communications are too restricting and stultifying. Studies of time sharing vs batch processing have tended to find time sharing more popular but less efficient (Sachman, 1970). One of the most fascinating results (Seven, Boehm, & Watson, 1971) concerns the beneficial effect of an enforced time-out period after each computer request in an interactive problem-solving situation. In short, people sitting at the console feel constrained to use it, so they type things without enough prior thought, perhaps to give the impression that they are working. How to change this pattern of behavior is worth more study.

COSTS

One difficulty with the time-shared console is its cost. At minimum it will cost \$1000 per year and, if it is popular, perhaps as much as \$3000 per year. This is still substantially below the cost of amortizing and supporting an in-house laboratory computer, but it is not cheap. One of the crosses time sharing has had to bear is the per-hour charge. No other psychological equipment is rented by the hour; students and professors alike should try to avoid thinking of the timed charges, though of course they should be careful.

Will programs written for the time-shared console transfer easily to a laboratory computer? Yes. Most minicomputers provide BASIC; the others provide a close cousin such as FOCAL or TELCOMP. Thus the laboratory computer will typically be able to run the programs with little change.

My purpose in displaying several sample programs has been to show how very simple and straightforward the programs are and, thus, to imply that they are not difficult to learn how to write. There is an added implication that the computer permits much more intricate experiments than have been the fashion in the human experimental laboratory.

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Misuse, abuse, overuse, and unuse of on-line computer facilities by psychologists

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When Dr. Tepas asked me to discuss some of the negative aspects of the psychological use of on-line computers, my first thought was, "Oh boy, here's a chance to view with concern and point with alarm at what must be a morass of one or another sort of computer application evil." After a little thought, though, it appeared to me that this was not exactly the case and such a perspective could not be entirely accurate.

In the last 7 or 8 years since the announcement of the first computers of a size that made possible almost personal facilities, there has been a flowering of installations, applications, and I think, in the main, wholesome productivity. Computers now cost inconsequential amounts compared to the other expenses of running any sort of a laboratory operation, and thus the economic issues are no longer of primary concern. The most important aspect of laboratory computers is still their ability to open new theoretical and empirical doors and to provide us with new techniques for the analysis of problems which could not possibly have been examined previously. I hope it is not too trite to repeat the oft-quoted analogy that the computer is as important to psychology as the microscope was to biology. Of course, we will have to wait a few decades or longer to see if what seem to be the likely long-range contributions of this instrument do develop.

As I look around now, I see that there are many, many on-line computer installations. I know of a number of individual psychologists who are simultaneously using two or even three small computers in a productive fashion. The many positive reasons for this increasingly important

contribution of computers to experimental psychology are clear. The other two speakers this morning have spoken to these points, and I shall not reiterate the argument for computerization. I would like to note that the fact that laboratory control computers are so ubiquitous is itself an argument of considerable consequence for their general utility and importance. Unfortunately, there are also instances of unfulfilled needs that seem so obvious among some of our colleagues that they must be acknowledged. I am convinced that there are many more of my colleagues in need of computer assistance in their laboratories than there are those who are misusing them.

Before I start with my assigned task of a broad critique, I do want to make it clear that I believe that the positive side of on-line computer applications does very much outweigh the negative aspect. But my assigned role today is that of the critic, and I must to some degree view with alarm and point with concern; as usual, it is always possible to do so. Some issues I will raise will be of considerable importance. Others will seem, to some at least, to be of only minimum consequence. All, I hasten to warn you, are based upon my personal perspectives and prejudices, and I am sure there are reasonable men among us who will find ample and perhaps even valid reason to differ with me on certain points. If there is any theme which I can see to all of these comments, it is that they will be concerned mostly with strategies and global approaches to the use of control computers rather than to any specific details of their use.

THERE IS NO "BEST" COMPUTER!

First, by way of preparation and to at least partially define my perspective, I should note that I have a general philosophy about the computer hardware available today. It is true, I am quite convinced, that almost any one of the minicomputers available from the 50 or more manufacturers in this field is capable of serving the needs of almost all psychological research. Even the smallest machine can have sufficient memory and adequate input/output devices to serve the needs of almost any form of psychological research one could imagine. Even those simulators of psychological and biological processes who have for so long been calling for more core storage (like Helmholtz did for "more light") have somehow never seemed to benefit as they once thought they would now that the enormous high-speed storage units are widely available. I suppose one could imagine some sort of problem that could exceed the core capacity of a small computer, but frankly, I think it is patently impossible to imagine any ordinary psychological experiment that could not be run on even the smallest commercially available computer. The submicrosecond time domain in which these machines operate is at least several orders of magnitude below the finest psychophysical or learning time constant or even the fastest reaction times one could conceivably encounter.

No one manufacturer, therefore, has anything special to offer to our scientific community in hardware, either in terms of capability or reliability. It is probably more important nowadays to base one's purchase of a new system on the availability of some special peripheral devices or of convenient service rather than on any complex analysis of the details of the computer hardware. Some manufacturers have more convenient interfacing capability or a special program or a convenient service, but these factors—secondary though they may at first seem—now, I believe, are actually the most important criteria for the choice of a laboratory computer.

IF YOU HAVE A COMPUTER, DON'T BE A "DEGENERATE" COMPUTER USER—USE ALL OF IT!

But now let me turn more specifically to my assigned task and discuss what I believe to be some of the misuses, abuses, overuses, and unuses of computers. Although the general purpose and productivity of computer application has been good,

there are some points susceptible to a contemporary second look which I will try to outline briefly.

Perhaps the best place to start is with what, in another context (Uttal, 1968)—that of computer teaching machines—I have referred to as the "degenerate" use of computers but what I would like to refer to here simply as computer unuse. It is still striking to realize that there are many professionals working with computers who still apparently do not understand or appreciate the essential nature of a computer and the special properties which distinguish it from any other automatic machine. The essential property is still pretty much the same as when Burks, Goldstone, and von Neumann (1947) first formalized it, namely, the quality of contingency. The computer alone among all the products of our cybernetic technology is capable not only of performing a sequence of instructions on a set of data, but also of modifying the sequence of those instructions contingent upon the outcome of the calculations they solve. There are many other automatic machines and computer-like devices, but, unless they have this contingent capability in some way built in, they are not what I believe should be classified as "computers." Address modification and, for that matter, instruction modification, as well as sequential branching, are functional characteristics that arise out of this essential property of contingency, but the basic idea is contingency. In spite of this basic or fundamental nature, we often see experiments designed and implemented which do not adequately utilize the contingent capability offered by the computer, even when it is badly needed. Though computers might well be electrically involved in the hardware, the mere presence of a computer is not tantamount to the true computerization of an experiment. Often linear and noncontingent experimental paradigms are used, and stimulus patterns are generated by manual means when they could much easier be algorithmically generated by the computer.

There is, of course, no a priori reason why every experiment should fully take advantage of all of the computer's capabilities, but it is clear that the unuse of some of the powers of the computer when they are truly needed may lead to a high degree of inefficiency, on the one hand, and—more seriously—a rigid adherence to old perspectives and theories, on the other.

DON'T TRY TO BUILD YOUR OWN COMPUTER!

Closely related to this sort of

computer unuse, in which a computer is actually physically present, is "computerization" without the physical presence of a "computer." In this cryptic comment I am referring to the still-prevalent tendency, particularly on the part of some of my colleagues, to build—either from raw components or from functional modules—devices sometimes euphemistically referred to as "special-purpose computers." In doing so, they often exceed the purchase cost of a full-scale computer and yet manage to provide nowhere near the information-processing power found in even the very smallest minicomputer. I believe the phrase "special-purpose computer" is a misnomer and, as such, is fundamentally misleading these psychologists-cum-engineers into believing that they really have built something that comes close to the information-processing capability of a real computer. The usual final situation is that they have not, in fact, made a special-purpose computer but rather have constructed a special-purpose "noncomputer" or automatic control device with little, if any, of the branching, adaptive, or contingent capabilities of the real thing. It turns out, in so many instances, that the devices so constructed have been completed at terrible—although often admittedly invisible—costs, but with time delays which are all too awesomely visible. Typically, these devices are capable of controlling stimuli according to a predetermined sequence and of acquiring the response data which, if it is at all machine readable, is in the rawest possible form. It is more likely to be a printout not translatable into a format suitable for a subsequent analysis. The true computer, quite to the contrary, not only performs all of these functions and opens the door to contingent experimental design, but also is capable of preprocessing the information so that it is in an instantly assimilable form by the investigator.

In the last decade, I have seen many special-purpose control devices that have been built as alternatives to the purchase of small computers, and, in my estimation, it is surprising how often these very expensive monstrosities are either never or only briefly used before being assigned to the scrap heap.

If one considers what is truly "real time," I believe another important fact of computer application emerges. The speeds with which a true computer can be programmed and an experiment implemented and the same job done on a special-purpose computer do not compare favorably. With a moderate degree of luck, a computer should be in operation within a few weeks of

delivery. I have just taken delivery on the sixth computer in my career, and, as usual, it took about 10 days before our first experiment was running. A computer can also be reprogrammed to achieve a completely different experimental objective in a few minutes. But both the time scale of the original construction and of changeover for special-purpose devices usually is discussed in units of years rather than weeks.

One final word of advice to those who would have their psychological laboratories become miniature hardware factories. A couple of years ago, I sincerely thought that a substantial amount of money could be saved by building some of the input and output interface and control equipment in one's own laboratory. Reasonable advice a year or two ago would have been to buy as much central processor and core as one could afford and then to engineer the peripheral gear oneself. The economics of the computer market are such that advice of this sort does not last very long, and I now think just the opposite is true. The present cost situation and a somewhat belated appreciation of what is necessary as a "basic black" package for psychological research on the part of the computer industry are developments which have now completely reversed the substance of this advice. I would think that almost any interfacing on the part of the psychological user is now ill-advised. There is no way that we can do it any cheaper than the manufacturer's selling price, if we honestly include the cost of technician and programmer time and the costs of those inevitable delays between conception and implementation.

The most disappointing examples of unuse, however, are those instances in which some elegant computer equipment has been provided and has been unused in the simplest sense of the word. What psychological drives and blocks underly this sort of behavior I am not prepared to say, but it is quite clear that there have been many instances in which eagerly promoted equipment has simply never been put to use. Fortunately, there is always some eager student waiting for such a situation and a user can metamorphasize into a "host"—to use Sperling's (1971) words—quite to the profit of the community at large.

However, these examples of the unuse of computers are balanced by unfortunate overuses of computers, and it is to this class of application that I would now like to turn.

THE PROBLEM AND NOT THE METHOD SHOULD DETERMINE PRIORITIES!

A computer overuse, as I would

characterize it, is not so much a result of misuse of the computer itself as it is of the treatment accorded to the substantive problem under consideration. Computers have always had an aura about them that is capable of impressing not only the very naive but also sometimes the partially sophisticated. The mere insertion of a computer into a research problem or theoretical analysis, in spite of the technical virtuosity which may be displayed, does not always mean that an appropriate amount of thought has been given to the problem itself. At its most primitive level, of course, are the exercisers of the computer who search mightily for some problem, any problem, which will allow them to play upon their computational organ. The "music" so produced may be very beautiful indeed, as experimental designs are developed and as chords of information processing rise to crescendos of technical virtuosity. But is anything really happening? Is any really substantive point of knowledge being developed or a theory tested or even a "I wonder what would happen if . . ." sort of experiment being done? I suppose you have all seen examples of this sort of research. There is usually no introduction or discussion to the papers reporting the often superabundant findings. The problem area is not clearly defined, much less the specific issue being studied. In its place, the energies of the author are directed toward a sort of mindless and pointless mill grinding, with more and more minute perturbations of some basic paradigm finally culminating in publication of data with little meaning, significance, or even internal compatibility. The mill grinds on and on but produces nothing more than a totally intellectually nonnutritional output.

On a more macroscopic level and somewhat unrelated to the main theme of my assignment, but one which might profitably be considered, is the overkill of computer application to highly complex studies of future social systems. A notion of wide popular currency is that we can, with a greater or lesser degree of success, predict the future by formulating a system of simultaneous equations of a complex system and "simulating" society as a whole. Quite apart from the purely technical problem of whether this task might better be done on an analog rather than a digital computer, there are issues of considerable conceptual import involved in this approach to social problems. Most of these models assume a sort of continuity in society that is hardly supported by history. A basic assumption of all such social-system models is that the results are valid "if current conditions and

trends remain constant." But, of course, current trends and conditions never remain constant, and thus the basic foundation of all of this sort of cultural neoastronomy is pneumatic at best. Energies now spent on these computer simulations might better be spent on exploring the empirical interactions between different dimensions of the society—the basic data so necessary for any inductive theorizing—yet of which its very paucity can be said to disqualify some of these at once super- and pseudo-theoretical programs.

It is somewhat distressing to note that computer applications to the solution of other complex system problems have also been surprisingly ineffective as one reviews the past 20 years. According to the computer-world's resident iconoclast, H. R. J. Grosch (1971), speaking of the general problem of large-scale computer applications to air defense, business management, or even social sciences research: "... projects from the wilder shores, and especially command and control, never have been and never will be successful in a real life sense. . . . [They] will not run a giant business or defend a country." Indeed, recent newspaper reports have suggested that even some of the simple computer-based business projects like credit-card operations, which, although grand in replications, are simple in programming philosophy, are now beginning to be considered questionable business ventures. I cannot resist pointing out that this is a perfectly satisfactory outcome as far as some people are concerned, for the social implications of the unresponsive intertwined credit bureau and credit-card operations may be potentially more serious than some of the superficially more profound—though actually less socially influential—computer models of the entire organic universe.

IS A PROGRAMMER AN UNMIXED BLESSING!

But now let's turn to another aspect of computer abuse. In doing so, we turn to a more immediate level of concern about the interaction between a potential computer-using psychologist and his programmer. A programmer is a member of a slowly emerging social class of considerable importance and completely understandable independence. The problem I wish to discuss revolves around the question of whether or not the psychologist who wishes to use a computer in his experiments should take the time to learn how and then to do his own programming. On the deficit side of the ledger considering this question is the fact that it does take time to learn something new and

it also takes time to do the work oneself. However, I believe that those assets that accrue when one does at least certain of the critical programming parts make it quite clear that overuse of computer programming assistance can be a serious abuse of the E's responsibility toward complete knowledge of the technique, toward the development of confidence in the validity of his results, and toward his making the maximum contribution and the greatest possible advance in the frontiers of our knowledge.

In a computer-controlled experiment, there is an entirely different relationship between the computer and the user than there is in the situation in which data are simply being summarized and analyzed. The number crunching of the statistical analysis is, not only in substance but also in its real-time aspects, qualitatively a different sort of process than the real-time experiment. The logical and electrical nature of the computer itself is of far more fundamental importance to the user in the latter case than in the former.

More important, though, is the possible opening of new perspectives, of new approaches to old problems, and even of the new problems accruing to the user with an intimate understanding of the available programming techniques. This is particularly so when one is programming in the unmediated form of a direct machine or assembly language. It is an interesting experience when one programs the details, for example, of the central stimulus display and response acquisition portions of an experiment to discover the much more highly specific than usual statement of the experimental and control variables required. It is also impressive to see how often doing your own programming can suggest previously unappreciated avenues of experimental exploration of an important substantive problem.

In a very practical sense, it also sometimes becomes painfully obvious how difficult it is to communicate with words to a programming assistant the exact meaning of an experiment. It is easy for even a good programmer to misinterpret a verbal description or to execute a perfectly reasonable, though critically different, implementation of an ambiguous verbal statement. This sort of difficulty does not occur (at least as frequently) when one is directly communicating with the computer.

There is, furthermore, a fantastic amount to be learned about information processing from even the most rudimentary knowledge of

computer programming. It has often been said—and quite fairly—that the tools available to a scientific era determine the perspective and theories as well as the findings. Clearly, the computer is the most important tool to be encountered by experimental psychology in the 100 years of this science's history. Equally clear, however, is the fact that the potential impact on theory and perspective is going to be very small if psychologists develop a tradition of using this "information microscope" only through the mediation of a specialized technician. Perhaps the most important aspect of a new observation instrument is that it opens new vistas. But one has to look at those vistas directly to see what is to be seen. Second-hand tales borne by untrained observers will all too often leave out the revolutionary, though unanticipated, finding.

DON'T TIME-SHARE SMALL COMPUTERS FOR LABORATORY CONTROL!

And now I would like to turn to another issue which has caused me some concern, alarm, and amusement over the last few years. Because of the interest and relatively large amount of success with the time sharing or multiprocessing of large-scale computer installations, some attention has been given to the possibility that it might also be fruitful to time-share small computers for real-time laboratory operations. The rationale for such a proposal goes something like this: "Think of all of the central processor time which is wasted by the computer in a typical experimental operation. The human being is responding with but a bit or two every few seconds and even algorithmic stimulus generation and display takes a relatively small amount of the available CPU time." In fact, this is generally correct, and I would estimate that well over 95% of all CPU time in a typical single-experiment operation is spent in loops waiting for something to happen in the S's world or for some timing loop or real-time clock to signal the end of a delay interval.

At first glance, this seems terribly wasteful, and, if one adds to this the seductive pressures of the success of large-scale time sharing, there does seem to be some substance to the idea of time sharing a relatively small computer for a number of users. But it is only at first glance that it is a plausible argument, and a more detailed examination of the situation raises serious questions.

Over the years I have reviewed a number of proposals to time share small computers, and some of them

are most ingenious. The technique and the instrumentation proposed suggest a high degree of technical competence. However, it seems to me that many of these projects have neglected to consider adequately some important and fundamental issues. First, the sort of time sharing that is done on large computers to serve an entire university community is not a real-time operation in the same sense as the microsecond responsiveness of a fully committed small computer. It is only most rarely that really high-speed reaction times in a large-scale time-sharing system have been provided (I insert the equivocal terms "only" and "rarely" because I am sure that someone can always point to some special case in which it has been possible, but I believe the generality holds). The communication links into a multiprogrammed time-sharing system are typically from teletypewriters and other relatively low-frequency teletypewriter-like communication devices. In spite of the enthusiastic promises of the boosters of even the best time-sharing systems, I believe it is still the case that low-speed communication lines represent the archetypical interface and a second or a couple of seconds represents the archetypical response time. The point I am trying to emphasize is that the supervisory routines and the compromises that have to be made in a large time-sharing system make it virtually impossible for such a system to provide adequate real-time services in the same manner provided by a committed computer, no matter how small. As a limiting case proof of the truth of this assertion, consider an installation with two or more users, each requiring a rapid response simultaneously. While computer speeds have increased, they are still not infinite and time sharing is still a serial and not a parallel process.

Thus, surprisingly, it is the case that the microsecond-interrupt controlled response time one gets from a \$5,000 computer is almost as good as one can get even with a fully committed \$5 million computer. Time sharing a large computer must necessarily give rise to a situation in which there is a reduction in the fast response capability for the individual user. (The only exception to this may be in the case of some of the newer systems which themselves are more closely analogous to a family of small computers than to a single central processor.) In the small machine, with its limited instruction repertoire, the diminishing capability as one time-shares falls off even more quickly than in a large system with automatic paging and relocation hardware. In other words, the overhead is

substantially larger for a small than for a large computer!

So far, this criticism of small-computer time sharing would not be too compelling if, indeed, it were the case that all psychological responses were so infrequent and data rates so low that the time sharing could still handle the job. However, there are two highly important additional arguments which further mitigate such a procedure. With small computers costing as little as they now do, it is, in my opinion, far cheaper to buy a number of separate minicomputers and pass them out to each of the fellows rather casually than to time share. Even if one does waste that 95% of each computer, in the long run it is going to be far cheaper than time sharing.

There are several reasons for this. First, to time-share a small computer means adding additional peripheral equipment that is often necessary only for the time sharing and serves little other function. One recent document I saw suggested an expenditure 20 times the cost of the basic computer solely so that time sharing could be implemented. I am sure that the total power of this system would have been very much less than that available from 20 individual systems. In general, we can be assured that doubling the cost of a computer does not double the power; 10 \$10,000 computers represent a much more substantial amount of computing power than 1 \$100,000 computer, particularly if one's criteria are in terms of rapid response and experimental control, as they mainly are for experimental psychologists.

Second, the programming overhead involved in developing a tailored time-sharing system for a small computer can often be awesomely high. The usual rule of thumb among the programmers of large systems is that programming costs are twice as much on the average as the hardware.

All of this is time, effort, and money which is completely above and beyond the direct-application requirements of the computer. It is simply the resource investment necessary to get the time-shared system to the point where the newly delivered independent machines would have been as soon as they were uncrated.

The other major reason for avoiding small-computer time sharing is purely one of maintaining the small computer's rapid response capability. It just so happens that not all psychological research applications have the low data rates I mentioned earlier. Many are, for at least brief periods, quite vigorous consumers of CPU capabilities. The plotting of a

visual display on a small computer can fully occupy the resources of the computer (even a large one) for a period of a second. This means that, during that period, all other time-shared operations would have to be locked out and would have to wait. Physiological experiments, in particular, are generators of massive amounts of data. A very modest-capacity analog-to-digital converter is capable of swamping the biggest computer imaginable if allowed to run continuously and freely.

Thus, for reasons of both economy and capability, I feel that it is most unwise at this stage to consider computer time sharing as a feasible alternative to multiple independent on-line facilities. I suppose that a case can be made for the simultaneous running of a number of homogeneous low-data-rate experiments such as a group of animal response boxes, but anyone who suggests to a physiological psychologist that he share a central facility rather than acquire his own independent system is doing him a serious disservice. While learning and reaction-time experiments might be time-shared to some degree, I cannot, on the basis of my experience, see how a couple of really interesting perceptual experiments would not quickly exhaust the capability of any sort of time-sharing system.

In addition to these economic and practical considerations, I think I should also add that there is an enormously important argument that can be made for individual computers in terms of the social interactions between the users. Like gases, two investigators can share the same computer space, but each alone is usually also capable of filling the space. The potential for aggravation and abrasive interaction in this particular situation is particularly great and perhaps a more important determinant of the "best" mode of computer utilization than any of the more "objective" measures already mentioned.

"ECONOMY OF SCALE" IS A MYTH!

A related argument often used to induce time sharing is that there is an "economy of scale," i.e., the larger the machine, the cheaper it is to execute a given instruction or program. You have heard it before: "If we all pool our resources," they say, "we can buy a machine which can do more and which will be cheaper on an instruction-by-instruction basis than a small machine." This notion is at the heart of much of the philosophy that supports the trend toward computer centralization in the last decade.

The notion of economy of scale is completely, absolutely, and, with but one specific exception, false—according to a most important paper by Jerome Cox (1969) of the Washington University School of Medicine. That exception concerns the special-purpose circuitry once found solely on larger computers for floating-point arithmetic. Even this equipment, however, is now available for some smaller machines; but, since I am not sure if anyone has pursued Cox's 1969 analysis to include this new development, let us consider only the facts as they existed then. According to Cox: (1) Execution of a single addition costs half as much on a small machine like a PDP-9 as it does on a large machine such as an IBM 360-75. (2) Execution of a subroutine not involving floating-point arithmetic costs about one-seventh as much on the small machine as on the large one. (3) Floating-point-arithmetic algorithms, on the other hand, cost one-ninth as much on the larger machine as on the smaller one.

Clearly, there is no economy of scale; but, even if there were, it would still not be an argument for small-computer time sharing.

CONCLUSION

This, then, is a very personal and informal view of some of the peculiar trends of computer application in experimental psychology. If there is any single point on which I would prefer to end, it would be a reiteration of my belief that, on the whole, the positive attributes and contributions to our science of on-line computers have far outweighed some of these nagging aggravations. Regardless of what has been done and in what way it has been done, it is certainly better to have done something than never to have done anything at all. Our science, like a computer program, is also contingent and self-adapting, and most minor strategic errors are quickly corrected. But no one can debug a computer program or a major technological movement without at least having pushed the startbutton.

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