

64-1047

EXECUTIVE OFFICE OF THE PRESIDENT  
OFFICE OF SCIENCE AND TECHNOLOGY  
WASHINGTON 20505

64-0105/1

February 24, 1966

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Dear General Carter:

This is in reply to your letter and enclosure of October 17, 1963, requesting our views on what might be done to resolve the problems in non-numerical automatic data processing. You will appreciate that I have only recently involved myself in these questions so that this reply is in a certain sense preliminary. The questions raised hit at the very heart of the general problem in human communications. They are questions which have concerned us for some time and for which we would very much like to have answers.

We have a very strong interest in the communication of scientific and technical information and also in the techniques of command and control with its involvement with intelligence information. Your letter points out the universality of the tools and methodology of communications in your intimation that research in non-numerical information processing in any one field can be beneficial to communications broadly. We certainly agree with you.

Your letter points to three problem areas which you believe to be critical to the development of non-numerical processing and upon which extensive efforts have been focused. These are listed as follows:

- (1) The conversion of text on hard copy into text which can be read by machines;
- (2) The development of massive memories to which access may be had at random; and
- (3) Devising advanced techniques for processing information.

While these three problem areas describe the present programs of support in these fields, I might have used a different breakdown of

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the problems especially as one would plan them for future programs. Items (1) and (2) appear to reflect the earlier plans that were intended to engineer toward the solution by brute force. Our early efforts to develop advanced techniques appeared to have been guided by the desire to exploit the wonders of the newly arrived ADP equipment and electronic digital computers, hoping for the quantum step to fully automated systems which would accept and manipulate the massive information input.

The bulk of the government funds to improve the handling of information was funneled into the development of large systems which were, in all honesty, a rationalization of preconceived solutions. Inadvertently, and instinctively, these focused efforts were directed toward the simulation of the human brain, complete with massive memories, character recognition and Gestalt retrieval. Admittedly, this was a gamble, but it suffered as most focused research efforts suffer, in that its success depended upon the ability to resolve some fundamental obstacles. Unfortunately, subtle obstacles proved to be more formidable than originally anticipated.

I somehow sense that there is a widespread disenchantment with the large systems approach since the results of nearly ten years of effort and nearly \$100 million of expenditures have evidently fallen far short of the earlier expectations for them. As a consequence, there may in all likelihood be a widespread inclination not to do further work in the development of advanced techniques, such as non-numerical information processing, i. e., item (3) of your letter. I would hope that whatever disposition one makes with your items (1) and (2), it should not hinder the government's efforts in item (3).

I would advise in favor of stepped-up research in exotic non-numerical information processing but would recommend a re-evaluation of the goals, their redefinition, and the expectation of achieving them. I would also recommend that new support be given to research in the present information handling systems with the goal of achieving incremental improvements.

Considerable progress has been made in the methodology and tools for handling files, paper work, and other recorded information over the past several years, but these incremental improvements to operating systems resulted more from industry-sponsored than government-sponsored research and development. Even so, the government has

greatly benefited by the development of new schemes and equipment for filing, manual retrieval, photocopying, high speed composing, printing, etc.

If we rule out the large systems approach as being imprudent at this time, then there remain three possible avenues for the government efforts in non-numerical information processes:

- (1) The enhancement of present manual information systems with improved apparatus and methodology;
- (2) The fundamental studies of the system parameters; and
- (3) Research directed toward a system breakthrough.

I am unable to suggest at this time how one ought to apportion the government funding into these three avenues. The availability of talent should be the factor which determines this allocation of funds. If a retrenchment of the large systems is called for, then there would be more than sufficient funds available for these recommended efforts. This is essentially the sense that was embodied in a 1961 report by a PSAC ad hoc study panel on non-numerical information processing. Two excerpts from this report are as follows:

"It is therefore recommended that no more money be expended than that presently budgeted and planned, except in response to specific well-thought-out proposals embodying distinctly new theoretical or conceptual approaches. If anything, smaller expenditures on ill-defined global development projects could free some manpower for thinking."

\* \* \*

"The panel thinks that this is not a question of facilities, money, massive group efforts, or large research centers, but rather a question of properly motivating the present widely scattered individual workers and groups of workers to attack a tractable set of problems."

Perhaps we in the government are giving too little effort to our present systems for information processing -- the traditional manual systems

of reporting, indexing, filing, and classification. It is apparent that our efforts have failed for lack of a more fundamental understanding of human communications. The recently completed data base under the SCIPS effort of CODIB should contribute immeasurably to the verification of these fundamental studies mentioned above.

The present level of expenditure in this whole area of information processing, although it may be disproportionately allocated, is probably sufficient to cover a well balanced effort, including research and development for incremental improvements of present methodology research into advanced concepts. But a new balanced program most decidedly would involve a stepped-up funding in theoretical studies and in studies which attack the fundamental problems.

Finally, I would like to address myself to the matter of manpower for carrying out this work. While most of us have frequently heard the comment that there is a dire shortage of qualified talent to undertake research in non-numerical data processing, we probably can no longer accept this as meaningful in the light of our proposed expansion into the areas of more fundamental research. In the past and at present, much talent has been frozen in large systems efforts. Here, the individuals are part of an organizational structure which may indeed impose a tightening of creative talent within the relatively few corporations and laboratories which have large contracts. The so-called shortage of people may be in part due to the rigid structuring of systems efforts itself. I presume that much talent is sapped away from the primary problems by being slotted into the purely systems problems, i. e., such work as impedance matching among the components of the system. Thus, I believe the shortage of talent has in part been generated by our method of funding.

One of the strongest recommendations I could make at this time would be in the nature of changing the method of support for work in these areas. It seems to me that the only way to marshal the intellectual effort needed is to establish a broadly based grant mechanism to individuals or to small teams or groups of researchers. Concurrently, the retrenchment of support for the larger systems hopefully will release scores of top quality talent which, coupled with the expansion of support into virgin manpower pools, should provide more than adequate talent for the task. Admittedly, this will increase the problems of security, but these problems as you intimate in your letter,

are larger than intelligence information itself. Thus, it may be possible to provide support for unclassified work which would deal with problems in scientific and technical information or in other modes of communication, yet in fact the results would be indistinguishable from research and development in intelligence information processing.

In summary, your letter is welcomed as an invitation for all of us to examine the intra-government communications process as the science it is.

Concern for the problem of non-numerical information processing has been felt also in other quarters, especially the Department of Defense. With this rising interest it may be an opportune time to establish (1) a government-wide program in communications processing under the aegis of the Federal Council for Science and Technology; and (2) a non-government advisory activity in communications research and development under the aegis of PSAC.

While these are possible answers to promote coordination of similar efforts among the Departments or Agencies, they are mentioned merely as possible approaches to a group of problems. I would welcome your reaction to them and perhaps your suggestions of other possible mechanisms.

Sincerely yours,

Donald F. Hornig

Lt. Gen. Marshall S. Carter, USA  
Deputy Director, Central Intelligence Agency  
Washington, D.C. 20505

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MEMORANDUM FOR: *DDI*

*President's Science  
Advisor responds to  
a letter you wrote  
to Weisner last September  
about automatic data  
processing and computer  
machines.*

*Now that we've hatched it, can't  
let it die -* *WKE*  
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