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Polly Hill

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INTRODUCTORY CHAPTER

The Need to Engage in the Field Experience

As a fieldworker who has long studied agrarian systems and economic inequality in the tropical third world, more recently with particular reference to the causes of severe impoverishment, I have suffered much condescension since about 1970 owing both to my unfashionable methods of field enquiry and to my inability to formulate logically coherent conceptual systems of any general appeal. Now that I have widened my scope to include a district of south India (my earlier work for a period of fourteen years from 1953 had been confined to West Africa), I have ventured to write this book as a practical demonstration of the possibilities of formulating, on the basis of detailed fieldwork, a set of coherent hypotheses relating to a specific type of rural under-development which has recently come into existence in certain very densely populated dry grain zones in both West Africa and south India – as doubtless in certain other regions of these and other continents. There is a crying need for *systematic categorisation of types of rural under-development in the tropical third world*, and I hope that in identifying and analysing this particular ‘dry grain mode’ I shall have done something to encourage other fieldworkers to identify other modes.

The conditions associated with the existence of this dry grain mode are summarised at the beginning of Chapter II, which serves as an introduction to Chapters III to XI – the remaining chapters being historical. In this introductory chapter I try, in a general way, to justify the use of detailed field enquiries in the tropical third world, without regard to my own success, or otherwise, in adopting such procedures.

The long and agonising process of detailed exploration in the field has as its aim the formulation of certain general principles – it should not be seen as an old-fashioned kind of nature study pursued as an end in itself, but as a necessary preliminary to socio-economic analysis and the pursuit of theory. But partly because the problems facing the rural third world are rightly seen as so urgent, intellectual standards are commonly thrown to the winds: despite extreme ignorance of rural conditions, the formulation

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of policy takes priority, followed by analysis and theory generally – description and classification (much of which ought to be statistical) trailing far far behind, if it is there at all. From bitter experience I know that a fieldworker who fails to give priority to policy questions is invariably accused of lack of compassion.

This contemporary contempt for proper field enquiry emanates from a great many quarters, some of them unexpected, and has resulted in some unholy alliances between, for example, marxists and apolitical ('conventional') historians, both of whom are apt to assume that there is a large body of proven received wisdom of sufficient consistency and universality to require no testing in the field – whereas I myself doubt whether any such 'wisdom' has universal validity. Much of this orthodoxy is based on simple evolutionary notions, such as the inevitability of recent 'cash crop revolutions' and the belief that rural inequality necessarily derives from the urban world. It takes account of the wide extent of extreme poverty, but has little real conception of its depth.

Partly in the hope of improving their standing in relation to economists, whom they regard with unnecessary awe, economic anthropologists, whether they be marxists or not, are apt to emphasise their theoretical rectitude by denouncing their enemies, in the manner of M. Bloch:

The criticism which marxists and others would make of empiricism is not so much that it is wrong but that it is impossible; this means that there are theoretical postulates present in the work of all social anthropologists and that these have political significance¹

Since such a fear of field enquiries is shared by economists, who are nowadays especially terrified of the dangers of wasting money by pursuing work of no practical utility, I now counter Bloch's statement by listing a set of general postulates which taken together, and in no particular logical order, might help to justify the experience of field enquiry.

A list of general postulates relative to methodology

1. There *are* theoretical postulates present in the minds of all trained fieldworkers – they are unavoidable.
2. Some, but not all, of these postulates are necessarily unconscious.
3. Some, but not all, of these postulates have political significance: the full array is necessarily unascertainable.
4. The postulates themselves are, to some degree, necessarily transformed by the 'work process' involving the three stages of fieldwork, analysis of material and writing (with all that that involves); we do not know what we have found out until we read what we have written.

¹ From the introduction by M. Bloch to *Marxist Analyses and Social Anthropology*, ed. M. Bloch, 1975, p. xiii.

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5. The 'findings' and the transformed postulates therefore, become, to some degree, inextricably confused; at least this means that one travels with a revised set of postulates on the next expedition to the field.

6. Theoretical systems are not, in themselves, therapeutic – as the field experience ought to be; whether one adopts a theoretical approach or not, there is no hope of expunging one's unconscious postulates or of ensuring that they fit one's conscious theoretical apparatus of thought; entire purging is impossible and no one can render themselves classless.

7. Whether we happen to be marxists or not, our unconscious postulates, which cannot be wholly eradicated by theoretical training, are necessarily to some degree ethnocentric; by denying this, in the sense that we condemn others (though not ourselves) for their ethnocentrism, we falsify our work.

8. Part of our susceptibility to the criticisms of others is due to the fear that they perceive our unconscious postulates whereas, by definition, we cannot do so ourselves; by adopting a wholly theoretical framework which, by definition, is supposed to oust unconscious postulates, theoreticians vainly hope to reduce their vulnerability.

9. Anthropological fieldworkers may or may not ultimately come to have a better understanding than their readers of such of their erstwhile unconscious processes as have ultimately surfaced; however, the threefold work process to which they necessarily submit themselves may give them some advantages in appreciating their earlier naivety. (Is naivety necessarily ethnocentric?)

10. Yes, the psychoanalysts have taught us that we are all creatures whose preconceptions are partly based on our unremembered experiences; one and all, marxists, amaxrists and non-marxists, we are necessarily to some degree ethnocentric and class-centric and there is a limit to which our preconceptions may be modified, even with the help of therapists. (As elitists, indigenous fieldworkers in third world countries are apt to be greatly hampered by their class-centrism.)

11. The deliberately ahistorical approach of many theoreticians, which will be discussed below, renders them peculiarly ethnocentric; maybe the past lies in the future, but the unconscious mind is far too inventive to tolerate a vacuum.

12. It is, also, arguable that marxists are among the most emotional (and hence subjective?) of all investigators, if only because they find themselves obliged to identify various classes of actual 'villain' in the village – however, 'subjective' has an old-fashioned ring in this general context and is best avoided.

13. An exploratory field approach essentially relies on the testing of hypotheses in the field; although one would not know it at the time, unconscious as well as conscious hypotheses are certain to be tested.

14. The testing approach is much facilitated if resort to history is possible.

15. 'The general and particular interact and modify each other at every level. Nothing important is a matter of chance, but nothing can be seen to be caused by a single factor.'²

² A citation from a review by B. Crick of *Politics and History* by R. Aron, *The Observer*, 22 April 1979.

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16. The more unfamiliar the field, the more appropriate the method of submitting oneself to experiencing the realities of the unknown; educated 'experts', whether Westerners or members of the third world, know far less about third world rural economies than they think and owing to their alienation from the countryside they become increasingly prejudiced – this is particularly true of north Americans who usually refer to third world cultivators as 'small farmers'.

17. The fact, insofar as it is a fact, 'that our thinking is determined by our social position is not necessarily a source of error. On the contrary, it is often the path to political insight.'³

Abandoning these polemics, I now get down to more banal matters. I start by identifying the various world establishments which are apt to disdain the use of inductive field methods in studying socio-economic matters in the rural tropical world.

The *first* is the general establishment of Western academic economists, whose ideological outlook is reflected in the fact that it seldom encourages its graduate students to undertake such inductive fieldwork⁴ – this being in great contrast to various establishments of Western historians which, in the past fifteen years or more, have successfully emulated social anthropologists in both India and Africa. Why should the economists, who are so severely rent by political dissension, at least find themselves in agreement on this subject? I pose this question because I cannot answer it but can only roam around it. Perhaps economists do not want to understand 'the peasantry', which is regarded as outmoded or withering away, but only to transform its way of life – for is not industry 'the engine of growth'? Perhaps they think that all work resembling economic anthropology, with its apparently obsessive concern with unnecessary detail, is invariably devoid of practical value? Perhaps they suppose that they have already developed a sufficient body of received doctrine? These are mere random thoughts on a subject which seems to receive little attention. Ought the relationships of economists to the power structure of modern capitalism to be more clearly delineated?

The *second* establishment consists of official grant-awarding authorities, including the British Social Science Research Council and its counterparts elsewhere, which are all too apt to require their applicants to forecast their conclusions when outlining their research projects and to demand that

³ *Ideology and Utopia* by K. Mannheim, 1936, cited in *Caste, Class and Power: Changing Patterns of Stratification in a Tanjore Village* by A. Béteille, 1971, p. 12.

⁴ This is subject to the qualification that such institutions as the School of Oriental and African Studies in London or the West African Studies Centre in Birmingham are apt to encourage socio-economic fieldwork. See the list of 'Thesis Titles for Degrees in the United Kingdom 1977/8 and 1978/9', *Economic Journal*, March 1979.

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work should be 'useful'. But if one does not know what is not known, how does one know what might be useful? I think that it is owing to false analogies with the experimental methods of scientists (proper), who enjoy far more general prestige than social scientists, that it is thought that a set of detailed pre-packed assumptions, identifying the main socio-economic variables, should be taken to the field by the innocent applicant, as though it were a camera.

Third, there are the specialised agencies of the United Nations, such as the FAO, which seldom support *intensive* socio-economic studies affecting agrarian systems and never integrate them with projects such as vast irrigation schemes, until it is far too late. Our ignorance is as great today as in 1968 when Myrdal reported that an accurate statistical picture of the numerical strength of the main 'social groups' in farming communities could not be produced for any South Asian country.⁵

Myrdal identified our *fourth* group of anti-inductivists, certain elitists in the third world, when adding that:

The fact that rigorous enquiries – even those obliged to improvise somewhat arbitrary conceptual categories – have not been sponsored officially must be partly ascribed to the vested interests in concealment among the upper strata, both in rural and in urban areas.⁶

After spending nearly a year in south Indian villages I certainly agree that:

the numerical strength of the various social groups in the village, and the area of land each commands, may well be said to be among the best guarded secrets of the South Asian economies.⁷

And our ignorance of West African rural economies is far more profound.

Among the numerous reasons which account for the consensus of the four establishments I pick out the following. *First* is the failure to realise that we are so ignorant of the economic condition of man in rural tropical regions that we do not know how ignorant we are. *Second* (a connected point) was the precipitate and premature way in which economists started 'teaching under-development' soon after the second world war, despite their own profound ignorance; many of their early hypotheses about rural conditions, based on little if any evidence, have since hardened into truths. *Third* is urban bias and the failure to realise that the great bulk of the population in most third world countries will continue to rely on agriculture for many years to come. *Fourth* is the inherent belief in the essential reliability of official statistics⁸ relating to rural conditions which

⁵ *Asian Drama* by G. Myrdal, 1968, p. 1056.

⁶ *Ibid.*

⁷ *Ibid.*, p. 1056.

⁸ See Appendix to this Chapter.

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has been assiduously fostered by UN and governmental agencies and by third world countries themselves, notably India; it is no exaggeration to say that as the statisticians' power to manipulate the figures increases, so their lack of interest in the quality of the 'raw data' declines. The insistence that official statistics keep us basically well informed, in terms both of the identification of the salient variables and their fluctuations, is one of the fundamental intellectual immoralities of our time – it is, of course, regarded as one of the main justifications for dispensing with intensive fieldwork by individuals, although this may be the only means by which the basic defects in the raw material can be exposed.

Fifth is the wish not to offend officials in the third world by implying that they share our ignorance of rural conditions. Visiting experts usually have a touching and sincere belief in the omniscience of the officials they happen to encounter and are often unaware that urban bias is even more pronounced in the third world than in the West; unconscious colonial attitudes on both sides result in an inversion of former roles, the expert being over-subordinate. *Sixth* is the unfortunate respectability of the 'questionnaire approach' in villages, which necessitates identifying the salient variables in advance. *Seventh* is the poor quality of many village surveys which have been undertaken by indigenous economists since the last war, especially in India; it is true that the villages are far too often treated as isolates, both in space and time. *Eighth* is the genuine contempt which most 'technical experts', such as agronomists and irrigation engineers, feel for anthropologists and other students of rural life, on the grounds both that they lack expertise, their methods being no more than 'glorified commonsense', and that they pursue their work for its own sake, irrespective of its usefulness to the under-privileged.

Finally, there is a failure to realise that it is not the function of the fieldworker himself to produce a body of doctrine which will influence the course of events, but rather to provide relevant data for the use of those who construct the influential analytic systems. But if the fieldworkers' findings appear irrelevant to the mainstream of current thought they are always wholly ignored.⁹ So unless one is prepared to sit down indefinitely in an intellectual backwater it is essential to concentrate on broad subjects which might be expected to appeal to the policy maker – whose servant one is in the last resort. That is one reason why I, for my part, concentrate on the matter of economic inequality, hoping that my findings will have some

⁹ See *British Economic Thought and India 1600–1858: A Study in the History of Development Economics* by W. J. Barber, 1975, pp. 207 *et seq.*, on Richard Jones's failure to offer economists acceptable analytical criticism of the Ricardian master model.

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slight relevance to the passionate worldwide debate – but I have experienced a singular lack of success so far. I fear that the only fieldworkers whose findings have impact are those like C. Geertz who in *Agricultural Involvement*¹⁰ combined the three functions of investigation, analysis and policy formulation in an appealing and succinct manner. Fieldworkers must continue to hope that they may be permitted a narrower scope – that division of labour may ultimately prove acceptable, their findings being useful to theoreticians and policy makers.

My list of general postulates relative to methodology at least implies that the distinction between theoretical and field methods is not as straightforward as is nowadays often supposed, for both of them rely on the testing of conscious and unconscious (preformulated and unformulated) hypotheses in the field – if the theoreticians ever get there. Perhaps most theoreticians would dispute this, on the grounds that they are concerned to validate laws not hypotheses. But what could be the point of going to the field to test axioms of universal validity, such as the following, which I have arbitrarily formulated for present purposes:

Laissez-faire rural economies in the third world, in which most exchange transactions, including transactions in land, involve cash, are necessarily such that the rich constantly tend to benefit at the expense of the poor, whatever their conscious intentions, to the extent that non-radical official intervention, designed to improve the situation of the poor will necessarily fail to reverse the general tendency.

It is the theoreticians who have confused the issue by assuming, as Victorian political scientists did not, that the two methodologies are entirely distinct.

In 1890 John Neville Keynes, the father of John Maynard Keynes, offered the following outline of the deductive method in political economy, which consisted of three steps:

It is necessary, first to determine what are the principal forces in operation, and the laws in accordance with which they operate. Next comes the purely deductive stage, in which are inferred the consequences that will ensue from the operation of these forces under given conditions. Lastly, by a comparison of what has been inferred with what can be directly observed to occur, an opportunity is afforded for testing the correctness and practical adequacy of the two preceding steps, *and for the suggestion of necessary qualifications.* [My italics]¹¹

¹⁰ Subtitled *The Processes of Ecological Change in Indonesia*, 1963.

¹¹ *The Scope and Method of Political Economy* by J. N. Keynes, 1890, pp. 212–13.

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As J. N. Keynes proceeds to add:

It will be observed that only one of these three steps – namely the middle one – is strictly speaking deductive. The so-called deductive method in its complete form is thus seen to be not an exclusively deductive method. It . . . is still aided and controlled by induction.¹²

It will be noted that to the fieldworker the weakness of such a deductive method is that it provides no opportunity for the formulation of new, testable, hypotheses, such as had not been anticipated, but which had emerged in the course of the work, but only for ‘the suggestion of necessary qualifications’. If these qualifications are inconsistent with the ‘laws’ formulated in the first stage, then presumably the deductive method has failed?

Owing to the false belief that under-development in the tropical rural world is a standard condition with certain necessary salient features (and also to the associated belief that all members of any particular rural community are apt to have similar economic responses),¹³ many deductivists and textbook writers sought, from the 1950s onwards, to identify these features in the simplest possible terms so as to enhance their universality. The list of common presumptions, which underwent constant swings of fashion, makes very sad reading owing to the crudity of the ideas involved and the speed with which they suddenly seemed outmoded. I deal with a few of them here.¹⁴

The presumption of amorphousness and homogeneity is well illustrated by the following citation from a very respectable textbook by H. Myint:

A peasant family could have produced a much larger agricultural output than it was actually producing *But it chose not to do so* [my italics] for the simple reason that every other peasant family could do the same.¹⁵

In the 1950s it was commonly presumed¹⁶ by textbook writers that Asian and African cultivators were (unfortunately) all too apt ‘to choose leisure’

¹² *Ibid.*, p. 213.

¹³ Whereas the economic behaviour of the poorer people is often the mirror image of that of the richer.

¹⁴ I am not here concerned with the obsolete debate instigated by the Polanyi school since it is evident that the kind of tropical rural economies to which this book relates are necessarily regulated to some extent by ‘market principles’.

¹⁵ *The Economics of the Developing Countries* by H. Myint, 1964, p. 43. The generalisation referred to the ‘thinly populated peasant societies’ of Asia and Africa.

¹⁶ But not by P. T. Bauer and B. S. Yamey in *The Economics of Under-Developed Countries*, 1957, who insisted, p. 86, that there appeared to be ‘no ceiling to economic development imposed by inflexibility in habits of consumption in many, perhaps most, under-developed countries’.

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when, in their own interests, they ought to be working – an idea which was respectably dressed up in terms of a backward sloping supply curve of labour. (That this notion was later ousted by a belief in the over-responsiveness to the price mechanism on the part of all cultivators, reflected the post-colonial need to presume that the erstwhile ‘native’ was more like ourselves than we had thought.)

An associated idea was that peasants preferred leisure because their range of ‘wants’ was so limited. This was given particular cachet by G. M. Foster in ‘Peasant Society and the Image of Limited Good’,¹⁷ who based himself on fieldwork in Mexico. The article is altogether invalidated by the assumption that the ‘peasant community’ was a closed system, for if ‘peasant’ has any proper application to third world rural economies it must refer to cultivators who are, in some straightforward sense, economically subservient to the capitalists of the wider world; nor do the formal qualifications of the premiss, that it is made for the purposes of analysis and argument only, give the work any pertinence.¹⁸ On the one hand censorious outsiders are always apt to deplore crippling ‘bride prices’ and deep indebtedness; on the other hand, there is the contrary presumption of ‘limited wants’ – which is deeply offensive to those of us who have witnessed extreme poverty in the field.

Myint was also responsible for developing the ‘vent for surplus’ theory of international trade in relation to third world countries¹⁹ – a concept which ultimately stemmed from Adam Smith. But why should such a theory, which states that exporting led to no decline in other forms of economic activity, necessarily be of general application, when circumstances are so variable? Whereas detailed fieldwork has convinced me that the original growth of Ghanaian cocoa exporting represented a large net gain to the rural population concerned (partly because food-farming was basically the responsibility of women), the same was by no means necessarily true of the early years of groundnut exporting from northern Nigeria, the main effect of which may have been (for all we know) to deprive the local population of much needed dietary fats and proteins in the form of groundnut oil.

* * *

¹⁷ *American Anthropologist*, April 1965, pp. 293–315.

¹⁸ The author’s ‘image of limited good’ even assumes that ‘peasants’ believe that such attributes as friendship and love (as well as wealth) ‘exist in finite quantity’ and are ‘always in short supply’. *Ibid.*, p. 296.

¹⁹ ‘The “Classical Theory” of International Trade and the Underdeveloped Countries’ by H. Myint, *Economic Journal*, June 1958, pp. 317–37. For criticism of this theory see ‘The Vent-for-Surplus Model and African Cash Agriculture to 1914’ by J. Hogendorn, *Savanna*, June 1976.

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In the course of this book, I shall call in question the conventional usages of several fundamental economic terms such as *subsistence* and *surplus* (the latter in certain contexts) on the grounds that they incorporate and conceal implicit assumptions which it is our particular function to investigate. It is the very words whose meanings appear to be the most obvious which particularly impede our enquiries. But I deal now with the semantics of *peasant* which, readers will note, is employed nowhere in this book – and with no resultant circumlocutions.

Very abruptly in the 1960s *peasant* became the vogue word, denoting virtually all the inhabitants of the rural tropical world. Prior to that date we anthropologists had distinguished between farmers (or cultivators), farm labourers, artisans, traders and other groups – but suddenly they all became subsumed under *peasant*. The operation reminded me, in mirror image, of the moment when, virtually overnight, the word ‘native’ had been removed from all the exhibits in the (then) Imperial Institute, being replaced by ‘African’, ‘Indian’ and so forth – and rightly so, for *peasant* in its contemporary usage in the tropical third world is the semantic successor to *native*, incorporating all its condescending and derogatory racial overtones, especially among town-dwellers in the third world. We anthropologists had previously managed quite well without that Eurocentric word and it seemed odd that its use should suddenly have become compulsory.

In stating my numerous objections to the use of this emotive word, I lay primary stress on the power that it has to conceal the extremities of individual poverty. While *peasant* is necessarily very derogatory in many contexts, it also has cosy connotations: it does not readily conjure up a situation where a high proportion of the population rather resembles an under-employed, landless rural proletariat. In literature and tradition a peasant is a hardworking, suffering man who tills his own small plot with a little success – not a propertyless man who is nearly always hungry.

The other main objection to *peasant* is its power to confirm our aboriginal ideas of an amorphous (undifferentiated) tropical peasantry, despite the extraordinary efforts that began in the 1970s to qualify the word appropriately. Considering the political leanings of those who were most insistent on employing *peasant*, it is astounding that so little heed was paid to the warnings uttered by Lenin in *The Development of Capitalism in Russia*²⁰ on the dangers of obscuring differentiation by the use of notions relating to average peasants of various categories. Lenin himself noted²¹

²⁰ Vol. 3 of the Collected Works of V. I. Lenin. Originally published in 1899.

²¹ English translation of the second edition, p. 173.