mental purposes is neither difficult nor expensive and permits variations in transmission that may at times be desirable.

The greater number of dyes, particularly those approximating black, strongly absorb the range that we wish to transmit, but a combination of methylene blue, fuchsin and phosphine, all of which transmit considerably in the near ultraviolet, will make an approximate black. These can be used in a liquid cell, but it is much better to incorporate them in a gelatin film, as they are then fairly stable and very much more convenient.

To make a glass filter the use of old photographic negatives is recommended but not essential. The old gelatin or emulsion may be readily removed with a 1-per cent solution of caustic or half-strength nitric acid. Either method leaves the glass sufficiently clean so that a substratum is not necessary provided the emulsion side is marked and used to receive the new gelatin coating, which is prepared as follows:

Dissolve .4 gram of methylene blue in 100 cc. of hot water to which is added 1 drop of glacial acetic acid. Make a similar solution of fuchsin and phosphine, omitting the acetic acid. Make a 10-per cent solution of gelatin by allowing it to stand until it is swollen, and melt with gentle heat. Add 2 drops of glycerin to each 25 cc. Pipette 10 cc. of the phosphine, 8 cc. of the methylene blue, and 3 cc. of fuchsin into a cylinder, and add 19 cc. of the gelatin. Mix thoroughly and, if necessary, add 1 drop of normal butyl alcohol to break the bubbles. This amount should be flowed over an accurately leveled 8 × 10 glass and, when gelatin sets, this may be dried in an upright position.

One of these glasses will be sufficient when the fluorescent ultraviolet tubes are used, but it may be desirable to use two, gelatin surfaces face to face, when screening out the visual light from the mercury arc. Visual appearance of fluorescent materials with filters made in this way is comparable to the nickel oxide glass, although the transmission for a given amount of screening the visible light is not quite as good. As commercial dyes were used, it is not expected that the proportions given will produce the most efficient results unless an accurately standardized dye is used. This ratio is based on using Heller and Merz Blue 2 B Dustless, Fuchsin RTN Powder, and Phosphine 3 G 100 per cent. Of course, dyes of other manufacture can be used, and the variation can be adjusted by altering the proportions. Visually, these filters almost screen out an illuminated white cloud with a residual color of deep red, modified by a slight blue transmission. Very thin layers appear to be a rusty gray.

By doubling the concentration of the gelatin and correspondingly increasing the glycerin, it is possible to strip the film from the glass if desired. By treating plate glass with a silicone stopcock grease and polishing off the last visible trace, the gelatin film can be easily stripped merely by cutting around the margins of the plate of glass. In this way the transmission can be extended further into the ultraviolet that would be absorbed by glass.

Letters to the Editor

Some Notes on the Cancer Problem

In writing the following lines of comment on K. S. Pilcher's letter (*Science*, 1946, **104**, 167) concerning an organized attack on the cancer problem, I wish to acknowledge his well-done, traditionally constituted argument for a coordinated, well-financed research program. Certainly I do not wish to argue against a program of such highly important and humanitarian purpose. I merely wish to point out a point of view which, in my opinion, may not be basically correct.

The argument for a planned and completely coordinated cancer program on a national scale is based on the rapid results obtained in the fields of atomic fission, penicillin and other antibiotics, etc. by means of highly coordinated research efforts. However, an examination of the background of these problems shows that the rapid practical evolution was really in the region defined as "development" rather than "research" or "discovery." Thus, the rapid development of the atomic explosive was subject to the fortuitous discovery of the fission of a particular nucleus under a particular treatment, the fission occurring in a particularly desirable manner. I believe that I am correct in saying that the fundamental idea of a self-perpetuating atomic fission chain is relatively old, including the general ideas of power generation and the causation of explosive decomposition. That is to say, the principle of generation of such a reaction was seen to lie in finding a nucleus capable of producing enough of the right kind of secondary particles to carry on the chain. On the basis of knowledge in the 1930's, the finding of such a nucleus may well be called fortuitous. In the case of penicillin, too, I believe that it is correct to say that it was essentially chance that the particular conditions were accumulated simultaneously to show the antibiotic effect in a spectacular manner; once the goal was seen, the mass-development of the processes, etc. was a foregone conclusion.

However, in a field in which no clearly defined line of approach can be said to exist, it is quite doubtful that a rigidly planned program would produce results more rapidly than the time-honored method of "independent groups." The difficulty of a planned program in such an instance is obvious: a considerable concentration on much detail in relatively few directions. The probability of hitting the correct solution would depend simply on having one of these few directions coincide with a particular, a right, method of approach. Looked at in this manner, the probability of hitting the correct solution is not very good.

While I am by no means a specialist on the cancer problem, I feel that it is justifiable to say that as yet we do not have a clear-cut line of approach to the solution. We are still irradiating with electromagnetic energy and with radioactive materials, we are still using surgery, etc.; in a word, we are doing much the same things that were being done years ago, in the factual sense of the word. To be even more specific, I would say that we are still completely at a loss, and that no one can predict which of the many branches of the physical science will eventually lead to the answer. While the promising lines should be pushed by sheer "man-hours," there must be at least a corresponding support of the "individual groups" which, not being tied to a definite approach, would have, in my opinion, a greater chance of indicating the right direction to be followed by the mass-effort, which would not be too difficult to finance once the probability of success begins to approach unity.

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On the Theory of Acids and Bases

There is one theory in our biochemical and biological sciences that needs drastic revision, and that is the idea of acids and bases.

It seems to me that we should accept the theory that an acid is "anything" (ions, molecules, or particles) that yields hydrogen ions and that a base is "anything" that combines with hydrogen ions. This would get rid of the idea, for example, that a sodium ion is a base and brings forth the idea that such ions have other functions in the animal body, such as irritability and osmotic pressure regulations.

Many biochemists and biologists say that a food is basic in reaction because it contains sodium. This seems incorrect, because the negative ion should be considered the base. For example, if one takes sodium citrate into the body, the citrate ion is the base which, when oxidized, yields the bicarbonate ion, one of the most important bases of the blood. Vegetables are basic forming foods because they produce bicarbonate ions. The production of such a base takes place because there must be as many negative as there are positive ions, and when carbon compounds are oxidized in the body, they will yield bicarbonate ions if there is a positive ion with which to combine; otherwise, the $HHCO_3$ formed is eliminated as CO_2 and H_2O. When a chloride or sulfate ion, as in table salt, is already present in the food, the $HHCO_3$ is also eliminated as stated above.

The theory that the kidney functions in acid-base balance because it retains sodium when the acids are formed in the body is out of date. It seems more logical to say that ammonia is formed in the kidney which combines with the acid to be excreted, so that the body does not deprive itself of the sodium needed for osmotic pressure and nerve regulations and in order that the bicarbonate ion (alkali reserve) will not be used up. For example, we can show:

$$2\overset{+}{Na} + \underset{\text{base}}{2\overset{-}{HCO_3}} + \underset{\text{strong acid}}{H_2SO_4} \rightarrow 2\overset{+}{Na} + \underset{\text{very weak base}}{\overset{--}{SO_4}} + \underset{\text{weak acid}}{2H_2CO_3}$$

$$H_2CO_3 + \overset{+}{K} + \underset{\text{base}}{\overset{-}{Hb}} \rightleftarrows \underset{\text{weak acid}}{HHb} + \overset{+}{K} + \underset{\text{base}}{\overset{-}{HCO_3}}$$

$H_2CO_3 \rightleftarrows CO_2 + H_2O$ or to prevent loss of alkali reserve,

$$\underset{\text{base}}{2\,NH_3} + \underset{\text{strong acid}}{H_2SO_4} \rightarrow \underset{\text{weak acid}}{2\,\overset{+}{NH_4}} + \underset{\text{very weak base}}{\overset{--}{SO_4}}$$

Organic acid $\xrightarrow{(o)} CO_2 + H_2O$ is therefore not acid in reaction when metabolized in the body. Why say the potassium or sodium ion is a base when it does not enter into the reactions?

It should be made clear that meats act acidic in the body because when they are metabolized they form H_2SO_4 and H_3PO_4, which are strong acids and must be neutralized. Sodium chloride and sodium sulfate are not basic, because the chloride and sulfate ion are very weak bases, while monoacid phosphate and bicarbonate ions are rather strong bases. Why can't such ideas be used in the explanation of acid-base balance in the animal body? This would get rid of the ridiculous idea that a sodium ion is a base.

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Injection vs. Oral Administration of Folic Acid in the Chick

Previous experiments (D. V. Frost, F. P. Dann, and F. C. McIntire. *Proc. Soc. exp. Biol. Med.*, 1946, **61**, 65) indicated that 10 μg. daily of synthetic folic acid (*L. casei* factor, kindly supplied by Lederle Laboratories and identified as pteroyl glutamic acid) was sufficient to produce good growth, normal feathering, and near-normal pigmentation in chicks over an 8-week period when the material was given by intramuscular injection. When 10 μg. daily of folic acid was given orally under similar conditions, growth, feathering, and pigmentation were relatively poor. At levels of 2.5 and 5 μg. daily of folic acid there was an equally marked difference in the response between injection and oral administration. The following experiment was set up to resolve clearly the question whether synthetic folic acid was more active by injection than when given by mouth.