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COMMENT

Reply To Pilling

THOMAS BLACKBURN

I must confess that I am somewhat disconcerted to find that Arnold Pilling has apparently joined the small but select group of senior Californianists who have recently become afflicted by chronic (and possibly terminal) intellectual dyspepsia. I can find no other logical explanation for either the tone or the content of Pilling's (1978) review of *Flowers of the Wind*, a review that is replete with the kind of acerbic *non sequitur* that can drive even the most patient author or editor to drink. Pilling states that our work is characterized by "parochialism and/or *ivory-towerism*," and offers in evidence the fact that there was no citation of the "Yaqui data" collected by Carlos Castaneda, nor any comparisons with "Australian and/or Tibetan accounts of shamanism." The last criticism might have been relevant if any of us involved with the book had been even remotely concerned with the topic of the "common origin and great antiquity of shamanistic beliefs and practices"—but such was not the case. The reference to Castaneda's work I find somewhat amusing in light of Ralph Beals' recent comments (1978) on the subject; however, Pilling may consider Beals' work "parochial" as well. I suppose we should at least be grateful for not being chided for our failure to cite such other notable contributors to the same genre as Erich von Daniken, Barry Fell, James Churchward, or Immanuel Velikovsky.

I detect more than a trace of intellectual arrogance in Pilling's strictures regarding the

implied failure of the several scholars who contributed to *Flowers of the Wind* to address substantive issues. Thus Pilling is "startled at the lack of reference to contemporary Native California religion." He is rightly indignant that "little is even said of the recent dynamics of Native American ritual and belief," and that "dates are given to almost no events, making analysis of change difficult, if not impossible." I am in total agreement with Pilling regarding the legitimacy and importance of such concerns, but must respectfully point out that they did not happen to be a major focus of this particular book. To criticize an author or editor for failing to address issues of personal interest to the reviewer is scarcely fair, and can in fact contribute to a serious abrogation of a reviewer's primary responsibility. In a recent editorial on book reviewing in the *American Anthropologist*, Richard Woodbury raised a number of points that seem especially apposite to the case in hand:

If a single word were to summarize the role of the reviewer, it would be "responsibility." Reviewers accept the heavy responsibility to their colleagues to represent a book fully and fairly, balancing the author's aims against the results (without taking "the author to task for not writing the book the reviewer would have liked to write," as the victim of an unfair review recently wrote to me). The readers of a review need to know the book's scope and purpose, its relation to other works on the subject, its limitations (both intentional and unintentional), strengths, weaknesses, special viewpoint or bias, and its possible

usefulness for various purposes [Woodbury 1977:551].

In conclusion, I would like to endorse Woodbury's sentiments, and urge anyone who is involved in the reviewing process to approach the task honestly and *responsibly*; I also hope that, in future, the staff of the *Journal* will apply the same high editorial standards to the selection of book reviews (and reviewers) as are presently employed in the evaluation of articles.

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On the Correspondence Between Villages and Wetlands in the Great Basin

MARGARET M. LYNEIS

Robert Bettinger (1978) argues that adaptive strategies in the Great Basin cannot be explained as the product of differences in local environment. In two of the five cases he examines, he finds no correspondence between environment and adaptive strategy. He is mistaken, however, in his analysis of one of the

two cases, that of the Warner Valley subsistence network in south-central Oregon (Weide 1974). As a result, he muddles the relationship of settlement-subsistence systems and environmental variability in the Great Basin. He creates a false problem where none exists, a mystery of discordance between environmental potential and evolutionary adaptation.

Bettinger describes five cases: prehistoric settlement-subsistence systems from the Owens and Surprise Valleys in eastern California, the lower Humboldt and Reese River Valleys in Nevada, and Warner Valley, just north of Surprise Valley in Oregon. He proposes a two-category classification of prehistoric adaptive strategies in the Great Basin, a Desert Culture strategy and a Desert Village strategy. Bettinger classifies the Reese River and Warner Valley cases as examples of the Desert Culture strategy, placing the remaining three systems, including Surprise Valley, in the Desert Village category. Recognizing the general environmental similarity of Surprise and Warner valleys, Bettinger puzzles over the supposed dissimilarities in their adaptive strategies, and strays into speculation concerning separate cultural-historical origins for the two adaptive strategies.

I identified the Warner Valley as a settlement-subsistence network which included annually reoccupied, permanently situated lowland winter villages. This semi-sedentary settlement system was adapted to the permanent lakes, streams and extensive sloughs and marshes of the valley floor. The Warner Valley case is an excellent example of correspondence between environmental possibilities and more sedentary lifeways, contrasting with models of Great Basin settlement patterns derived from Julian Steward's work (Weide 1968).

Bettinger metamorphoses the Warner Valley case in the course of his discussion. He begins by properly characterizing the Warner Valley settlement-subsistence network as consisting of three settlement types, one of which is