

itself might arouse suspicion, especially when another investigator claims that GSSG acts as an "H-acceptor" after penicillin injections (6). In a study carried out with microorganisms, it was concluded that "such simple experiments do not themselves afford unequivocal proof of the participation of glutathione in the mechanism of penicillin action. However, it is *generally assumed* [italics ours] that -SH groups are involved" (7). Penicillin was found to inhibit the enzymic hydrolysis of GSH, but this inhibition could be overcome by the addition to the medium of more GSH and glutamine (8). If the -SH groups of GSH would be blocked, then the addition of GSH to our medium could not be expected to inhibit the oxidation-reduction system postulated by us. It certainly could not inhibit the synergism caused by insulin. The complete inhibition whenever an excess of GSH was added may be better explained by the well-known inactivation of insulin in the presence of GSH, or by its ability to keep the added vitamin C or other still-unidentified factors in their reduced states.

Penicillin is known to be inactivated rapidly at 37° C, the temperature at which all our experiments were carried out, but the nature of the inactive end product is not known, nor is it definitely established by what mechanism the bactericidal action of penicillin proceeds. The hypothesis is advanced that some assimilatory processes are blocked at the cell walls, such as the assimilation of glutamic acid (9). One might speculate that some of the biological activity of penicillin is related to its optical configuration, which happens to be the "unnatural" one, and that penicillin may be able to enter metabolic process in microorganisms capable of metabolizing compounds having the D-configuration. Whether such reasoning still holds for mammalian cells is debatable. Many enzyme preparations from mammalian cells, for instance, serve usefully in the enzymic resolution of amino acid racemates.

It is very dangerous to compare *in vitro* experiments with clinical cases, because of the extreme differences in conditions. Clinically, the favorable results obtained in cases of rheumatoid arthritis with DOC plus vitamin C are open to question, and, according to the most recent clinical findings, previous favorable results have not been confirmed. It is therefore fallacious to advocate clinical trials involving penicillin as an adjuvant to DOC plus vitamin C on the basis of speculative inferences drawn from *in vitro* experiments. The favorable results obtained by the combined use of DOC plus vitamin C in psychiatric cases are also being questioned. Clinically, the combined cortisone and insulin shock therapy in psychiatric treatment gave immediate favorable results in about 50% of the cases (10), but the final result was not different from insulin shock therapy alone after discontinuance of cortisone.

In a preliminary report on clinical trials (11) it was shown that the administration of insulin reduced the cortisone requirement in the treatment of rheumatoid arthritis in all stages by as much as 75% of

the dosages usually given, and that this type of therapy apparently eliminated all the usual objectionable side effects of cortisone. None of these patients had received penicillin at any time immediately preceding this type of therapy.

These are but a few of the reasons why we have not taken penicillin into consideration as an active participant in our system. We believe that penicillin serves no other purpose than to keep our system free of bacterial contaminants, and that any other conclusions drawn from our experiments with respect to the action of penicillin cannot be supported on the basis of experimental facts available up to date.

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The Monarch Butterfly

IN YOUR pages (*Science*, **113**, 68 [1951]; see also p. 729) there was published an article by W. D. Field, J. F. Gates Clarke, and J. G. Franclemont on the Commission's decision that in future the name *Papilio plexippus* Linnaeus (1758) shall apply to the butterfly known in America as the Monarch, thus putting an end to a controversy that has troubled lepidopterists for at least a generation.

In the Minutes of the Paris Meeting of the Commission, this decision is recorded in the following words:

- (1) to use their plenary powers to direct that the trivial name *plexippus* Linnaeus, 1758 (as published in the binomial combination *Papilio plexippus*) should be applied to the American species figured as *Danaüs plexippus* by Holland (W. J.), 1931, *Butterfly Book* as figure 1 on plate 7;
- (2) to place the name . . . as determined in (1) above on the *Official List*. . . .
- (3) to render an Opinion setting out the decisions recorded in (1) and (2) above.

This statement, for which I take my full share of responsibility, means no more, and no less, than it says. It does not fix any "type" specimen or "type" figure of *plexippus* and it does not imply that the figure to which reference is made belongs to any particular subspecies of *plexippus* from any particular "type" or other locality. It says, in effect, only that

this is a figure of the species with which we are dealing. Nobody can deny that Holland's figure represents a specimen of *Danaus plexippus*. That issue, therefore, is settled.

However, it was very soon pointed out quite conclusively by Field (*Proc. Entomol. Soc. Wash.*, 52, 234 [1950]), that Holland's figure was not of *Danaus plexippus plexippus*, but of *Danaus plexippus megallippe* Hübner (or *nigrippus* Haensch), the Central and South American subspecies. Though I cannot see that this fact is really of the slightest importance, I admit it would have been better had a figure of *D. plexippus plexippus* been quoted. For this "error" I am to blame, not Hemming, though in fairness to myself I must reaffirm that I considered, and still consider, that any good, easily accessible figure of *D. plexippus*, of any race or subspecies, would serve the Commission's purpose. Nevertheless, Field argued on the basis of his findings that "the Commission did not fix the name *plexippus* to the North American Monarch . . . but to a distinct subspecies," a statement for which I cannot find the slightest support in fact or in logic.

In order that the Commission might consider how best to remove from the minds of lepidopterists the doubts (however unjustifiable) that had arisen through the reference to Holland's figure, it was decided to consult a sample of well-known lepidopterists on this specific point, prior to rendering the necessary Opinion. The letter and draft application to the Commission subsequently sent out did unfortunately contain errors of fact, to which your correspondents properly call attention. It would have been but common courtesy to have called the Secretary's attention to these, especially as the circular was a personal inquiry from the Secretary upon which the personal views of the recipients were sought.

However, as your correspondents preferred to comment publicly and in the severest terms on these private communications, and on the prior action of the Commission, it may be well to point out some of their own errors.

i) It is false to charge the Commission with having committed in Paris "a serious error" "in haste": your correspondents are in error in reading more into this decision than is there; and to reach a decision quickly on a matter that had been before the Commission a year or more, and before the lepidopterological public some 25 years at least, cannot justifiably be called "hasty."

ii) "Hemming's campaign to replace the Law of Priority by *Nomina Conservanda*." There is no such campaign; on the other hand, there is very strong pressure upon the Commission from all parts of the world to put an end to uncertainties and unnecessary changes in generic and specific names, and I am convinced the methods now being adopted, including in particular the expansion of the *Official List*, are the best for the purpose.

iii) The "careful lectotype designation already published" does not exist. The reference is no doubt to

Corbet's paper (*Proc. Roy. Entomol. Soc. London*, B, 18, 184 [1949]). Corbet's words are, "I have no hesitation in taking the male specimen bearing the Linnean name label as the name-type of *P. plexippus* Linnaeus, 1758." This is not a lectotype designation; it is a claim to have recognized the holotype. [The authors of the article make no reference to the fact that, in spite of this, Corbet's article ends with the statement that in submitting his application to the Commission he recommends the retention of the name *plexippus* for the American Monarch butterfly.]

iv) "Hemming neglected . . . several prominent interested lepidopterists." This statement is unworthy of your correspondents. No attempt was made to send the circular to all interested lepidopterists. A random sample, whose names were supplied mainly by me, was taken. Would I have selected the authors of this article had my choice been biased? Nevertheless, I regret that other names did not occur to me at the time.

v) The Commission having reached its decision on the application of the name *plexippus*, a decision endorsed by Congress, it is not open to the Commission to "reconsider" the matter, as requested by your correspondents.

Much more could be written in criticism of your correspondents' article. But there is much wisdom in the old saying "least said soonest mended." I have no wish to add bitterness to this controversy. It requires objective cooperation, not polemics.

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Up or Down?

E. C. ZIMMERMAN's note (*Science*, 113, 391 [1951]) on volcanism as a contributing factor in change of sea level states that the outpouring of lava on the sea bottom has been sufficient to raise sea level as much as 500 meters, "even if generous allowance is made for compensating subsidence."

It would appear to me that the "accompanying subsidence" would have been 100% *plus* the volume of new land raised *above* sea level. Thus the net result would be a slight *lowering* of sea level. If subsidence did not equal the displacement of outpouring there would have had to remain gas-filled cavities and this seems untenable.

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Erratum

In Table 1, on page 675 of our article entitled "An *In Vitro* Method of Screening Amoebicidal Agents Using the Phillips Culture" (*Science*, 112, 674 [1951]), the formula 7-iodo-5-sulphonic acid-8-hydroxyquinoline should read 7-5, diiodo-8-hydroxyquinoline.

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