of different magnitude. Although a central tendency can be calculated, it varies among different insects. Thus, the mean would seem to be a much better measure of an individual's inherent susceptibility than response to a single test. If such dynamic variation exists when measured by recovery time, it is not unreasonable to assume that it exists when mortality is the end point observed. Unfortunately, a mean lethal dose cannot be estimated for each individual.

Another type of variation than that expressed by the standard deviation is that of the means of the test groups. Even though the slope of the dosage-response curve for a given toxicant applied to a test insect is relatively stable, it is well known that the LD_{50} is found to vary from day to day, from culture to culture, from laboratory to laboratory, and from one condition (e.g., temperature) to another. It is thought that some of these observed differences are due to differences in technique, and certainly some of them are. But considering the world population of a single insect species, each test group is but a small sample of the population at a particular time. The means of all test groups must vary widely from place to place, from time to time, and under different conditions, even though the techniques for study might be identical. At present nothing is known about the distribution of these group means. The range of distribution might be so wide, however, that in localized areas little chemical selection would be required for segregation of resistant groups.

Of the three variations—variation in response by the individual insect, variation in response by individuals about the group mean at a particular time, and variation of the means of test groups (considering the entire population of the species at all times)—only the second can be described easily, and only this one has been used in selection for resistance. And yet this may be the one least likely to yield the desired results. If the first type of variation is to be found generally with different insects and with different toxicants, the apparent phenotype as judged by a single test may be quite different from the genotype, and genetic studies based on selection using the dosage-response curve may be faulty, success being largely fortuitous.

There is one technique that has been used with signal success in demonstrating development of resistance in the laboratory, probably because the first type of variation is unwittingly taken into consideration. This technique (Bruce and Decker, *Soap Sanit. Chemicals,* 26, [3], 122, 145 [1950], and others) involves the exposure of houseflies to DDT—not as a single application, but continuously throughout larval life. This treatment would eliminate any individual which was even temporarily susceptible and would permit survival of only those individuals that were consistently resistant.

By taking cognizance of these three types of variation, it may be possible to reconcile data that now appear conflicting, but to do so requires more information about the distribution of the respective variations, their interrelationships, and the physiologic and ecologic factors contributing to each. Certainly, genetic studies can be conducted on a more secure basis if this information becomes available.

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Research vs. Proprietary Interest

WHY is it that many scientists are behaving like the "man in the street," who may be a well-balanced individual until someone brings up a controversial subject in religion or politics? I had always considered a good research man as the best available example of judicial detachment, able to study scientific matters objectively, and to discard even his own data if found to be of doubtful significance.

Now I am unhappily discovering that a considerable number of rather important scientists are unable to discuss security regulations or loyalty oaths without exhibiting either an attitude of sophomoric resentment or the type of prejudiced argument one expects from the man in the street, to whom the subject under discussion appears to be either all white or all black. Although there have been some calm and scholarly presentations of certain dangers to academic and scientific freedom, such have not seemed to be the rule. In a subcategory of this group, I find that some scientists who have served with, or been in contact with, the military allow themselves to sound off much like the ex-soldier who hated the first sergeant.

Enough of complaint. All our training and experience in research lead us automatically to consider both sides of a moot question. Matters of procedure now being argued—in typical American style—are of the gravest importance to the future of research and to the future of our country. Both these matters are important. It is the security of our country that makes possible the significant advances in all of science. Men of the greatest sincerity are trying to maintain that security, and those who are charged with that duty are obliged to set up rules, a procedure necessary in any institution of great size. It is true that some of the men who administer these rules, although possibly sincere, may be men of limited vision. It is also true that there are occasions when the earnest scientist, immersed in his own problems, finds a fence where he thought to find a gate, and in his frustration speaks out in unscientific style (most of us show such human frailty at times). May we not wait to cool off, however, before we write a book or a review about it?

It appears to the writer that scientific research, like all human existence, is beset with obstacles, differences of opinion, obstinate data, annoying rules, limitations of time and space, etc. To that is now added the occasional cooperation and sometimes the supervision of some government agency. Not all of us will admit that this supervision is necessary (some of us still dislike stop signs in traffic regulation), but the situation does exist. It is certainly not palatable to consider this as a sort of parental supervision, considering the mental caliber of some of the supervisors, but it could be likened to a business or family partnership. The wise man knows that his business partner or his wife may sometimes want to go when he wants to stay, but he also knows that the disadvantages of the partnership —the items on which he surrenders his own preference —are greatly outweighed by the advantages. Therefore, he accepts certain limitations. Is it not possible to do scientific work within such a framework in some degree of contentment, at least until certain potential dangers have receded?

If the trends that appear to threaten academic freedom and even seem to hamstring the progress of research are as dangerous and ominous as claimed, why not consider the matter as a research problem and really study both sides? This type of thing is susceptible of reasonable solution in the conference room, provided both sides are represented by intelligent men who can see the forest as a whole, as well as by men who are still threading their way through the trees. The military and administrative sides must be properly represented, for the scientists alone may find it difficult to make up their collective mind (*cf.* the National Science Foundation!). Of course, we scientists admit privately that our mental processes are a bit superior, but let us try to listen with complaisance to the viewpoints of others.

The turmoil about faculty loyalty oaths has always puzzled the writer. Is it entirely because of the threat to academic freedom, or do unmentioned feelings of personal dignity regarding the unassailable integrity of the scholar complicate the situation? This is only one point of view, but the writer would be very happy to see the research scientist approach the whole problem with more scientific detachment, trying to understand the necessities of those charged with the protection of our national security (and with it our fine research facilities), recognizing without condoning certain weak links in the administrative chain, and, above all, carrying scientific methods and ideals and dignity into the argument, not forgetting these ideals when someone gets a blow on the nose.

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Pressor and Oxytocic Hormones of the Pituitary Gland

IN THE past few years, by the use of improvements in analytical methods of extraction and of adsorption and elution, du Vigneaud and his collaborators (Pierce and Turner) have added much to previous knowledge concerning the chemistry of the pressor and oxytocic hormones of the pituitary gland. Enough is now known concerning the amino acid constituents of these hormones to warrant the following observations concerning the relationship between them.

According to du Vigneaud and his collaborators, the acids common to both hormones are tyrosine, proline, glutamic acid, aspartic acid, glycine, and cystine. In addition to these, the pressor hormone contains arginine and phenylalanine, and the oxytocic hormone, leucine and isoleucine.

The presence of phenylalanine in the pressor hormone is in contradiction to the work of Stehle and Fraser, who reported it to be absent. The absence of isoleucine from the same hormone is in contradiction to the work of Stehle and Trister, who reported it to be present. The work of Stehle and his collaborators was done with a preparation much inferior in potency to that investigated by du Vigneaud and his collaborators. The isoleucine reported by the former may have been contained in the ballast of the pressor preparation. The absence of phenylalanine is not easily explained, since conditions were favorable for its detection. If the reader is willing for the moment to accept the results of Stehle and his collaborators as correct, the results of du Vigneaud and his collaborators have what seems like a plausible explanation. In the starch column method of separation, phenylalanine and isoleucine appear in close sequence in the eluate, so that it is possible what was reported as phenylalanine may have been isoleucine.

If this is true, then the interesting conclusion follows that the only difference between the pressor and oxytocic hormones is the occurrence of leucine instead of arginine in the oxytocic hormone and, vice versa, the occurrence of arginine instead of leucine in the pressor hormone. The possibility that one hormone may be derived from the other occurs immediately. The introduction of a guanidine group into leucine with the elimination of a methyl group would convert the oxytocic hormone into the pressor hormone. The reverse, the conversion of the pressor hormone into the oxytocic, requires the elimination of the guanidine radical from arginine and the introduction of a methyl group.

The ideas expressed are not compatible with the conception that the two hormones are split products of a giant molecule.

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Electrokinetic Behavior of Dilute Monodisperse Sulfur Hydrosols

THE development of dilute monodisperse sulfur hydrosols by LaMer and Barnes (*1*) has resulted in the study and solution of a number of problems previously unattainable with polydisperse sols (*2–5*). However, the electrokinetic properties of the dilute monodisperse sols had not been studied in connection with any of these investigations.

Recently such a study was made, using a microelectrophoresis method (*6*). Sols prepared with dilute sodium thiosulfate (0.002 *M*) and HCl (0.001–0.003 *M*) were found to contain *positively charged particles.* Previously, the charge on the sulfur particles in