

Communications to the Editor

TO THE EDITOR:

I read with great interest Chuang and Wolf's account of "Marriage in Taiwan, 1881–1905" (*JAS* 54.3 [August 1995]), waiting to see if their model of hot marriage market/prosperity would take account of an explanation of bride adoption in which I have a minor vested interest, having personally espoused it in a hopefully soon to be published book manuscript (Cooper in press).

Chuang and Wolf are extremely humble in characterizing their effort as belonging to a first stage of inquiry, in which accepted explanations are disproved, and substitute hypotheses offered. They postpone to a later time the second stage in which decisions will be made as to which assumptions come closest to capturing the truth about Chinese society. In the spirit of contributing in a positive way to the crafting of a truthful perspective, I offer some thoughts derived from recent fieldwork carried out in Dongyang County, Zhejiang Province.

The data for Dongyang are no match for the Japanese records for Taiwan that Chuang and Wolf employ in their hypothesizing. I have only the testimony of a compendium of folk custom *Dongyang Fengsu Zhi*, compiled from past editions of the county annales *xian zhi*, edited locally, that the adoption of child daughters-in-law was common in Dongyang in traditional times, with no statistical frequencies noted.

The modernist commentary of the *Fengsu Zhi* editors provides several practical explanations for the custom in Dongyang. The first of these suggests that to give away a daughter as a child daughter-in-law, was to be forced by circumstances, a last resort, a means of overcoming poverty. In addition, the custom is characterized as a means of providing additional work hands in the family, of obtaining a wife for a youngest son, or of acquiring a wife for a son with a physical deformity! These are precisely the kinds of mechanistic formulations that Chuang and Wolf's analysis seeks to counter.

Still, how explain the custom? In my readings on the subject, Bernard Gallin (1960) provides the definitive answer. Gallin explains the custom of bride adoption as a means of creating affines when one would otherwise have to wait. Where a girl was "adopted into a family for the purpose of eventual marriage to one of their sons, from the very time of adoption the two families assumed a *qinqi* [affinal] relationship" (Gallin 1960, 636).

This, it seems to me is the gist of it.

Adopting out a daughter-in-law is an attempt to hustle along the creation of relations, both *qinqi* and *quanxi*. From a somewhat different perspective, Gell (1992, 89) would have understood the custom as a classic example of making things happen so as not to be overtaken by events, of moving things along, all within the system, just getting a head start in time.

Indeed, Chuang and Wolf seem poised to accept just such a formulation when they conclude that "minor marriages were just major marriages *timed* to take account of the bride's age" (p. 793 emphasis added). But then, if this conclusion is accepted, the category "minor marriage," which if my memory serves me correctly was introduced into the literature by Wolf and Huang (1980), would seem quite

superfluous. Especially given the “delayed transfer” marriages discovered by Janice Stockard in Guangdong (1989) which represent an inflection in timing in the opposite direction, and complete the continuum as it were. All the regional diversity can be understood to represent differences in *degree* of divergence in timing, rather than in kind of structure involved. The diversity conceals a greater unity.

The Gallin/Gell explanation affords a certain validity to the folk wisdom that the custom was associated with poor families, those most in need of a jump start in the creation of affinal networks, especially where no sons were present on whom to build such relations in the future. There’s a hypothesis that could probably be tested.

Chuang and Wolf use the example of the poor family that did not arrange minor marriages for three generations while poor, and only after prospering showed a marked preference for raising their sons’ wives. But this does not disprove the poverty hypothesis, since in China it was common for wife takers to be more prosperous than wife givers (the tendency for female hypergamy). It would still be to the advantage of the poor to “deploy” their daughters as early as possible in the creation of affinal networks, into the families of more prosperous wife takers willing to raise their sons’ prospective wives. Bearing in mind that it is the poverty of the wife giver that motivates adopting out a daughter-in-law, gives some further credence to the folk explanation.

It also suggests that the custom might not be associated with high per capita income so much as with a highly polarized class structure, daughters of the poor entering the market earlier and earlier as poor families vied to create affinal links as a means of coping with or pulling themselves out of poverty.

The scattered occurrence of the custom in other parts of China, like Dongyang, was therefore more likely a manifestation of individual efforts on the part of local families to hustle along the creation of affinal alliances, and less likely the institutional debris of cooled down previously hot marriage markets.

It seems to me that Chuang and Wolf’s market analysis can be improved upon by giving added attention to the enduring relations between families effected by transactions in women. It is Gallin’s contribution to have emphasized the lifelong exchanges between affines that marriage merely initiates.

It’s not that there wasn’t a market in women. There clearly was. But what made it “hot” could well have been competition to secure “ahead of schedule” *qinqi*.

Put that daughter to work in the creation of alliances.

Too young? Don’t wait. Get a head start. Adopt her out!

Or as was traditionally said of all brides in Dongyang,

“Sell her out” *ba ta mai chu qu*.

These considerations require that we rephrase our question: What sorts of circumstances would put a premium on securing affines early? And such a perspective in turn may require that we come up with new or revised hypotheses.

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TO THE EDITOR:

Cooper argues that minor marriages are best understood as a way of achieving the benefits of affinal ties sooner rather than later. People gave away their daughters and raised daughters-in-law in their places "so as not to be overtaken by events, [to move] things along . . . [to get] a head start in time."

Cooper attributes this explanation of minor marriages to Bernard Gallin and cites as evidence Gallin's observation that from "the very time of adoption the two families assumed a *chin-ch'i* [affinal] relationship" (1960, 636). But why, if minor marriages gave people a competitive advantage by "hustling along the creation of affinal ties," were they relatively rare in Central Taiwan where Gallin did his field research? And why were they three times as common in the Taipei Basin? Will Cooper accept the implications of his argument and hypothesize a greater need for affinal ties in Northern Taiwan than in Southern Taiwan? Why, in his view, should this be the case?

Arthur Wolf and Hill Gates have in press a paper that sharpens these questions by comparing minor marriages in Taiwan and Szechwan. This comparison shows that three of the essential characteristics of minor marriages—their frequency, their relationship to social class, and the age at which the bride was adopted—are closely correlated. Where the frequency was low (less than 25 percent of all first marriages, as in Southern Taiwan, Szechwan, and North China), age at adoption was late (averaging more than five years), and the relationship to social class was strong (with only the poor choosing to raise their sons' wives); where the frequency was high (more than 25 percent of all first marriages, as in Northern Taiwan, Southern Kiangsi, and large parts of Fukien), age at adoption was early (averaging less than five years), and the relationship to social class was weak (the wealthy choosing to raise their sons' wives almost as frequently as the poor). Thus, to maintain his argument, Cooper must eventually show that in some parts of China only the poor were motivated to extend their affinal ties as early as possible and that even they did not give their daughters away as infants, while in other parts of the county almost everyone was motivated by these considerations and so strongly motivated that a large proportion of all female children were nursed by their future mothers-in-law.

It could be that the frequency of minor marriages and the advantages offered by affinal ties covaried in just this way. We do not deny the possibility. We only want to point out that Cooper's explanation entails an as yet untested assumption. Our more substantial reason for not giving affinal ties the attention Cooper feels they deserve is that there is ethnographic evidence suggesting that minor marriages did not create reliable social relations between the families of the bride and groom. At one point in his account of Kaihsienkung (in Southern Kiangsu) Fei Hsiao-tung notes

that in minor marriages the “affinal relation is very loose, and in many cases entirely eliminated,” and at another he notes that the minor form of marriage is “despised” because “its effect in loosening ties of affinal relationship, affects the normal working of the kinship organization” (1939, 54–55). Moreover, in his study of marriage and the family in Singapore Maurice Freedman found that “most ‘little daughters-in-law’ have this in common, that in accepting a bride-price for them, their parents largely abandon them to their ‘adopters’ and refrain from interfering with them. In other words, the relations between parents and the daughters they give away in this system (despite its terminological assimilation to marriage) are not those between parents and married daughters.” The not very surprising result is that in Freedman’s experience “*shim pu gia* [minor] marriage does *not* involve significant social relations between families” (1957, 65).

Similar evidence is cited by Wolf and Huang (1980) and by the contributors to two special issues of the *Minzoku Taiwan* (1943). It is not sufficient to conclude that Cooper’s affinal ties hypothesis is mistaken, but it is the reason we prefer our version of the marriage market hypothesis. We hope Cooper will try to show that the advantages offered by affinal ties varied from locality to locality and that minor marriages were most common where those advantages were greatest. We can then decide which of the two hypothesis offers the best solution to the problem posed by minor marriages.

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TO THE EDITOR:

The review of my book *The Last Nizam: The Life and Times of Mir Osman Ali Khan* by Karen Leonard in *The Journal of Asian Studies* (54.2 [May 1995]) recently raises certain issues which to my mind go beyond the simple question of the contents of a review, and its adequacy as an evaluation of the book in question.

I am surprised by the assumption of the book review editor of *The Journal of Asian Studies* that only a person with knowledge of the state of Hyderabad is capable of

reviewing a book on the Last Nizam, a figure who is central to the understanding of the entire Indian states' problem in the twentieth century. One might as well as say that a book on Kashmir should be reviewed only by a Kashmir specialist, or, a book on Junagadh by a Junagadh specialist. A quick glance through the contents of the book would have made it clear that it deals with major all-India issues, not only in the concluding chapters, which deal with the events leading to the Indian police action of September 1948, but also in earlier chapters. There are references to Aurangzeb, Nadir Shah, Tatyá Tope, Queen Victoria, and others in the first chapter; to Shah Waliullah and Sir Muhammed Iqbal in the second chapter; to the Caliph of Turkey; the Swadeshi movement, the Congress, and the Hindu Mahasabha et al. in the third chapter. There are 33 references in the index to Mohammed Ali Jinnah, 21 to M. K. Gandhi, and 15 to the Arya Samaj, to give only a few examples.

The reference of the book to this particular reviewer appears even stranger when it is noted that I had objected to the tone of a review by her of my earlier book *The Nizam between Mughals and British: Hyderabad under Salar Jang* (S. Chand and Co., 1986). My objection had been published in *The Journal of Asian Studies* along with her reply.

Assuming that a specialty in the history of Hyderabad state exists, it is questionable whether the reviewer is such a specialist. So far as I am aware, she has published some important articles on Hyderabad history in *The Journal of Asian Studies* and other journals in the 1970s and the early 1980s. Her book *Social History of an Indian Caste* (Delhi: Oxford University Press) is to me a work of social anthropology, and does not raise or answer any major questions of history. The same can probably be said of her book on the Punjabi-Mexicans in California, which I have not seen, but which appears to be a part of American ethnic studies, rather than that of Indian history.

Given this background, it is perhaps not surprising that the review takes no stand on the character of the Last Nizam or his role in history. Not having undertaken archival research on Hyderabad state for over a decade, the reviewer falls back on vague generalization and other dicta such as the statement early in the review, that the book "fails to make a clear and well-supported argument about the Nizam's place in history."

Most other reviewers have stated just the opposite. Professor Tariq Rahman of the Qaid-e-Azam University, Islambad, refers in his review (*The News*, Lahore, 25 March 1995) to "the achievement of Bawa in having written an authentic biography of a man about whom there were only anecdotes and fables. For many years to come—perhaps forever—we shall see Mir Osman Ali Khan, the last Nizam of Hyderabad, through the eyes of V. K. Bawa. And may I say that these are eyes which do not seem to be tainted by bias. There cannot be a better recommendation for a biography."

B. P. R. Vithal, a member of the last Finance Commission of India, says in a letter: "You have written a remarkably good book. I learnt much that I did not know. It confirmed many impressions I had carried from childhood memories. . . . All in all I agree with your assessment of the man. You got the right balance. The book is so well-written that it was a pleasure reading it, even where its subject was not."

Neither of these persons could have qualified as a reviewer for *The Last Nizam* in *The Journal of Asian Studies*, as neither is a 'Hyderabad specialist,' though one was born here, and another is a scholar from a neighboring country. When I asked Dr. Leonard to read the manuscript, I was aware that she had a longstanding connection with many old Hyderabad families. I only wish she had not allowed her love for the Last

Nizam's Hyderabad to override her objectivity in judging a book she was asked to review for an academic journal.

V. K. BAWA

TO THE EDITOR:

I am sorry to read Dr. V. K. Bawa's reaction to my review of his book. He clearly would have preferred a different reviewer and a different kind of review. I undertook to discuss his important book conscientiously and objectively, and I believe I did so. And I did take a position on the Nizam, saying that he remains an enigma.

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