

Lepidophyma; he supports the hybrid theory for many species of *Cnemidophorus* (3, 4). These references and many others are included in my review (11) that supports both the hybrid and nonhybrid modes of evolution of parthenogenesis in reptiles. Although Cuellar received the reprint of this review that I sent to him in 1975 (12), his recent article (1) did not acknowledge the dual philosophy of the other workers.

Readers who need accurate information should refer directly to the primary literature, following Cuellar's suggestion: "For the sake of clarifying what is inherently a highly complicated biological problem, future workers theorizing on this subject should strive to be meticulous in search of the facts" (1, p. 841).

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Concerning my statement that bisexual *Cnemidophorus lemniscatus* are not found in the Amazonas River, it was, unfortunately, my impression, at the time, that the bisexual populations Vanzolini (1) alluded to were either rare or had been replaced by the unisexuals. Regarding his comments (2) on distribution I have no doubt, as he states (1) that *C. lemniscatus* occurs predominantly in urban and periurban environments. My concern is how it is colonizing such environments where, according to Vanzolini "cities are separated by forests in a series of nuclei of very dense populations separated by broad areas where density is zero or negligible . . ." I merely added to his guess that *C. lemniscatus* appeared to be expanding its range along the main course of the Amazonas and was proceeding westward (1). By uniform distribution I did not imply high density but simply presence in the natu-

ral communities. This view does not really differ from his own. For instance, he stated that more research was needed to show whether the present geographical distribution of unisexual populations resulted from the spread of a single original strain with superior powers of migration and selective advantages as compared to local bisexual populations, or whether unisexuality was evolving locally. He suggested that gradual expansion along the deforested banks was a more probable mechanism than was rafting and roads, which he excluded. He also added that they had not yet found the species in farms and pastures. In reference to such comments I stated (3), "adequate sampling will probably reveal a uniform riparian distribution from which adjacent towns draw their unisexual populations." Recent tissue grafting studies (4, 5) in some North American *Cnemidophorus* indicate that large populations or entire species may be isogenic, suggesting their evolution from single individuals rather than from multiple origins. Similar studies, if conducted on *C. lemniscatus*, may reveal much on the evolution of this most interesting unisexual species.

In answer to Cole's remarks (6) that I did not cite certain papers because they would contradict my own divergent philosophy regarding the hybridization theory, I should say that whenever I deliberately omitted a paper I did so only because I did not consider it sufficiently original or relevant to a general and theoretical review of the ecology of animal parthenogenesis. My article (3) was not intended to be an exclusive review of lizard parthenogenesis nor of the hybridization theory. As far as that goes, I suspect that I omitted at least 50 papers dealing in one way or another with parthenogenesis among insects and other invertebrates. I find it paradoxical that Cole should raise the issue of omission since his own review (7) omitted the one critical paper questioning the fundamental basis of the hybrid theory (8). Although White (9, 10), who previously upheld the spontaneous theory, has now convincingly established a hybrid origin for the parthenogenetic grasshopper *Warramaba virgo*, he does not ignore, as did Cole, the cytogenetic difficulties inherent in this mode of origin.

Wright's (11) comments largely deal with cytogenetic and biochemical questions rather than ecological ones. Therefore, his comment would be more appropriately discussed in light of my article (8) dealing with the cytogenetic factors on the origin of parthenogenesis, and I suggest that this article provides clarification of Wright's comments. I should

clarify a misconception in both Wright's and Cole's remarks concerning my views on the role of hybridization in parthenogenesis. In no instance have I denied the hybrid nature of parthenogenetic organisms. I have indeed questioned the cytogenetic relationship between hybridization and parthenogenesis (6), but not, as is claimed, the existence of parthenogenetic hybrids. The correlation between parthenogenesis polyploidy and hybridization has long been recognized in both plants and animals (12) and I have emphasized this point previously (3, 8). The essential question is not whether parthenogenetic organisms are hybrids, but whether hybridization results directly in parthenogenesis (8). According to White *et al.* (9, 10) it is extremely difficult to conceive that hybridization would automatically cause major modifications of the maturation divisions or that an act of hybridization and a cytological macromutation would happen simultaneously. Thus "the cellular causes for this remain mysterious" (9).

The fact that technical comments were prompted by this issue is good evidence of a controversy, and controversy is a healthy practice in science, particularly when the questions are mysterious, as they are in the evolution of parthenogenesis. Wright has played a major role in exploring this difficult problem and his contributions have undoubtedly influenced my own ideas. As I now see it, it was precisely this clashing of viewpoints that has set the stage for future workers to focus on precise issues that will ultimately unveil the enigma of parthenogenesis. Evidence that this may now be in progress is provided by the discovery of White *et al.* (9) that "a very few species-hybrid combinations spontaneously manifest premeiotic doubling and it is precisely these which have given rise to the parthenogenetic species."

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Parthenogenetic Lizards

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