lying and injustice are phases of immorality, the vitamin deficiency in the human race of our generation is indeed appalling! When we have the vitamins that prevent dishonesty and injustice, the millennium will have come. But as I read and listen to the 1942 propaganda for vitamin pills, I am led to suspect that at least some vitamin vendors do not take their own medicines. They just sell them. And the selling noise is so loud that our attention is distracted from the more important role of adequate proteins in preventing one of the fundamentals in the overall malnutrition created by such catastrophes as crop failure and war.

(F) *Waste of food.* Waste of food in the family increases the cost of food to the family. It also contributes to food scarcity. Food waste in our country is partly avoidable such as the waste in the home, waste or neglect of fruit and vegetable on the farm; partly perhaps unavoidable such as that produced by oxidative rancidity of fats in the foods. I have already referred to the huge waste of skimmed milk, so far as this is turned into channels other than human food. The waste of food in the home tends to increase with the economic prosperity of the family. It is not "refined" to clean the plate. It is a measure of prosperity and caste to waste food. When we are told that under conditions of war and food rationing in England and Scotland the economically more fortunate eat one third less food than they did in times of peace, this probably means that they eat just as much food in war as in peace, if they can get it, but in war they waste one third less food at the table. When nations are facing universal food shortage, the wastage of food in the home or on the road from the farm to the home may seriously contribute to national malnutrition. Much of this food waste is even to-day preventable, but we are up against individual and social habits and agricultural practices that will yield only to education or dire necessity. Some food wastage in the home as well as in the food-processing industry appears to be based in part on the erroneous assumption that the least roughage in the food we eat the better for our health. So we peel and prune fruits and carrots, cabbage and potatoes and with such peelings and prunings we decrease or eliminate valuable food elements. The facts are the normal human alimentary tract must have some indigestible roughage to work at its optimum, and it will probably take millions of years before man has evolved, like that of the honey-bee, an alimentary tract which can function on nectar and pollen alone. It is difficult to assess the blame for some of the waste of our food, particularly the fats of animal origin. We seem to have developed the idea that the fattest hog and the fattest steer is the best hog and the best steer. These animals may be best in the sense of providing greater income to the farmer, but a great deal of the fat of the steer and even some of the fat of the hog does not reach the human stomach. From the kitchen or the dining table it passes into the garbage can or down the drain pipe.

(To be concluded)

RESEARCH IN WARTIME

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AT a time when the nation is engaged in a gigantic war that will determine the very survival of civilization, it is appropriate to examine all our activities in the light of values in the war effort. The only purpose of any real significance for any one's actions is the defeat and destruction of the ememy. All personal or political motives represent various forms of treason. Every activity not contributing to the war effort should be either stopped or redirected. Scientific research should be examined with this point of view.

Modern war requires modern weapons, and these are directly the product of scientific research. It is true that it is a long way from the birth of ideas in the brains of the scientists, their demonstration, verification and study in the research laboratories to the final perfection of powerful weapons for the destruction of the enemy or of materials and tools for the aid and comfort of ourselves and friends. This way, however, follows a direct path that is fully recognized. From the source of the original ideas it progresses through development, engineering, trial and production. All the weapons, tools and materials of vital importance in the war, to a very considerable extent, have had their beginnings in the scientific research laboratories. The research of the past produced our present practices and materials. New things for the future will have their beginnings in the research being done at present. The only sure way of not having new weapons or major improvements of old ones is to stop scientific research.

Research is a word that is used for different meanings by different people. In some industrial firms it is made to include the development of industrial processes after the original ideas are all well founded. In other places it is used to include data collecting or the

operation of well-known and recognized techniques on a small scale in a laboratory. These activities coming under the name research are easily evaluated in relation to the war effort. A process or machine should be developed if it contributes directly to the war effort. Data should be collected if there is a wartime need for it, but otherwise not. In the same way small-scale laboratory operation of known techniques should be continued or not in relation to the use or non-use of the product for the war effort.

The more subtle meaning of the word research—the birth, demonstration, verification and study of new ideas—is a much more difficult thing to evaluate. This type of work is chiefly done in college and university laboratories. To the layman it appears to be only an academic exercise as it is not clearly defined, is not represented at that time by human use on a large scale, and most frequently is clothed in a formidable garment of technical terms. Neither industrial nor government funds are available for it, as the results to be achieved can not be written out in advance. All industrial firms and government agencies require well-formulated objectives for a project as well as a detailed outline of methods of procedure. For fundamental and original research as defined above, these are completely impossible. The real cost of this kind of work comes chiefly out of the hides of those whose artistic or fanatical urge causes them to do it. It comes in the long days (and nights), the Sundays, holidays and vacations spent on the work, frequently without much encouragement or material aids. After the fundamental work is well on its way and the new ideas have been amply demonstrated, government agencies or industrial firms will carry out the development work. The history of science gives numerous examples of the above. The early work in electricity, in organic chemistry and in physical chemistry can be cited, for these, despite their present large-scale industrial use, began in the academic scientific laboratories.

This kind of work has in the past produced results that have changed the entire course of history. Examples of this are the discovery of electromagnetism, the vacuum tube and the synthesis of ammonia and of indigo. On the other hand, the work of any one particular man may not result in such world-startling discoveries, it may only add a small but valuable amount to the sum of human knowledge. It is impossible to predict the value of the fundamental research of any one man. We do know that in general it is this work that is the fountain head of new developments. How soon or how long any particular piece of original and fundamental research will produce results that can be directly or indirectly of military value can not be stated. The undisputed fact is that the more of this kind of work in progress the greater chance there is of new weapons.

The national cost of fundamental research in men and materials is relatively insignificant. There are only a relatively small number of scientists engaged in this work (a few hundred) and their requirements in the way of equipment and help, when considered on a national scale are relatively insignificant. However, at the present time it is not sufficient to take a point of view that we can "let" them work. Letting at this time must be a positive and not a negative action. It must mean that such scientists and their students be not required for other duties such as military service or civilian defense. Their income must continue. They must have help and funds for securing materials. They must be provided with assistance and technical aids. In other words, funds, priorities and draft deferments have to be considered as part of the word "let." The assignment of priorities for research materials shows that this is recognized by the government. It is of course also important that the institutions in which this work is in progress should not consider it as an unessential academic exercise or an extra burden on the overhead account.

At the time, several years ago, when our government first recognized that our entry into the war was unavoidable, it was natural to take the point of view (and this was done by some of our military people) that the research that would be used or needed in this war was all done and that the war would be fought with what we had. They thought the same about steel capacity and the designs of tanks and planes. As the military leaders can not predict the length of the war, the time available for research to aid in it is also unpredictable. If the war is short, there is no need of disturbing or changing the present fundamental research program. If it be long, then there is a vital need for both continuing it and augmenting it in the near future. No one can tell from what laboratory will come new and important discoveries. We are certain, however, that the more fundamental and original research done the greater the chance of the discoveries. The one way not to have the discoveries would be to close the laboratories. We may be in vital need of new discoveries. Without them we shall be placed in the position of giving the enemy the initiative and following his discoveries with a time disadvantage.

Scientific advancements made during the war may be very important. The development of the synthesis of ammonia in Germany in World War One caused the blockade to be much less effective than the military

leaders of the Allies figured, because Germany was then not dependent upon Chile saltpeter for its explosives. It is the new discoveries made now or in the near future that may shape the course of victory.

To be unduly critical of any one particular fundamental research project as impractical or of only "theoretical" interest and therefore not significant for the war effort at this time is highly unsound reasoning. How can any one tell in advance to what extent or how soon it can be put into practice. What is highly theoretical to-day is put into large-scale practice to-morrow.

One can not determine the value of research to the war effort by whether or not it is fostered or financed by a government agency. Government agencies support and develop discoveries once they are made, and they also direct this development toward ends of military value. New ideas and discoveries, however, are most likely to spring up in laboratories devoted to original research, and it is these new discoveries that can then be taken by the government agencies for development. In fact, those discoveries made in the recent past in chemistry that are having the most significance at present in the war effort were not made in government sponsored laboratories. Some of these in their beginning stages would not have been financially aided by any government agency or industrial firm, as they differed greatly from commonly accepted practice. In its inability to know just which laboratory, working on original work, will produce the most vital results for the war, science is not much different from the military. The high command can not tell in advance just where the most important battles will take place. Many garrisons and bases will be unused. Many divisions and material will not see action. We can not in advance, however, say that these divisions are unnecessary.

Fundamental and original scientific research is, therefore, not only of great value for the war effort, but its continuance may be vital for victory. It is not an activity of secondary value for such things as morale or the selling of bonds, regardless of the values of these activities. Its products may have a direct bearing on killing the enemy and certainly will have, if the war lasts for years. How to continue it under present difficulties and restrictions is a matter for serious consideration. In this connection the following suggestions are made:

(1) Scientists of recognized tendencies toward original work and of proven accomplishments in fundamental research should be encouraged to continue. They should not be taken for what may be more emotionally satisfactory, work on a development project under a government contract.

(2) Priorities and draft deferments should be provided.

(3) Funds will be needed; but these should be free from the requirements of extensive reports and outlines of procedures in advance, such as are now required in most government and industrial work.

(4) The work should not be "directed" by some "committee chairman." Committee chairmen can not be familiar with the work—otherwise they would be doing it.

(5) The funds should not be dependent upon the individual scientist's political connections or his ability to get elected or appointed to committees or society offices.

(6) Students should be permitted and encouraged to enter fundamental scientific work. A great deal of this kind of work has been done in the past by students under the direction of a professor interested in fundamental research. At the present time there is danger of encouraging students to enter the so-called more "practical" lines of work. General Hershey in his statement regarding the deferment of graduate students considers only those assisting in instruction and those working on a sponsored "war project" as eligible for deferment. Research on fundamental and original scientific problems should be placed in the same category as the work sponsored by government agencies as its chance of direct value in the war is at least as great.

(7) In order to insure that the scientist engaged on original and fundamental research can direct his thoughts along lines such that his creative ideas will have the best chance of being of immediate military value, he should be made familiar with the problems, materials and techniques of military importance that are related to his branch of science. This can be done without confining him to work on any particular project and without danger of loss of confidential and secret subject-matter. The scientists of this country realize full well that our defeat will mean that they, each and every one, will be executed shortly thereafter. They have the knowledge of what has happened to their colleagues in the occupied countries of Europe. They are very anxious to help so that this will not happen here.

The day that fundamental and original scientific research stopped in Germany was the day that Germany began losing the war. This will be true provided that we do not stop our fundamental scientific research. We are in great danger of doing this at this time with our scientists being taken for development projects and other activities. If the war is a long one, we will greatly regret the loss of the scientist and his students doing original work.