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MEASURING TIME ALLOCATION, DECISION MAKING
AND AGRARIAN CHANGES AFFECTING RURAL WOMEN:
EXAMPLES FROM RECENT RESEARCH IN INDONESIA

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SUMMARY

In small-scale research on rural women a combination of quantitative and qualitative approaches can serve as a check on the biases inherent in each of them. Quantitative data in themselves can rarely explain anything, but they have important uses in the forceful challenging of conventional ideas on rural women and in raising new questions for more detailed study. Examples are given from recent research on the allocation of labour time by men and women, the relative participation of husbands and wives in decision making, and changes in modes of female farm labour recruitment and payment.

Bio-data

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MEASURING TIME ALLOCATION, DECISION MAKING AND AGRARIAN CHANGES AFFECTING RURAL WOMEN: EXAMPLES FROM RECENT RESEARCH IN INDONESIA

Benjamin White

These notes describe attempts to provide reliable measurement of three important aspects of the lives and work of rural women in various research projects in Java in the 1970s.¹ The first concerns the simple but basic question of how men, women and children use their time; the second, the relative influence of wives and husbands in decision making and the third, changes in modes of labour recruitment and payment in rice harvesting, one of the most important sources of seasonal income for rural women in Java. The examples are all taken from studies of single villages or small samples of villages that can be undertaken by small teams of researchers, rather than large-scale surveys covering representative samples of the entire population such as are normally undertaken by national sample survey agencies. I hope they may serve to highlight the benefits of combining quantitative measurement with the qualitative insights made possible by small-scale research at community level, involving at least some period of residence in the village.

TO MEASURE OR NOT TO MEASURE?

'If I use qualitative data, I believe it but noone else does; if I use quantitative data, everyone believes it but I don't'

It may be useful to begin with the problem highlighted by this remark,² or more generally with the unfortunate and often misconceived tension between 'measuring' and 'not measuring', between quantitative and qualitative approaches in field research. Both approaches, I think, are about equally subject to mystification and both present equally dangerous possibilities of the arbitrary imposition of order on a complex and confusing world. Hostility towards quantitative work is often justified because while laying claim to objectivity it is so often used to measure the wrong things in the wrong ways, to conceal all kinds of real-world ambiguities and complexities behind the neat facade of tables and graphs and to squeeze the resulting data into unrealistic models which either distort or certainly do not increase our understanding of people's lives and problems in any important way. Meanwhile those who cleave firmly to the superiority of qualitative methods often ignore the fact that months or years can be spent living in villages,

'participant-observing' and recording whatever is seen and heard in volumes of field notes, only to emerge with confirmation of whatever preconceived notions had been brought there in the first place, thanks to the extraordinary capacity of the human mind to see and hear only what it expects to, resolutely suppressing the 'noise' of contradictory instances.

Nor do I think too much attention should be paid to the view that quantitative approaches are in some way essentially more reactionary and qualitative approaches more progressive in orientation. Such notions confuse 'methodology' in the broader sense (including also the conceptual frameworks and assumptions which determine the goals and objects of observation and measurement) with 'techniques' or 'approaches', the latter being simply information-gathering instruments that may be employed for either reactionary or progressive purposes (cf. Bryceson 1980).

The problem, surely, is not to set the two approaches in opposition and stand on one side cheering, but to find more fruitful ways of combining them. Some types of quantitative measurement can provide important checks on the biases inherent in qualitative work; equally, while measurement can often show us that things are not as they seem, it is sometimes only qualitative research that can help us explore the new questions raised by the results of measurement. A further use of quantification is rarely understood. Research is a process not only of learning, but also of communicating what we have learned to others (something that researchers are not always good at): not only to those who see the world in the same way but also - and far more important ultimately - to those who see the world in different ways and need to be convinced. One way of doing this is to present reliable quantitative data, in simple and forceful ways. In many kinds of social research, and particularly those which find themselves in an environment of widespread public ignorance and prejudice (such as the study of gender-related issues) this is a more important objective of quantitative work than the testing and elaboration of sophisticated mathematical models.

MEASURING THE ALLOCATION OF LABOUR TIME IN RURAL HOUSEHOLDS

It is a curious reflection on research priorities that so little attention has been paid until recently to developing practical methods of data collection on the simple but basic question: how much work of what kinds is done by the

different members of a society? It is now widely recognised that conventional concepts of 'employment', and the methods of data collection and statistics based on them, fail to capture the realities of labour conditions of both men, women and children in many ways, for example: reproductive work (child care, housework etc.) is excluded; many types of so-called 'productive' employment are not recorded, as in the case of multiple occupations which characterise the survival strategies of both rural and urban populations in much of the Third World; even if efforts are made to record all types of work, the time devoted to each of them is inadequately measured or not measured at all; and seasonal variation in all of these is often not recorded (Benería 1981; Boulding 1983; Connell and Lipton 1978; White 1976a). What then are the alternatives?

The most reliable way to collect information on the use of time, it might be supposed, is direct observation: 'following' individuals throughout the day and recording the nature and duration of their activities. Such studies are quite rare (but see Edmundson 1976 in Indonesia, McSweeney 1979 in Upper Volta, Gillespie 1979 in Nicaragua for some examples) and limit us to a very small coverage: one researcher can follow only one individual at any time, so that either the sample of individuals or the sample of days observed (probably both) must be very small. Another problem is that of obtrusiveness: the activities of those observed will almost certainly be influenced by the observer's constant presence, the more so to the extent that (s)he is a foreigner or belongs to a different class, gender or age group than those observed. For example, in a study of the economic activities of children in Javanese rural households (White 1976b, 1976c) I soon learned that any attempt to follow children and record their activities resulted only in my being followed and observed by large numbers of curious children; hence the development of the alternative methods described below.

One way to avoid these problems while still retaining some of the advantages of direct observation is the technique of 'random instants' observation developed by Allen and Orna Johnson in a community of Machiguenga Indians in the Peruvian Amazon. Households and times of visit are selected at random and the researcher records, not a sequence of activities or durations, but the activity at the precise moment of the visit (ideally, at the instant 'just before s(he) became aware of the observer's presence'). A small number

of researchers can record a large number of these 'random instants' in a relatively short time (the Johnsons, working once a week for an entire annual cycle, collected about 3,500 'person instants' from 13 households) and together they provide a reliable composite picture of time allocation and the division of labour in the community (Johnson 1975).

Another possibility among literate populations is to persuade people to keep detailed daily records of their own activities and the time spent in each; this has been used in many industrial societies (Szalai 1972) and also in some literate peasant communities (Minge- Klevana 1977 in Switzerland; Hayami 1978 in the Philippines). This technique, however (excepting the unlikely case where people record their activities at the moment they are performed) depends essentially on the respondents' recall and is therefore subject to the same problems as the other recall procedures described in the rest of this section; its major function is to transfer part of the work from the researcher to the individuals being studied.

The 'Recall' Method: Comparison of More and Less Intensive Techniques

In many societies functional literacy has not reached the level where record-keeping by respondents is possible (but see Mencher et al. 1979 for an attempt to overcome this by the use of pictorial charts among illiterate women in India), and we are left with the alternative of interviews in which the respondent recalls, and the interviewer records, his or her activities during a specific period of time spent in each activity. Any work of this kind must compromise between conflicting demands:

- for representativeness in the sample (requiring large samples of households and individuals, especially where comparisons between different groups and classes are relevant);
- for reliable data at each interview (requiring the shortest feasible recall periods);
- for coverage of seasonal variation (requiring frequent interviews throughout the year).

While each individual study has to balance these conflicting demands, in one area (the length of recall periods) compromise is only possible with great sacrifice in reliability.

In my 1972-73 study of 'Production and Reproduction' in small-farm and landless households in a Javanese village (White 1976a, 1976b, 1976c) it was

particularly important to measure accurately the labour contributions of men, women and children in all kinds of work, both direct production and reproduction. After various experiments I decided on a procedure in which 44 sample households were visited by myself or locally-recruited assistants (all close neighbours of the sample households) every sixth day throughout the year - thus, a total of 60 visits for each household - and each household member was asked to describe his/her activities during the 24-period preceding the interview, in sequence and estimating the time of beginning and stopping each activity.³ This procedure, which involved the recording of more than 10,000 'person-days' of activity was perhaps over-intensive in one respect: the sample of 60 days per household was not strictly necessary for the quantitative purposes of my study, although it brought many extra benefits of a qualitative, case-study kind. For example, looking through individual cases over the year one can get a vivid idea of how a young boy or girl of 4 or 5 years gradually begins to make a labour contribution in the household; how a household rearranges its division of labour with the seasons (for example, men taking over child-care and cooking when women have opportunities to earn rice-harvesting wages), or to cope with the sickness, absence or death of one of its members; the relatively small disruptions in women's income-earning work caused by pregnancy and childbirth; or how an elderly landless woman was compelled to spend up to 17½ hours per day weaving pandanus-mats for sale at times when no better-paying work was available, earning barely enough to keep herself alive and with virtually no time for any other activity except a few hours of exhausted sleep.

On the other hand, I do not at all regret the restriction of the recall period to only 24 hours: experienced researchers seem agreed that the use of recall periods longer than 24 hours does not yield reliable results (Asia Society 1978: 9-13). For those who may doubt this view, it may be useful to summarise the results of another village-level study in Java in which three different recall procedures were used with the same sample, providing a rare opportunity for direct comparison of the results.

As part of a study of 'Rural Household Economies and the Role of Women' in two West Javanese villages (Pudjiwati Sajogyo et al. 1979 give a general description of the project), four women researchers collected time-allocation data using two different procedures - 24-hour and 30-day recall - from the same sample of 60 households over the same one-year period (November 1977 -

October 1978). Another research project of the Agro-Economic Survey (of which our 'Rural Women' study was an offshoot) had visited the same sample of households in one of these villages one year before, with a long questionnaire survey attempting (among other things) to obtain estimates of the household's total labour inputs in direct production during the entire 12 months preceding the interview. In the latter case, then, the data cover the year preceding the 'Rural Women' study, but no important economic climatic or other changes had intervened which might be expected to have altered the work patterns of the sample households, so that comparison of the results is still of use.

Before examining the results we should briefly consider the different procedures used in each case.⁴ In the 'Rural Women' study the researchers lived in the village for 10 days each month during the year, visiting each of the sample households monthly and asking each household member aged 10 or above about his/her activities (1) during the 24-hour period preceding the interview day (from waking the morning of the previous day, until waking on the morning of the interview) and (2) during the previous 30 days (i.e. since the previous visit). For the 24-hour recall, respondents were asked to describe how they had spent their time, from the beginning to the end of the 24-hour period, with the times of beginning and ending each activity: in other words, the complete sequential 'history' of a day's activity, including all activities both work and non-work, in the order in which they had occurred. For the 30-day recall, such an hour-by-hour account of activities over a whole month was clearly impossible, and the researchers instead used a long check-list of activities, asking each individual whether they had engaged in each activity during the previous month, and if so on how many days and for how many hours each day. The one-year recall effort of the Agro Economic Survey researchers a year previously had used a basically similar check-list technique, but in this case the questions were asked not of each individual but of the household as a whole, so that it was impossible in this case to assign the work-hours recorded to specific individuals; in comparisons involving this one-year recall effort, therefore, we must use 'household' rather than individual totals.

As may be imagined, when using the longer (30-day and one-year) recall techniques, no attempt was made to collect information on time spent in reproductive work, since the researchers were already convinced that only the

24-hour technique would provide usable results; unfortunately for present purposes, then the comparisons below cover only directly income-producing work.

Starting with a simple comparison of results of the one-year and 24-hour extremes, we can see from Table 1 that the working-hours captured with the one-year technique are much less than those reported in 24-hour recall; assuming for the present that 24-hour recall provides relatively reliable information, something between 40% (in the poorer households of Classes I and II) and 57% (in the better-off Class III households) of working-hours in direct production appear to have been missed through use of the long recall period).

(INSERT TABLE 1 HERE)

This is hardly surprising in view of the virtual impossibility of remembering with any accuracy a whole year's work in one exhausting interview session. Is the accuracy much improved by reducing the recall period from one year to one month? Table 2 compares the results of 30-day and 24-hour recall (this time for individuals rather than households, and for men and women separately) and shows that the difference, though somewhat narrowed, is still considerable: 30-day recall appears to miss about one-third of working hours, and this applies to men's as much as to women's work.

(INSERT TABLE 2 HERE)

This difference of about one-third - the difference between a 40-hour and a 60-hour working week, or a 6-hour and a 9-hour working day - is enough to raise serious questions about the value of any data-collection procedure based on longer recall periods. Detailed comparisons for specific activities (see Wigna et al. 1980) show that the differences are found in all important types of work; in rice cultivation, for example (the largest single directly productive activity of both men and women in this village) the difference was closer to one-half, in both own-farm and wage work, for both men and women. There was in fact only one common type of activity in which the two techniques produced relatively similar results: the care and feeding of live-stock and poultry, something which must be done every day and which requires about the same time every day, so that a monthly estimate of the time involved is relatively easy.

These comparisons, then confirm the view that longer recall periods lead to severe underestimates of labour utilisation. It should be noted that the 30-day and 24-hour recall techniques both required the same frequency of

interviews (12 per household per year) so that no savings in time or costs were achieved by using the longer recall period: the only advantage it offers is a spurious one, that of covering all the year rather than a sample of days over the year, but at such cost in reliability that the other technique is surely to be preferred; quite apart from the added advantages of 24-hour recall in allowing realistic estimates of time spent in reproductive work. However much we might like to expand the coverage through longer recall periods, can we expect our respondents to achieve feats of memory which we would not even expect of ourselves (how many readers of this Bulletin, even those who have 'regular' jobs - still less those whose work is not of the 9-to-5 kind, is not the same from day to day, and/or combines income-producing with reproductive work - could accurately recall the hours devoted to different categories of work over the past year, month or even over the past week?). Why is it then, that most national labour-utilisation surveys ask respondents to recall their working hours over the past week (and on that basis, classify the labour of some of them as 'underutilised by low hours of work!'), and conventional farm-management surveys often continue to base their estimates of agricultural labour inputs on recall over an entire crop season?⁵

Uses of Time Allocation Research

If time allocation studies are only reliable when relatively intensive and laborious procedures are used, are the results worth the time and effort required? I would first suggest that for almost anyone wanting to learn about the lives of rural men, women and children some attempt to observe and record how people spend their time is useful. For example, I would always recommend young students and researchers (including action researchers) to spend at least a few days, early in their fieldwork, observing directly and/or asking people of different age, sex and class how they have spent their time during the day. One learns new things in this way, and the results themselves may be less important than the chain of further questions they raise. A simple example would be the case of the elderly woman mentioned above, weaving pandanus-mats for 17½ hours a day in order to earn the equivalent of 6 US cents or 0.5 kg of rice: that she works so hard for such a miserable return is itself startling, but it forces us to ask why she has no access to work with better returns: why does a woman weaving pandanus-mats earn Rp 1½ per hour, while a man weaving bamboo mats earns Rp 3, and

a woman transplanting rice earns Rp 6-7 and in harvesting Rp 16-20? Why was she unable to save enough agricultural wages during the busy season to supplement her income during the rest of the year? Why are none of her children, other kin or neighbours supporting her, in a society which many authors have depicted as characterised by 'shared poverty'? Such questions all point us towards the kinds of relationships she is involved in, as a woman, a widow, and a landless person, which limit her access to resources and work-opportunities, which involve her in debt relations with merchants who purchase her output at low prices and so on - all of which need to be approached with qualitative, case-study work but which might not have arisen without the prior effort to measure carefully her labour time and returns.

But besides the value of this kind of work in sharpening the researcher's own awareness of living and working conditions, is there some further use in collecting such information in more systematic and laborious ways (with larger samples and regular visits throughout the year, as described above) so that the results may be used to convince a wider audience? One of the main difficulties in moving from general research objectives to a specific research design is the need to decide which issues justify an intensive effort to obtain detailed quantitative measures. These decisions involve not only a ruthless ordering of our own research priorities but also consideration of the existing state of knowledge in each area of enquiry. It may be useful to consider the kinds of contributions that research of this kind during the 1970s has made to our understanding of labour utilisation patterns in rural Java. Several studies at village level, using either direct observation (Edmundson 1976, which unfortunately covers only adult men) or variations on the short-term 'recall' technique (my own 1972-73 study and the 1977-78 'Rural Women' study already mentioned; Hart 1980; Moji 1980; Sigit 1981) have provided a cumulating set of information - such as that illustrated graphically in Figures 1 and 2 - which has led to quite drastic revision of standard views of the work patterns of rural men, women and (in cases where these data were collected) children.

(INSERT FIGURE 1 ABOUT HERE)

It is now rather clear, for example, that the lives of men and women in poor peasant and landless households in this densely-populated island are characterised not by involuntary idleness through unemployment but by very long hours of work with low returns; that women work longer hours than men, and that a significant proportion of their time is devoted to directly income-producing

work; that even while most men describe themselves as 'farmers' or 'farm-labourers' in conventional employment surveys (and their wives often as 'housewives' or 'helping the husband on the farm') it is common for more than half of their time in direct production to be spent in a variety of supplementary, non-agricultural activities giving even lower returns than the prevailing agricultural wage; that in the 'slack' agricultural seasons we rarely find a reduction in working-hours but often the opposite, longer hours of work for lower returns because of the low returns to available off-farm employment; that a large amount - in one study, more than half - of all work done in the community is done by children who are normally excluded from conventional employment statistics; and finally, that the sexual division of labour is in practice by no means as rigid as the prevailing ideology suggests, not only because women are involved in large amounts of direct production but also because men make significant though smaller contributions in various kinds of child care and housework. This last conclusion raises further questions: why and how is the ideology of separate and bounded gender roles maintained when in the daily practice of individual households they are often exchanged, and whom does this ideology serve?

All of the findings just noted (and others which could be added) may seem rather obvious. But they certainly were not obvious in the 1970s to most intellectuals, policy-makers and activists concerned with issues, policies and projects that directly concern the lives and working conditions of rural women and the rural poor in general (for example, pronouncements by public figures that 'our main problem is how to make the rural population work harder' and 'the female part of our population do not participate in national development, because they spend all their time in housework' are still common). In these conditions of large-scale ignorance and prejudice at the centre, 'hard' data can play an important role, and it is partly because of the growing number of reliable studies now available from several countries that serious discussion of these issues - and hopefully, the efforts of policy-makers in countries where they are interested, and activist organisations in countries where they are not - have some hopes of becoming more realistic.

It would of course be naive to suppose that better information on rural labour use, however reliable and relevant and however widely disseminated, is in itself a source of improved recommendations for action. One recent intensive study (based on 24-hour recall) of time allocation in 24 villages of Central Java, after showing that women contribute more than half as much time in

direct production as men (besides almost 6 hours of reproductive work daily, compared to 1 hour by men) concludes:

'Under these conditions, enhancement of women's roles should not emphasise skills to increase their income-earning capacity... Women clearly will be more productive in housework than in work outside the home. Replacement of their role in the home by outsiders or other family members will reduce productivity in this work, which in turn will harm household management. Enhancement of rural women's roles can be achieved by increasing the efficiency of women's household work' (Sigit 1981: 3. My translation).

The point here is not to question the recommendation as such (which by restricting official efforts to women's reproductive work appears to promote a further segregation of gender roles beyond that apparent in actual work patterns) but to note that it does not follow from the time-budget data themselves but from a number of unstated prior assumptions about appropriate roles for women. Other researchers - and perhaps the women themselves - might start from a different set of assumptions and use the same data as the basis for a quite different set of proposals.

With the availability of several reliable time allocation studies using similar methods, comparative work also becomes possible and raises a new set of interesting issues. For example, Figure 2 compares labour use for men and women in three economic classes in two villages of West Java: village 'S' at the top (the site of the comparison of different techniques in Tables 1 and 2) is clearly more agricultural while village 'P' at the bottom, closer to the capital city Jakarta and its expanding satellites, has a rather high proportion of non-agricultural production. The comparison suggests a tendency for women to be less involved in direct production and men less involved in reproduction in all three classes in the second, industrialising village. This disturbing possibility - that the sexual division of labour becomes not less but more rigid with rural industrialisation - now needs further investigation at two different levels: with less intensive research in a large number of villages to confirm whether it is generally the case, and with small-scale, qualitative work in selected locations to discover why it is the case, what are its consequences for the position of women in the home and in society, etc.

(INSERT FIGURE 2 ABOUT HERE)

MEASURING 'DECISION-MAKING' IN RURAL HOUSEHOLDS

We now turn from the measurement of rural men's and women's activities - which however complicated in practice is conceptually rather straight-forward - to the measurement of one aspect of the relations between them. As part of the 'Rural Women' research project already mentioned, we wanted to examine how decisions are made in rural households and especially the relative participation of men and women in decisions relating to production, consumption, savings, the birth and rearing of children, and social-economic relations with other households. We were particularly interested in examining the validity of the common view (reflected in community ideology as well as in the writings of many social scientists and in the organisation of development programmes) that in rural society men make decisions about production while women control the household budget. The conventional 'survey' approach to such questions normally involves formal interviews and questions of the form: 'In this household, who usually makes decisions about (choice of crops, fertiliser use, labour recruitment, sale of the crop, how much to spend on food, the daily menu, birth regulation.....etc.)'. We felt that answers to such questions are more likely to reflect socially-acceptable norms about who ought to control decisions, rather than the actual division of spheres of influence between individual husbands and wives. We therefore tried to develop a method more likely to capture the reality of actual decision-making practice, which may be of interest (though not without its problems), particularly because it involved an essentially 'qualitative', case-study approach to data collection but one which also allows quantitative analysis and presentation of the results.

During the second half of the year's fieldwork, when the research team had already been long resident in the village and were well acquainted with all the sample households, they began a series of informal visits and discussions with husbands and wives in these households, using only a kind of interview guide which simply listed various areas of day-to-day life in which we wanted to know how decisions were made. They did not ask all these questions at one visit, nor always in the same way, but sought opportunities for discussion with men and women together and separately, trying wherever possible to elicit concrete cases of decisions recently made. From these accounts and impressions, recorded in field notes, the researchers attempted to construct a cumulative picture of the pattern of decision-making in each individual

household. This material may be used in its original 'qualitative' form, to provide illustrations and to acquaint readers with the kind of information we are dealing with. The extracts from field-notes below, for example, show contrasting cases in the areas of (1) production and (2) household expenditures:

1. 'After pests had destroyed the paddy crop in 1977, Mr. K decided to stop planting paddy in all the rice-fields except one plot close to the house. In the other fields paddy was replaced by intercropped tubers, beans and several kinds of vegetables, even though this broke the local prohibition on planting crops other than paddy in the rice-fields. This decision has important consequences for the seasonal pattern of household income (small amounts will come in more frequently, rather than large amounts twice a year) and also for the seasonal pattern of labour inputs by both Mr. K and his wife, but he took the decision himself without consulting her'.

'After pests had destroyed the paddy crop in 1977, Mrs. U suggested a change of cropping pattern on both the rainfed and irrigated rice-fields. The rainfed field could be planted earlier than usual by making a seedbed before the monsoon came, and as a result they had finished transplanting while other farmers were only beginning to prepare the seedbeds. On the irrigated field, she suggested they try a dry-season crop of paddy instead of the usual beans and pulses, and this was also done successfully. Mrs. U described the process by which farming decisions are made: "I make a suggestion; he asks why; I explain, based on my experience, and then he agrees to do it"'.

2. 'Mr. X seems to be the decision-maker in almost all household affairs, including expenditures on food and clothing (which he buys himself) and education (sending the children to religious school because it is cheaper). Mrs. X commented: "That's what I learned from my parents - the husband is the one who makes decisions". These include decisions about her own health care and that of her children; he decided that a traditional paraji (midwife) should assist her in childbirth, takes children to the dukun (traditional healer) himself when they are sick, for example when one child recently got smallpox and he decided not to take him to the Community Health Centre'.

'Mrs. R made all the arrangements for repairing the house: buying the materials recruiting labourers and finding money to pay for them, even deciding where the door and windows should go. Mr. R would have nothing to do with it, not even helping to oversee the labourers even though he was sitting idle in the house while much of the work was going on. She complained about his lack of help: "Well, I don't care if it isn't well done - he left it all to be done by a woman".

However useful it may be to the researchers, this kind of information (collected from 93 married couples in the two villages and covering about 30 areas of decision making in each case) cannot be presented to a wider audience without conversion into some more summary form. The researchers

therefore used their case materials to place each couple along a scale in each area of decision-making, with decisions made by 'wife alone' and 'husband alone' at each extreme and three intermediate categories of 'joint' decision, as in Table 3 below. The results can thus be analysed and presented quantitatively and suggest many interesting variations between different areas of decision-making, between classes and between individual couples in the same class (and also between the two villages, although space permits an illustration from only one village in the Table shown here).

(insert Table 3 about here)

Table 3 shows the form in which the results can be presented (for a selection of decisions in the field of production, expenditure and family formation). The table shows the percentage of couples falling in each place on the scale of husband/wife dominance for each type of decision, with indicators helping the reader to see where the greatest number of cases lies. However, the cases falling outside the majority are as important as those inside; the results suggest one interesting conclusion, that norms do not rigidly determine the actual division of decisions between husbands and wives. Wives appear neither so excluded from decisions in the extradomestic domain of production nor so wholly in charge of the domestic (reproductive) domain as is implied by the normative segregation of these spheres of influence in community ideology (and as is reflected in the division of official extension efforts into 'agriculture' for men and 'home economics, health and family planning' for women). In areas where the majority of cases follow the norm there are always significant exceptions; in other areas even the majority of cases do not follow the norm. This might be held to point to the existence of considerable room for manoeuvre, room for struggle and room for change, and also raises a question parallel to that already noted in our discussion of the division of labour: if local norm and actual practice are so much in contradiction, what is the function of the norm and how is it maintained? Why, for example, does ideology succeed in placing 'housekeeping' so firmly in the hands of women, when in reality men are also deeply involved? These questions are better approached with qualitative research, but collection of the kind of data I have described, although they only begin to scratch the surface of decision making, is one way to bring them to the attention of those who may not previously have seen the need to ask them.

The problem still remains that however quantitative and systematic the data may look, they were collected by the qualitative procedures just described and are certainly subject to various kinds of bias. By concentrating on actual decisions recently made rather than on general questions about how decisions are 'usually' made, we hope to have peeled away a part of the layers of 'norm' that characterise discourse about these topics and come closer to actual decision-making practice; but how much closer? It is not realistic to suppose that the results are yet free from the pre-conceptions and values of either the respondents or the researchers.

In short, although in this case we have measured something and probably with as much care as is possible, we are not quite certain what it is; and even if these procedures have succeeded in capturing actual decision-making practice, great care is required when presenting the results to emphasise that we have not measured anything more than that. As was suggested some years ago in Safilios-Rothchild's review of conceptual and methodological issues in the study of family power structure (1970), decision making is only one aspect of family power relations; and furthermore, even in decision making itself the most important aspect may not be the one that we tried to measure (who makes decisions?) but rather who influences those decisions, who has the power to delegate decisions to a spouse and to overrule them in cases of conflict, and whose interests are ultimately served by the outcome of those decisions? Quantitative approaches are not likely to be fruitful here. And although in reporting this research we preface our quantitative analysis with discussion of other aspects of social and economic life in the two villages which tend to place women in a structurally subordinate position to their husbands (cf. White and Hastuti 1980) there is a real danger that readers may too easily focus on the quantitative material and jump to conclusions based on them, leaving the researchers behind with a long list of qualifications, cautions and methodological disclaimers.

CHANGES IN FEMALE LABOUR RECRUITMENT AND PAYMENT

In recent years various authors have discussed the problems of assessing the impact of agricultural 'modernisation' on women, especially strategies of intensification and commoditisation in food grain production aided by 'green revolution' technologies (cf. Agarwal 1981; Palmer and von Buchwald 1980).

In Indonesia few studies have been directed specifically at this question; since the early 1970s however a number of publications, appearing as by-products of more general studies, have drawn attention to changes in the mode of labour recruitment and payment in rice harvesting which threaten the employment and incomes of landless and near-landless rural women (some available English-language publications are Collier et al. 1974; Sinaga and Collier 1975; Stoler 1977; Utami and Ihalauw 1973). A wide variety of changes have been reported, including the change from the small finger-knife (ani-ani) to the sickle which reduces the number employed and often involves a shift from female to male harvesters; a decline in the level of bawon wages (bawon is an in-kind wage of proportion of the quantity harvested); tebasan, the sale of the standing crop just before harvest to a middleman, who brings in his own harvesting team and thereby denies employment to large numbers of villagers; various means of restricting the number of harvesters allowed to enter the field, and ceblokan or kedokan arrangements by which, in order to gain access to a harvesting wage, the worker must perform other tasks such as transplanting or weeding, without pay.

While these changes are by now well documented, there is much confusion as to the time at which they began to occur. For example it is often assumed, or concluded from interviews with a few informants, that they occurred rather suddenly after the introduction of high-yielding varieties in the late 1960s and that before that time harvesting arrangements had not changed for generations, with open access to all and a traditionally-determined bawon wage not subject to bargaining or dispute (cf. Collier et al. 1974). Here again, the problem arises of trying to get beyond general statements of informants (in this case, about how things used to be) to a more concrete and systematic approximation: more precise dating of the changes, for example is important if we wish to explain them, and in particular to see whether they are indeed related to the introduction of 'green revolution' technologies.

The example of rice harvesting is useful, not only because of its importance to rural women (in many studies it has been the largest single source not only of women's but also of household income in the landless and near-landless households who make up one-half or more of the rural population), but also because it highlights the problem confronting researchers who want

to investigate agrarian changes affecting women but who do not have any 'base line' information (from the period before the change) on which to base their comparisons. Researchers from the Agro-Economic Survey in a series of village studies have developed a simple retrospective technique of estimating the nature, timing and rate of harvesting changes which does not require data from earlier periods (although most of their studies have in fact been carried out in villages where base-line survey data are available from the late 1960s). Samples of about 60 farmers are asked to describe the system of labour recruitment and payment used in their last harvest, and for how many years they have used it; then the system previously used, and for how many years; then the previous system, and so on until we reach the point at which they first took over the farm (in cases of married farmer couples, the questions should be addressed to husband and wife since it is frequently women who organise harvest labour recruitment and payment, as may be seen in Table 3 above, cf. also Stoler 1977). Precise dating is of course difficult, but the beginning is easily fixed (people remember when they first took over a farm) and intermediate dates approximated by reference to local or national events whose dates are known (the 1965/66 change of regime, a local flood, building of a new dam or road, etc.).

(INSERT TABLE 4 ABOUT HERE)

The results can be assembled as in Table 4 (from a village in West Java, reported in Kikuchi et al. 1979). Even allowing for some imprecision in the dates, the table permits some important conclusions. First, we cannot speak of a uniform 'system' of harvesting at any period; a variety of arrangements have always coexisted. Second, harvest arrangements have been shifting in various ways (but always in the direction of more restrictive and less generous arrangements) for at least the past 30 years - long before the 'green revolution' - and these shifts can be divided into four phases: (1) in the early 1960s, towards a more 'closed' pattern of recruitment with greater numbers of farmers limiting access to villagers or restricting the numbers allowed to enter their fields; (2) in the middle 1960s, a marked shift towards limitation of access to those specifically invited, and the appearance of the ceblokan system in which harvest wages are tied to previous unpaid transplanting; (3) in the late 1960s and beyond, a continued shift to increasingly onerous ceblokan arrangements, in which the bawon is lowered from 1/6 to 1/7 and weeding is added to transplanting as the unpaid requirement for access;

(4) finally, in recent years the appearance of harrowing (men's work) among the obligatory requirements, the first time that men's and women's labour have become linked in a single transaction.

While the details vary from village to village, a more general conclusion emerging from all the case-studies carried out so far is that harvesting changes can not be directly related to the 'green revolution' as such; at the most, it has merely accelerated changes already underway and explanations must be sought elsewhere. It is perhaps because of the tendency to relate the changes mechanistically to technological change that few researchers have paid attention to the conflicts and struggles surrounding them. From other qualitative studies, for example, we know that the labour-tying ceblokan system was banned in some areas during the 1960s thanks to the active pressure of popular left-wing organisations (cf. Boedhisantoso 1974; Tjondronegoro 1978).

As in the other examples previously described, this quantitative retrospective approach (which could be applied to many other agrarian changes involving women) does not in itself explain anything but can serve to question conventional interpretations and point to important areas for further investigation. Whether such pointers are followed of course depends on the interests and biases of researchers; the economists Hayami and Kikuchi who assisted the Agro-Economic Survey team in the development of this useful technique (and analyse the results of two village case-studies, 1981) manage to discuss these harvesting changes without once informing the readers that women are involved. In fact their text and tables are not populated by men and women at all, but only by a curiously genderless class of 'labourers'! This is all the more surprising for the reader since one main purpose of their work is to show that the function of all these harvesting changes has been to bring traditional (women's) harvesting wages in line with the prevailing 'market' wage rate (defined as the wage level in land-preparation, an exclusively male task); implying a remarkable degree of integration in the markets for male and female labour.

CONCLUSION

These three illustrations of quantitative techniques in village-level research share some common features. They move beyond general questions about the way in which life is 'usually' organised (which tend to produce answers reflecting conventional or publicly acceptable views) to the concrete experience of individuals ('What did you do today?', 'Who made this particular decision?', 'How did you recruit and pay your harvesters last season, three years ago, ten years ago?'). In all three cases, actual experience was at odds with conventional views, and a number of new questions thus raised which point in turn to issues for further qualitative investigation. This general approach to measurement, then, can serve both to question prevailing ideas about the life and work of rural women and also as a tool for focusing and re-directing the researcher to new issues; it does not itself provide explanations but helps to clarify what it is that needs to be explained, and as such is no substitute but rather a complement to qualitative work.

In trying to show the value of a combination of the two approaches I have passed over many problems attached to it. Since nearly all research plans are over-ambitious, many researchers find that quantitative work, with its relatively fixed demands in terms of sample sizes etc., leaves too little time and energy for qualitative work. There is also an undeniable tendency for quantitative work to alienate the researcher from the 'data' (and from the individual men and women who provide it); however, this problem is not inherent in the activity of measurement itself but in the unnecessary transference to small-scale research of both a technology and social organisation of research appropriate to massive sample surveys. In small scale research there is no need for research teams, even quite large ones, to be organised in a hierarchy of 'fieldworkers', 'data-processors' and 'research/analysts' (which results in the absurd situation of the 'researcher' trying to use a pile of ready-made tables of numbers to try to understand the lives of people (s)he has never seen). For the same reason, after some experience with both techniques I am quite dubious about the value of mechanical data processing and analysis, for example when field data are immediately transferred, coded, key-punched and disappear into a computer tape, only to re-emerge in the form of aggregated tables and statistical tests. The researcher is at once alienated from the information - no matter who has collected it - with no chance to go through it case by case, a process which itself helps us to see patterns and to understand variations in a way which no amount of staring at computer-output can replace. As I have tried to suggest, numbers themselves are not a source of answers, only of further questions.

FOOTNOTES

1. The three examples are taken from various research projects of the Rural Dynamics Study, Agro Economic Survey of Indonesia (in the first two cases, in collaboration with the Institute of Rural Sociological Research, Bogor Agricultural University). I would like to thank my colleagues in both organisations and to record my appreciation of their remarkable contribution to village studies in Indonesia.
2. Comment by a participant in the Research Training Workshop on Land Tenure and Agrarian Relations, convened by the Agro Economic Survey, Cipayung (Indonesia), October 1980.
3. A detailed account of the procedures and problems encountered in both collection and handling of the data will be found in White 1976c: Appendix B.
4. For a more detailed account of the procedures and more detailed analysis of the results, see Wigna et al. 1980.
5. The Laguna Household Survey project in the Philippines offers another opportunity for partial comparison of time allocation research results using two different techniques, in this case one-week recall and direct observation. In the second round of surveys (1977) in which procedures had presumably been made as efficient as possible, mothers in one-week recall appear to underestimate their own 'market production' time by 69% and that of their husbands by 49%; for 'home production' the results are closer, with mothers this time overestimating their own work by 6% and that of their husbands by only 3%. However, while the total time in home production seems to be accurately recalled, the observed and recalled division of this time between child care, cooking and 'other' work differ greatly (King and Evenson 1983:59f).

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Table 1

Comparison of results of 12-month and 24-hour recall:

Average annual working-hours per household in directly income-producing work in village S, West Java

Recall period	Interview frequency per year	Class I	Class II	Class III
12 months	1x	2373	2718	2549
24 hours	12x	4123	4538	5985
Percent of work apparently missed in 12-month recall		42%	40%	57%

Source: Wigna et al.

Table 2

Comparison of results of 30-day and 24-hour recall:

Average annual working-hours per adult man and woman in directly income-producing work in village S, West Java.

Recall period	Interview frequency per year	Adult men			Adult women		
		Class I	Class II	Class III	Class I	Class II	Class III
30 days	12x	1624	1997	1969	749	767	1048
24 hours	12x	2657	2810	2893	1184	1388	1366
Percent of work hours apparently missed in 30-day recall		38%	29%	32%	37%	45%	23%

Source: as for Table 1

Note to Tables 1 and 2

Stratification of the sample was as follows: Class I - farm-size less than 0.2 ha and off-farm income less than Rp 15,000 per month (n = 23 households, 26 adult men, 32 adult women); Class II - farm-size 0.20 - 0.29 ha or less than 0.20 with off-farm income more than Rp 15,000 per month (n = 15, 16 and 21); Class III - farm-size 0.30 ha and above (n = 22, 17 and 26).

Table 3

Distribution of married couples according to husband/wife involvement in various areas of household decision-making, in Village S (row percentage)

Class (n)	Nature of decision	WIFE	JOINT DECISION			HUSBAND
		ALONE	wife dominant	equal	husband dominant	ALONE
I	Production input purchase	0	18	0	54	28
(12)	Hired labour recruitment	0	34	11	44	11
	Food budget	67	33	0	0	0
	Clothing purchase	45	45	0	0	10
	Health care	0	33	0	33	33
	Social/cermonial expenditure	0	44	11	44	0
	Number of children	0	11	67	22	0
II	Production input purchase	9	0	0	55	36
(11)	Hired labour recruitment	10	30	10	30	20
	Food budget	64	27	0	0	9
	Clothing purchase	27	36	0	36	0
	Health care	18	36	0	46	0
	Social/ceremonial expenditure	0	45	0	45	0
	Number of children	0	18	73	9	0
III	Production input purchase	0	15	5	65	15
(20)	Hired labour recruitment	0	40	10	50	0
	Food budget	40	50	0	5	5
	Clothing purchase	10	70	5	15	0
	Health care	10	35	25	30	0
	Social/ceremonial expenditure	0	75	5	20	0
	Number of children	0	25	31	44	0

Source: White & Hastuti (1980).

Table 4

Changes in harvest labour recruitment and payment:

percentage of farmers using various harvest contracts in a village in Subang, West Java, 1950s - 1978.

	Bawon ^a				Ceblokan ^b					Total
	PO	OV	OM	LI	1/6(T)	1/7(T)	1/7(T+W)	1/7(H+T)	1/7(H+T+W)	
1950s	35	29	18	18						100
1960-61	29	31	21	19						100
1962-63	16	34	33	17						100
1964-65	9	16	16	32	27					100
1966-67	3	10	8	27	52					100
1968-69	1	4	6	19	44	24	2			100
1970-71			2	10	33	51	4			100
1972-73				8	17	67	8			100
1974-75				7	15	67	10	1		100
1976-77				4	7	67	18	2	2	100
1978				4		72	19	1	4	100

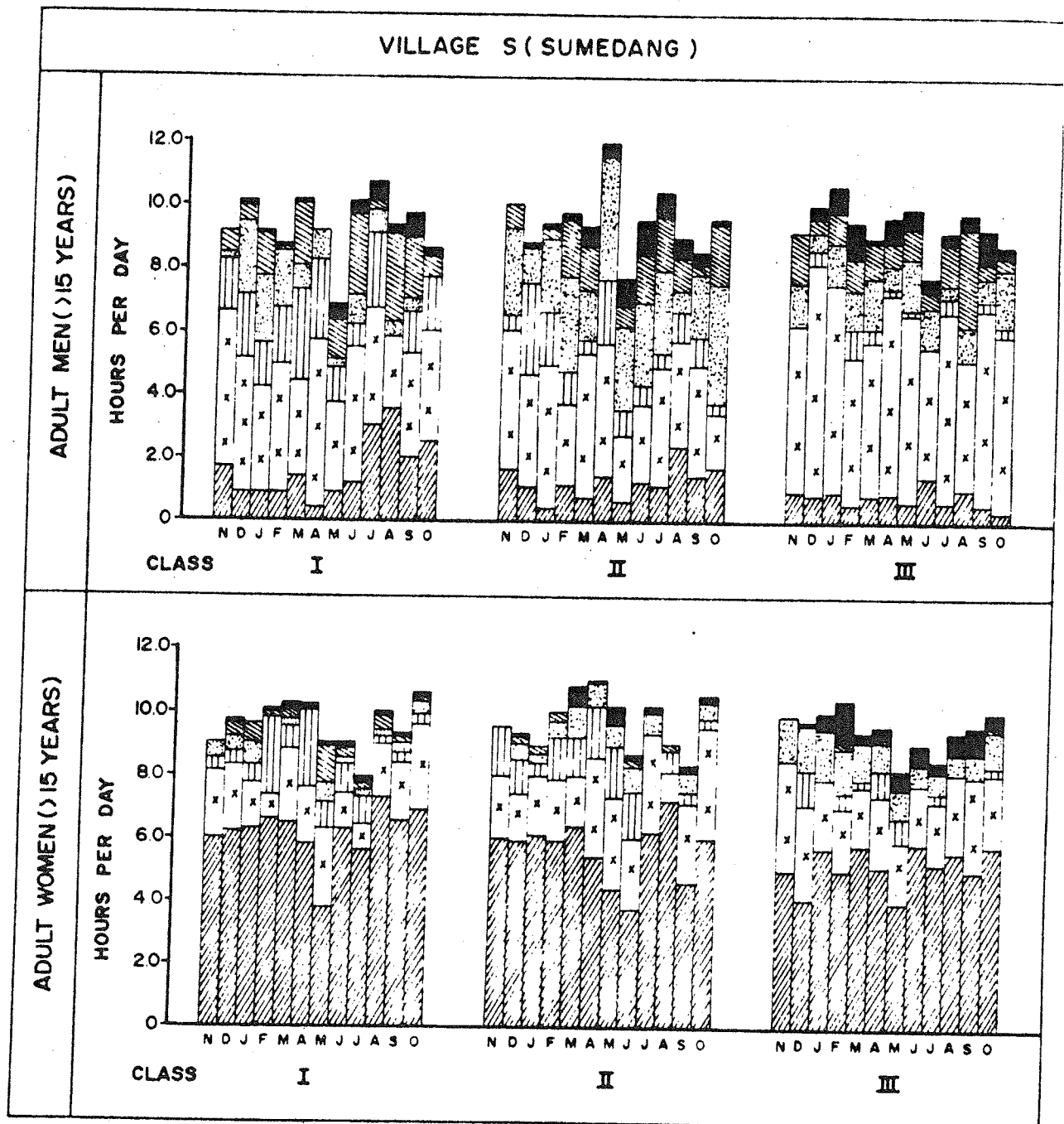
a. Bawon system: PO-purely open, OV-open for villagers only, OM-open with maximum limit, LI-limited to invitees.

b. Ceblokan system: 1/6, 1/7-harvesters' share; T, W, H-obligatory work to establish the harvesting right (T-transplanting, W-weeding, H-harrowing).

Source: Kikuchi et al. 1979.

Figure 1

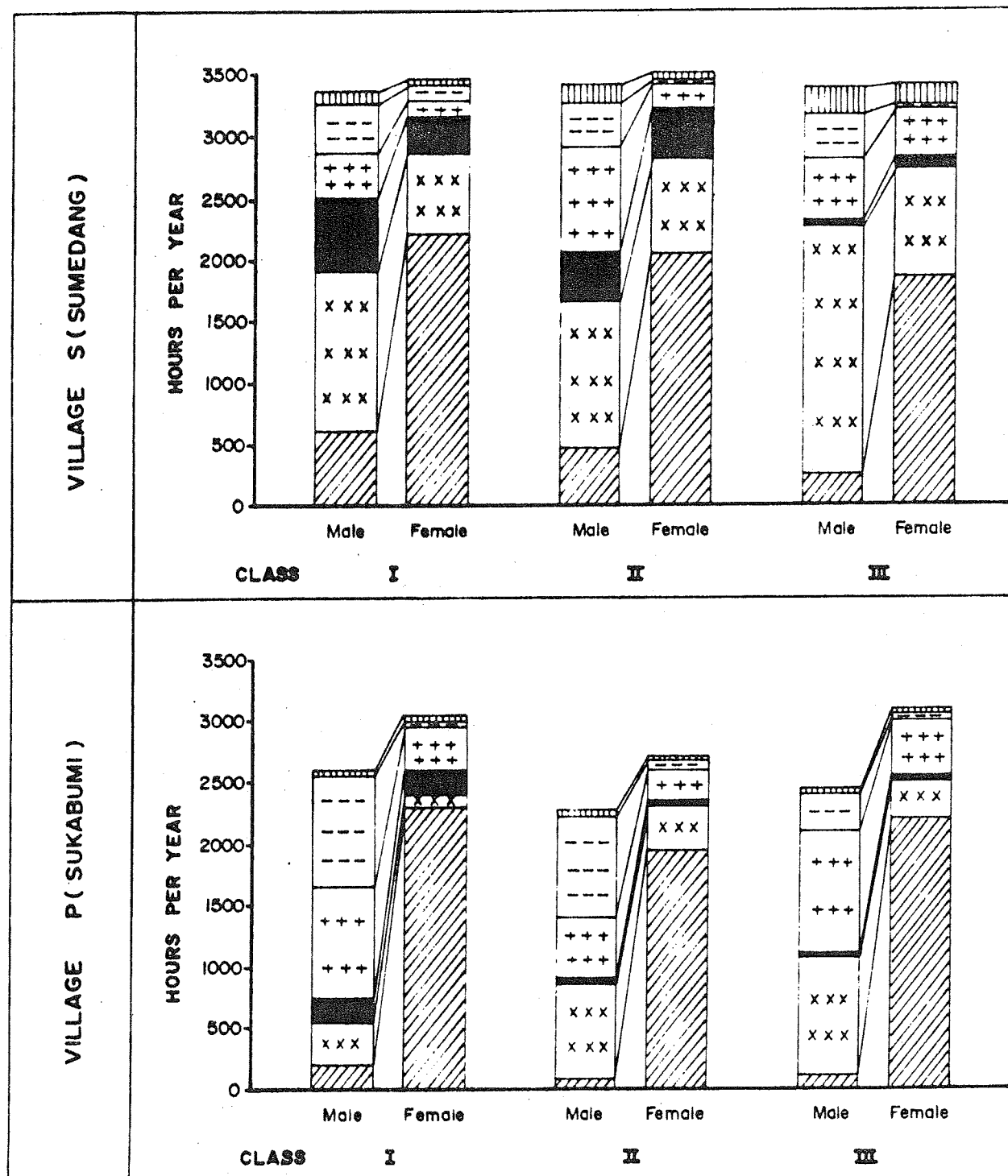
Average daily working hours of adult men and women by month, type of work and economic class in village S, West Java, 1977 - 1978



Source: Pudjiwati Sajogyo et al. 1980. For stratification criteria see note to Tables 1 and 2.

Figure 2

Average annual working hours of adult men and women by type of work and economic class in two villages of West Java, 1977 - 1978



Source: as for Figure 1