rather clearly, the use of the computer to make out telephone bills illustrates "Mons laboravit et mus parturit." For several years, it has been possible at a relatively small cost, say 3,000,000 dollars per computer, to soup—up the rapid access storage of an IBM 7090 from 32,000 to 1,000,000. Due to the shortsightedness and lack of scientific perception of those responsible for computer technology, this has not been done, and even now there is no comprehension of what an increase of this type means to the mathematician and the scientist.

Five years ago, the government should have initiated research into the problems of constructing digital computers 100 to 1000 times as fast and 1000 to 1000000 times as large as far as rapid access storage is concerned. It is absolutely absurd in a world in which science and technology are obviously of major importance to allow the development of this fundamental scientific tool, the digital computer, to be left to the whims, chances, and tax write—off of commercial computer companies.

We can hope that the situation will improve. Unfortunately, reading what is written, listening to what is said, I get the impression that the situation will not for quite awhile. Those most influential in the field of computer technology seem distracted by gimmickry, overimpressed by the glamour of "intelligent machines," and, generally, little informed in the needs and objectives of mathematics and science—and less sympathetic.

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