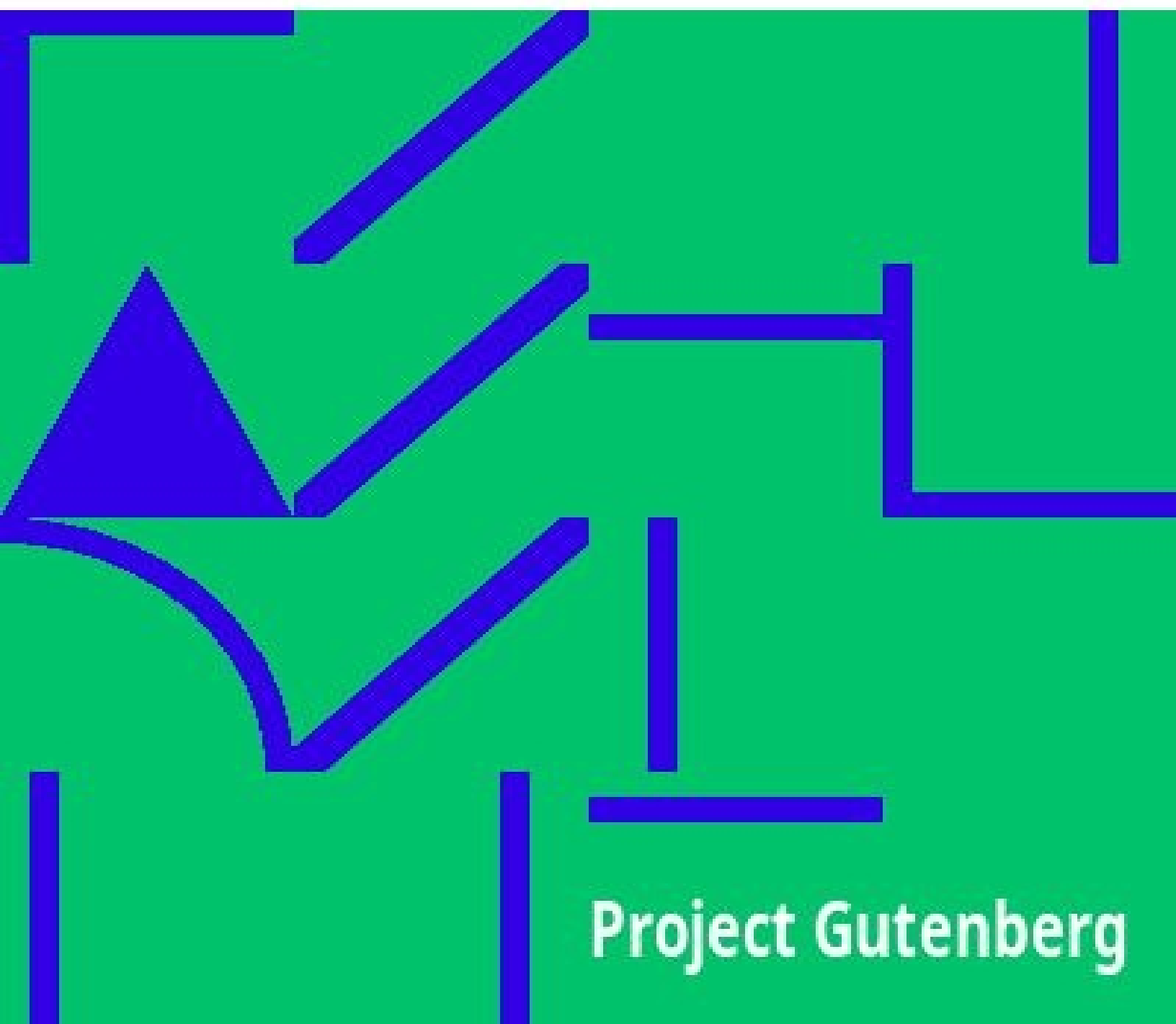


On Handling the Data

M. I. Mayfield



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... ON ... HANDLING THE DATA
BY M. I. MAYFIELD

Illustrated by Freas

Sometimes a story is best told by omission—!

September 16, 1957

Dr. Robert Von Engen, Editor
Journal of the National Academy of Sciences,
Constitution Avenue, N. W.,
Washington, D. C.

Dear Sir:

I am taking the liberty of writing you this letter since I read your published volume, "Logical Control: The Computer vs. Brain" (Silliman Memorial Lecture Series, 1957), with the hope that you can perhaps offer me some advice and also publish this letter in the editorial section. Your mathematical viewpoint on the analysis between computing machines and the living human brain, especially the conclusion that the brain operates in part digitally and in part analogically, using its own statistical language involving selection, conditional transfer orders, branching, and control sequence points, et cetera, makes me feel that only you can offer me some information with logical *arithmetic depth*.

The questions raised in this letter are designed principally to reach the embryonic and juvenile scientists ... the *scientists-elect*, so to speak. (I think the

“mature scientists” are irretrievably lost.) For many reasons, some of which will be explained in the following paragraphs, I think that it is of the greatest importance that some stimulatable audience be reached. As yet, the beginners have no rigid scientific biases and thus may have sufficient curiosity and flexibility about the world in which they live to approach experimentation with a mind devoid of “the hierarchy of memory registers which have programmed in erroneous data.”

What I have to say will not surprise nor shock *you*, or those who are at present engaged in scientific investigation. In fact, I have read many science-fiction stories that deal with the same problem. Perhaps that is the only way that it can be approached, through the medium of a story? Yet why not present it for what it may be? Let me tell it my own way, and then, please, let me have your *coldly logical* opinion.

As to my background, I am a graduate student in the Zoology Department of a midwestern university working toward a Master’s degree, or actually a doctorate—we can bypass the M.S. if we choose—in the field of Cellular Physiology. My sponsor is an internationally known man in the field. The area of research that I have selected is concerned with the effects of physical and chemical agents on the synthesis of nucleic acids of the cell. Obviously, this is a big field, and I hope to select from among the different agents, one or two that will give “positive results.” I have been doing active research for about half a year testing the different agents. As for the *fundamental* questions raised, I am positive that it would make *no* difference in what field of science I were to work.

By now I have had enough course work to realize that when performing any assigned laboratory exercise—they should not be called experiments—even of a cook-book type, little or even major discrepancies arise, and *always on the initial trials*, no matter how carefully one works! As you are probably aware, the teaching assistant in charge of the lab or the instructor, generally runs through the exercise before the class does in order to get the “bugs” out of it—I am deliberately generalizing, since the above holds for all of the laboratory sciences—so when the student gets confusing or rather contradictory results, the instructor can deftly point out the error in the setup or calculations, or *what have you*. He may *even* indicate what results may be expected. *The last is critical*. Similarly other students in the laboratory usually have friends who have had the course before and know what results are expected—*this technique is frowned upon*. Or one may consult textbooks and published papers. (This, by the way, is known as *library research*, and is generally conceded to be indicative of the

superior student, especially if he points out the fact that he is *so interested* that he just had to delve into the literature.) By any technique, *the expected results are always obtained. Always. And by everyone.* The initial confusions—that some *honest* students perpetuate—are easily brushed aside as errors due to inexperience, sloppiness, lack of initiative, stupidity of congenital sort, et cetera, et cetera.

Since being a teaching fellow, even simple cook-book experiments don't seem as cook-bookish. Some pretty weird things have happened when I tried out an exercise prior to the class. Fortunately, I was taught to keep data—in duplicate: indelible purple Hexostick original and carbon copy. These, *vide infra*, are a few of such happenings.

Elementary General Physiology Laboratory:

1. Initial maximal vagal stimulation:
Expected results: inhibition of heart beat.
Obtained results: one series of increased heart beats. (Possible explanation: I missed the vagus nerve)????
2. Frog nerve-muscle preparation:
Expected results: a single muscle twitch.
Obtained results: a beautiful nerve twitch.
(Explanation: Eyesight? How can *nerves* twitch?)??
3. Hypotonic hemolysis:
Expected results: red blood cell destruction.
Obtained results: crenation.
(Explanation: switched salt solutions *unconsciously*)?????
4. Curarized muscle preparation:
Expected results: a synaptic block with no response of nerve when stimulated.
Observed results: a typical strychnine response, violent *tetanus*, et cetera.
(Explanation: again, I switched bottles)????
5. I shall avoid the obvious mention of mishaps with mechanical or electrical pieces of equipment. I assure you there were similar deviations in initial attempts.

Since I realize that you are preparing a paper on *Memory Registers: Stimulation Criteria*, for the VIth Annual International Meeting of the Society of Theoretical Biomathematicians in London, and are short of time, I shall avoid going into the same kind of detail as the above for other Biology Labs, and get into the real heart of the thing ... the research problem. (After all that is what both of us are interested in.) By the way, please send me a reprint of the paper when it comes out.

I guess I am really hepped up on this, because I've just got to point out for emphasis other incidences usually of a type that involved missing a whole organ in dissections or a tissue structure in histology only on the *first* study, and then re-reading the assignment—after knowing what to look for—and *subsequently finding it exactly where it is said to be*. (Ever hunt for an unknown quality—or quantity?) *So it was there all the time*, sloppy technique? Or is this branching at a control point? *cf.* LC: C. vs. B. p. 251.

To get back to my thesis research, the pieces of equipment that I have been using in the research are fairly standard in physiological research: a Beckman spectrophotometer, a Coleman photometer, a van Slyke amino nitrogen apparatus, a Warburg respirometer, pH meters, Kjeldahls, Thunbergs, et cetera. Mostly, I'm in the process of getting used to them. Also there is a high voltage X-ray generator, U. V. source and other equipment for irradiation purposes. We also have an A. E. C. license so that we can get at least microcurie amounts of the usual isotopes for radioautographic work.

Now the literature in my area is pretty controversial. (You can appreciate *that*, especially since Bergbottom at the Kaiser Wilhelm Institute bombarded you with criticisms of your theories.) Different and actually contradictory results have been obtained for the same substance in the same organism, *e. g.* alkaline phosphatase in the frog liver cell (Monnenblick, '55, Tripp, '56, and Stone, '57). To give an example, when I start a run for respiration effects using a Warburg I don't know what results to expect. Whenever this has been the case, my results have been confusing ... to say the least.

On nitrogen-mustard treated cells, in some instances the controls respired significantly *more*—even with a statistical analysis of variance—in some instances the *experimentals* respired significantly more; and in other cases the respiration for both was *exactly* the same—even *closer* than the expected deviations that should be found in any random population. One run, the blank run, *containing no cells* ... and grease-free ... consumed the greatest amount of

oxygen. To cut this letter short, the same inconstancies apply to other trials that I have made. Whenever I didn't know what to expect, and particularly where the literature was controversial, my results have been completely haywire.

Needless to say, I was not happy with this so I discussed it with other graduate students. They have all encountered the *same thing!* But most professors won't admit this to be true and merely tell me that my technique is lousy. If anything, I am an overly careful worker. Why is it when I *know* what results are expected, I get comparable results even on the *first* trial?

Remember, *I obtained the expected results* when the literature wasn't confused or when my sponsor—a most important man in my life—gave me a clue as to what kind of results to expect. *Only then.*

Now this is the heart of the matter... The obvious explanation is the lack of experience. But, and this is what haunts me ... *what if those so-called contradictory results are meaningful?* What if they were executed with care—and *they were*—and are not the results of sloppiness or inexperience? *What if a nerve can twitch?*

Very respectfully yours,
Jonathan Wells

May 3, 1958

Dr. Robert Von Engen,
Editor, Journal of the National Academy of Sciences,
Constitution Avenue,
Washington, D. C.

Dear Dr. Von Engen:

I would like to thank you for your encouraging letter and advice. I agree completely with your statement that science has a long way to go before we can explain the various inconsistencies that crop up in research. But I certainly can't see how the letter is far too "unsophisticated" for inclusion in the *Letters to the Editor* portion of your journal. While your letter should have calmed me, I feel even more strongly now after a year of research about the matter than I did before. I have deliberately postponed answering your letter until I had more *facts*.

I now find that I have accumulated—as you suggested—three distinctly conflicting groups of data on nucleic acid synthesis of frog liver cells:

1. There is a conversion of ribonucleic acid to desoxyribonucleic acid.
2. There is a conversion of desoxyribonucleic acid to ribonucleic acid.

3. The synthesis of both types of nucleic acid are independent of each other. (In addition, I have some data ... that I don't want to think about too much ... that shows that there is absolutely no nucleic acid in the liver cell.) Thus, these data all accumulated by experimental work, support all three hypotheses. Moreover, the literature supports *all three hypotheses*. I intend to go to the Woods Hole, Massachusetts Marine Lab this summer with my sponsor and get some new ideas there, especially since Professor Gould M. Rice from the University of London will be there presenting a seminar series on his work in nucleic acid synthesis in *Oryzias*.

The point is not that there is a conflict in the data, but that the data conflict because there is a conflict in my mind and in the literature. *Don't you see it?* As you said on page 20 of “Logical Control: Computer vs. Brain”: “the order-system—this means the problem to be solved, the interaction of the user—is communicated to the machine by ‘loading’ it into the memory.”

Sincerely yours,
Jonathan

August 31, 1958

Dr. Robert Von Engen,
Journal of the National Academy of Sciences,
Constitution Avenue,
Washington, D. C.

Dear Dr. Bob:

Again, many thanks for your letter—and encouragement. I especially treasure the inscribed copy of “Logical Control: Computer vs. Brain,” and the current reprint. I am sorry that I didn't get an opportunity to get down to Washington en route to Woods Hole and talk over the whole thing over a bottle of beer, *dark beer*. From what I hear of the demands on a first-rate mathematician's time these days, you should be grateful that I didn't get to see

you, because I would have monopolized *all* your time. I appreciate your generosity in extending the invitation as a rain check to me.

Your mention of the Duke School of “psychology”—my quotes—leaves me cold. It’s too obvious and puts the cart before the horse. The important point that I was trying to make dealt not with the “possible parapsychological” manipulation of equipment or the materials *a la* telekinesis to produce the desired results, *but that our Science may not be studying natural phenomena and trying to interpret them at all*. The point, to get it down in black and white, is that our “Science”—yes, quotes—may be *inventing* the reality that it is supposedly studying. *Inventing the atoms, molecules, cells, nuclei, et cetera ... and then describing them, and in the description giving them reality*.

While I was at Woods Hole I had some really good bull sessions about this very thing. I realize now that I may have been falling into the trap of solipsism, “who watches the quad,” et cetera, type of thing. Incidentally, my research is finally beginning to fall into shape. My sponsor and I had some pretty good sessions about it, and some of the screwy results I wrote you begin to make sense. I had the good luck to talk to an outstanding man in the field of nucleic acid synthesis and he was quite enthusiastic about the caliber of our work. He feels quite strongly—but has no real evidence—that the synthesis of both types of nucleic acid are independent of each other and has pointed out some significant references that I did not know about. I’m anxious to buckle down and really lick this nucleic acid problem ... in time for a June degree.

Cordially,
Jonathan

P.S.

Please send me a reprint of your lecture on “Memory Banks—Transistorized Neurones.” The lecture was ingenious, but there are some biological phenomena with which I don’t agree. Remember, I’m the biologist. Honestly, Doc, don’t you think—*entre nous*—that your idea that a living organism, can be compared with automata in picking up informational items and processing them simultaneously in parallel, rather than in series, is naif?

J.

October 28, 1958

Dr. R. Von Engen,
Journal of the National Academy of Sciences,
Washington, D. C.

Dear Dr. Von Engen:

I apologize for not answering your letter sooner. I assume you were pulling my leg when you suggested that I make a science-fiction story out of “the confused ideas of a beginning graduate student.” You might give *your* idea of a “possible science-fiction story” to one of your acolytes that has some small experience in the field of writing—not science. I am afraid that your other suggestions are not germane to the problem of nucleic acid synthesis and metabolism, a problem that has been occupying *all* my time. In fact, I’ve been doing with three to four hours of sleep these days. With the kind of concentration that I can offer the problem, there is no question that the data are falling into line, and our research is going rather well. We will show, I hope, fairly conclusively that there is little or no interconversion between the two types of nucleic acid synthesis in the cell.

Despite your ingenious mathematical approaches for stimulation criteria, in biological research—a very abstruse field—even your multiplex machines with elaborate means of intercommunication are not sophisticated enough—or ever will be—to cope with the complexities inherent in the numerous interacting biosyntheses on the subcellular ultratopographical level of protoplasm.

Sincerely yours,
Jonathan Wells

November 8, 1958

The Editor,
Journal of the National Academy of Sciences,
Washington, D. C.

My dear Professor Von Engen:

From the tenor of your last letter it is quite evident that there has been a radical change in your originally sound and inspired ideas, and which clearly indicates to me that a discussion and exchange of basic concept would be fruitless. I’m rather hurt that you question my integrity with the statement about

the “slick, calculating, career-minded cult of Ph. Deism.” Moreover, I would appreciate, if possible, the return of my previous correspondence.

I don’t feel that I am totally inept, for I have been awarded a predoctoral fellowship that will support me during the remainder of graduate school. In addition, I am being seriously considered for a faculty position at an outstanding Eastern University upon completion of my thesis. Should you be interested, we now have an article in press on the *Journal of Cellular Physiology* entitled: “Nucleic acid synthesis in the frog liver cell: A definitive study.” We have found substantial evidence which demonstrates that there is no interconversion of the two types of nucleic acid.

I cannot help but comment about your recent paper in *Scientia*—I do not believe that it is at all possible to devise computers which can handle the species of data which we obtain. Your data being less complex, of course, may fit.

Naturally, I have your confidence in the entire matter.

Yours very truly,
J. Wellington Wells

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