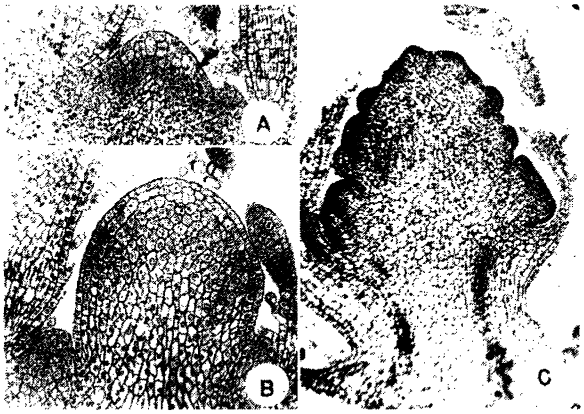

FIG. 3. Apices of *Xanthium* plants. *A*: vegetative; *B*: induced 4 days; *C*: blossom primordia after 7 long nights. (Preparations by Wm. F. Millington.)

growing points produce blossoms depends upon cultural conditions and plant vigor. The so-called vegetative shoots produced by "induced" plants, as following pruning, for instance, are in reality induced branches comparable in morphological condition to the main axis of a plant which has had only one or a few long nights and which requires 3–5 weeks for macro blossom buds to appear.

Plants that flower terminally have noninduced vegetative buds from which new shoots arise with a change in the environment.

It is obvious that the cocklebur, which has been looked upon as furnishing evidence for believing that induction causes blossoming, has a definite if not marked degree of independence of these two phenomena. Future studies of why plants blossom could profitably give more consideration to the "competition" or balance between the vegetative and flowering stages as well as the role of induction in blossoming.

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Comments and Communications

The International Commission on Zoological Nomenclature and the Name of the Monarch Butterfly

WE RECENTLY prepared a paper about the name of the Monarch Butterfly, protesting the hasty and wrong action of the International Commission and also Hemming's mail proposal to correct the error. After seeing the communication by Field, Clarke, and Franclemont (*Science*, **113**, 68 [1951]), which is so much like the one we were writing, we believe that our paper is unnecessary, and that a brief note will serve to show our entire accord with their paper.

First, we wish to say that it is impossible for us to understand the misstatements of fact made by Hemming in his mail proposal. We refer to his statement that the North American species "came to be known as *Danaus plexippus* (Linnaeus), the name universally applied to it" (p. 2, Point 4 of the Hemming proposal), and to his statement that the Indo-Oriental species "is now universally known as *Danaus genutia* (Cramer)."

Field, Clarke, and Franclemont have shown (and we have verified) that these are not correct statements. Misstatements of fact like these put Hemming in a very bad light, at least among us here. As secretary of the International Commission, his responsibility in writing and circulating such proposals is indeed a very great one—he should not advance statements that are so far from reality.

Second, an important fact was overlooked by Hemming in his proposal. This is the designation by Corbet of a "male specimen bearing the Linnaean name label as the name-type of *P. plexippus*" and thus as the lectotype of this insect. This male is a specimen of the Chinese species, not of the North American species. We agree with Field, Clarke, and Franclemont that the commission should first consider this designation before taking any action on the matter.

We go further and say that we doubt that the International Commission on Zoological Nomenclature should act at all upon problems having to do with systematics. This is not a problem of nomenclature but one of systematic zoology. We believe that only workers on the systematics of Lepidoptera or upon systematic zoology can discuss the designation of a lectotype for *P. plexippus* and the identification of this species.

Third, the designation of an illustration published by Holland in 1931 as the type of *Papilio plexippus* Linnaeus, 1758, is one consequence of work by people like Hemming who do not practice systematic zoology. This kind of action is a "new" systematics; it appears again in Hemming's mail proposal. There we are told to base *Papilio plexippus* Linnaeus on a figure by Clarke, published in 1941—a figure of an insect collected in Kendall, New York, and we have to say that the type locality is the state of Pennsylvania!

We were, indeed, very much surprised to see such statements in Hemming's mail proposal. We here in Brasil strongly protest against this kind of systematics—the designation of a figure not seen by Linnaeus as the type of an insect described by him, when there still exists in the Linnaean collection a specimen of this insect that was seen and labeled by Linnaeus. To designate a figure "as the standard for identifying" (Hemming's own expression) really amounts to a designation of a type[1] for the species and subspecies. To designate a figure based upon a specimen from Kendall, New York, and at the same time to say that the type locality is Pennsylvania shows a real and obvious ignorance of what is meant by the term "type locality."

We must also say concerning footnote 5 on page 70 of the Field, Clarke, and Franclemont paper that one of us (Almeida) received Hemming's mail proposal. It was received, however, after the date specified in their paper (i.e., December 10, 1950). Hemming's letter is dated October 31, 1950. We have not checked the date it was posted, but apparently there was some postal delay.

Finally, we want to state that we agree with the conclusions set forth by Field, Clarke, and Franclemont, and we also request (as they did) that the commission reconsider the whole matter of fixing the name *Papilio plexippus* L.

We have discussed this matter with some of our colleagues who work on systematic zoology in scientific institutions in the cities of Rio de Janeiro and São Paulo. We wished to learn their opinions about the way Hemming was trying to solve this question of *P. plexippus,* because it involved not only matters of interest to lepidopterists, but also matters of interest to all systematic zoologists and with implications about which all right-thinking systematic zoologists should be warned.

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After a careful discussion of the paper above, the undersigned agree *in toto* with the views therein contained.

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[1] We realize that Hemming has not used the word "type" here but uses the phrase "the standard for identifying." We interpet this expression (as did Field, Clarke, and Franclemont) to mean "a type" and, indeed, can see no other meaning. Nevertheless, we would not be surprised to hear from Hemming that in his new systematics this expression does not mean a type but some other thing.

Mathematics and Science

ALTHOUGH the authors of three communications (*Science,* **112**, 233 [1950]) take issue with some of my statements (*Science,* **110**, 566 [1949]), they do not try to controvert my contention that the theory of probabilities is very useful in applying principles for successful prediction, but not in discovering them.

In stating that "disordered systems can be specified with the same degree of precision as ordered systems," John C. Neess surely cannot mean what the words imply—that greater knowledge does not permit greater precision in specification. Does disorder mean anything more than that we do not yet grasp the order, perhaps very complex, that there may be in a situation? He rightly refers to "the confused atmosphere of du Noüy's *Human Destiny,"* but his statement is reminiscent of du Noüy's extraordinary conclusion (p. 26) that *"order is born of disorder."* He states that we "have removed a barrier to intellectual and scientific progress" by replacing "an older notion of causality" "with one of chance determination of events." Does "chance determination" mean anything more than that we don't know how the events have been determined? Arguments based upon ignorance are suspect. The "indeterminancy" of an electron represents the continuing ignorance of the investigator (H. N. Russell, *Science,* **27**, 249 [1943]) and is surely meaningless as to the character of the thing investigated, except as limited by our relations with it. "Relativity" expresses this limitation for man. When one of its leading exponents (Eddington) argues: "What we can't know doesn't exist," he should add "in us" or "for us." If he is logical, anyone who accepts this idea without the qualification is sure to founder on the rock of solipsism, since he must finally conclude