

Review

Reviewed Work(s): Hired Labour and Rural Labour Markets in Asia: Studies Based on Farm-Level Data by S. Hirashima and M. Muqtada

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Book Reviews

Hirashima, S. and M. Muqtada, eds., 1986, *Hired Labour and Rural Labour Markets in Asia : Studies Based on Farm-Level Data*, New Delhi, ILO—Asian Regional Training and Employment Programme, pp. 180.

This volume assembles a set of four reports based on primary surveys commissioned by the ILO and conducted in 1982-83 in Bangladesh, Pakistan, Philippines and Thailand respectively. The principal objective of the exercise was to understand the nature and pattern of the rural demand for hired labour, although the surveys were cast more broadly to cover the larger labour utilisation context in which the demand for hired labour is embedded. The survey design called for a sample, consisting of both landed and landless households, to be selected from each of three areas or zones: one representing low agricultural growth (called LAG), another representing high agricultural growth (called HAG), and a third with proximity to industrial employment opportunities (called NIC). Beyond this there is little uniformity to the four in design, nor in the quality of the resulting reports. Those on the surveys conducted in Pakistan, Philippines and Thailand, in particular, are often confused and internally inconsistent.

By contrast, it was a pleasure and relief to read the report on the Bangladesh survey. The paper is coherently written, so that at all times the intentions and objectives of the authors are perfectly clear. Labour hire in the three areas studied seems to have been confined largely to casual labour, with permanent and contract labour not apparently of much importance. If that is so, however, then surely a major shortcoming, for a study explicitly focused on rural labour markets, is the lack of any effort towards identifying variations in contractual types *within* casual hiring. The operation-wise wage-data presented are reported to be the "standard" rates prevailing in the area; however, there are also some accompanying, albeit tantalisingly brief, references to wage variations by duration of contract (p. 43), leaving the reader wondering whether the wage rates reported are for a full-day contract, or a weighted mean of part and full-day rates, and if the latter, whether the weights vary across agricultural operations. The report asserts that female participation is negligible except in post-harvest operations (p. 33). If true, that is very surprising indeed for a region where rice-culture dominates.

The general disregard of wage data in a set of studies aimed at understanding the rural labour market is very noticeable. The Philippine study quite simply does not report wage data at all, perhaps because of a stated but unsubstantiated assertion at the outset that the rural wage rate in Philippines is not market-clearing. The Pakistani study does report wage data, but in one of the many instances that can be found in it of misplaced emphasis, sex-wise data are presented only for permanent contracts (where female hire is infrequent, and only for domestic work). As for daily hire, no sex-wise figures are given of work-participation rates in landless households although, in yet another instance of misplaced emphasis, such details are given for landed households in the sample (where female involvement is expectedly negligible). However, there are references in the text to female participation in rice operations and also in "low paid casual wage employment" like groundnut digging, but no data on wages received by females as distinct from wages received by males in these types of work. Do we assume then that the operation-wise wage data reported apply uniformly regardless of sex, and that, by extension, uniform contracts are offered to males and females? The Thai study does report wage data by sex, but the tabular figures are strangely at variance with the following sentence which refers to the entire Chiang Mai region in which both LAG and HAG were situated: ".....the daily wage rate of one thang of unhusked paddy for one man-day of labour is the common practice and has remained unchanged for many decades regardless of changes in paddy price. Wages paid in cash is (sic) also practised but not as widely" (p. 171). If the prevalent wage rate has been fixed in kind for decades, why is the female wage-rate so much below the male? Is that because females work less than a full man-day? And why does the male rate itself vary from LAG to HAG, both of which lie in Chiang Mai? Is it because of variations in the paddy price between LAG and HAG?

Work done for India indicates that variations in contract type, and correspondingly wage received, both by sex and across individuals within each sex, can be quite considerable (Rajaraman, 1986). There is even reported evidence from India of large wage differentials between *adjoining* villages not dissimilar in agro-climatic and other respects (see Bardhan and Rudra, 1983; Bliss and Stern, 1981, and Rajaraman, 1987). The Bangladesh and Philippines surveys cannot possibly shed any light on this, since only a single village was studied in each of the three areas. The Pakistani and Thai surveys did include more than one village in some zones, but neither report looks at inter-village wage differences. All of this leaves one with the uncomfortable sensation of a golden opportunity sadly missed. Some of the more interesting aspects of such wage infor-

mation as is presented in the Pakistani report are not even remarked upon by the authors, such as for example the inverse relationship in the table on p. 104 between operation-specific wages for casual hire and size of farm. This is most startling; could it have arisen because some of the kind payments made by the larger farmers, like meals for example, may not have been adequately accounted for by the authors?

A redeeming feature of the Philippines study is the section covering the evolution of new contractual arrangements in rice harvesting—the *gama*, *prendes* and *bahig* contracts. Here again, the authors do not distinguish as carefully as they should between *gama* and *prendes* on the one hand, which involve an implicit reduction in wage per task and are clearly a market response to increasing labour supply relative to demand, and *bahig* on the other, which does not imply any fall in the wage rate and seems to have evolved at a time of rising demand relative to supply in Area 3. The subsequent disappearance of *bahig* with the collapse of the sugar market bears out the difference, but the section is so poorly written that there are passages which, as they stand, are mutually violently contradictory.

It is interesting that the *gama* contract developed even in Area 1 (NIC), where there are opportunities for employment in construction because of proximity to Manila (most unfortunately referred to by the authors as the “primate city”). In general, the impact of non-farm employment opportunities upon the rural labour market, which underlay the provision for a NIC area in the study design, is not sufficiently explored in any of the reports. As may be expected, it is only the Bangladesh study which explicitly addresses the issue, and concludes that aside from construction opportunities (similarly to the Philippines case), employment in the industrial centre was taken advantage of by outsiders with the necessary skills or connections. In the Pakistan study, it is the NIC zone (B) that paradoxically has the lowest rate of off-farm employment, and zone C (HAG) which has the highest (table 3.10). The authors ascribe this to the domination of zone B by large farmers offering farm employment, and C by small farmers. Non-farm income in zone C is reported to result from traditional artisans having made the transition to commercial activity—cobblers to shoemaking, carpenters to furniture-making—but the all-important issue of who provides the marketing and credit infrastructure, is left unspecified. (The Thai NIC is the simple case of cottage industry with access to a well-known tourist centre).

Considerable attention is paid to the issue of labour utilisation by crop, with the Bangladesh and Thailand studies in particular delving into variations by rice variety. This is indeed an important policy issue.

Even here, however, there is that curious failure to look at labour utilisation by sex. In rice culture, in particular, where the association of agricultural operation by sex is so marked, it would have been useful to have had data on the impact of HYVs on labour demand by sex—more than useful, vitally necessary, given the now-acknowledged centrality of women's economic status in the development process. The sample size in all the studies was large enough to accommodate quite easily a splitting of labour demand by sex. Once again, there is a regretful sense of a lost opportunity. In rare instances where male-female breakdowns of labour use are provided, such as table 5.9 for example on Thai rice production (consolidated across operations), what those percentages are is anyone's guess; the figures do not add up in the way the accompanying text suggests they should, and missing footnotes make it impossible to tell which crop variety each column refers to.

The Pakistani study estimates a formal demand function for hired labour. The coefficients accord with prior expectation, although sentences such as the following leave one in some doubt as to what the authors mean to convey: "Various other factors are also brought in to seek the explanatory power of the econometric model" (p. 77). The cropping pattern variable, perhaps the most important from a policy viewpoint, is defined simply as "% of labour-intensive cash crop" (p. 108), which indicates prior selection and leaves the crop in question unspecified (there is a list in the text, however, which excludes only wheat, among the crops grown in the area, but strangely includes maize). The section in the Philippines report on labour utilisation by crop leaves the reader totally exhausted from the effort to make sense of it. The authors conclude (p. 148): "The type of crop partly determines demand for labour. Between sugar and rice versus rice, the former has a high hired labour requirement". But it is clear from the diagrams of p. 125 and table 4.2 that it is Area 1, with rice monoculture but advanced production techniques, rather than Area 2, with its crop diversity, which has the highest demand for labour, and the lowest seasonal slack. The advanced production techniques of Area 1 might even *require* monoculture; the authors suggest this with their earlier statement (p. 126): "The predominance and the state of rice agriculture in Area 1 enables its cropping pattern to provide the greatest level of employment opportunities among the three case villages." How, then, is the conclusion about the superiority of cropping diversity justified?

Whatever the limitations of this study may have been, in terms of design and approach, the published report should at the very least have had clarity of presentation, so as to hold the interest of the average policy-maker at whom it is directed. Not many readers will have the patience

to fight their way through a tangled mass of data, poorly presented, and opaque prose peppered with printing errors, to search for information and conclusions of academic and policy consequence.

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Baru, Sanjaya, *The Political Economy of Indian Sugar*, Oxford University Press, New Delhi, 1990, pp. XIV+225, Price Rs. 175/-.

The slim book under review is a revised version of doctoral thesis prepared at the Centre for Development Studies, Jawaharlal Nehru University. It is an important contribution to the slowly growing literature on sectoral studies of the Indian economy. The title faithfully reflects the contents of the book. The aspects of political economy are discussed in the historical context. Both the narrative skill of the author and insights the study throws up are the striking features of this study. While the book is passionately written. There is much substance in it. Evidence is carefully articulated and arguments are persuasive. In contrast, econometric studies generally miss historical perspective as well as contextual framework of political economy. Ideology is useful as long as it does not become a dogma. Since it is not possible to go into details of the various aspects covered, I shall touch only on some selected aspects.

The emergence of the Sugar Syndicate in the Thirties and links with the Congress Party and the role it played is ably portrayed. The archival evidence contained in the correspondence between the late Rajendra Prasad