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ARTICLES		Page
The Demand for Manpower in Malawi	Desh Gupta	1
Education, Race and Employment in Rhodesia A Review of the Review of	M. W. Murphree By S. H. Irvine	13
Gold Mining in Southern Rhodesia 1919/1953	I. R. Phimister	21
Co-operative Marketing among African Producers in Rhodesia	A. P. Cheater	45

EDUCATION, RACE AND EMPLOYMENT IN RHODESIA

**—M. W. Murphree (Ed.), B. J. Dorsey, G. Cheater and B. D. Mothobi,
Salisbury: ARTCA Publications, 1975**

A REVIEW OF THE REVIEW BY S. H. IRVINE

M. W. MURPHREE*

In his review of this book (RJE, Vol. 9, No. 4, December 1975 pp 157/175) Professor Irvine concludes his summary of what he considers to be both its value and its defects with the statement, "Nevertheless it should be read. But not without qualification. And not uncritically". With this I concur completely. No intelligent reader should do otherwise, particularly with a book dealing with a subject of such central significance for this Country's social and economic development.

The same should be said of Professor Irvine's review. It is extensive if not exhaustive, contains points of valuable detail and raises one issue of major analytical significance. It is at certain points highly entertaining, and generous in endorsing certain arguments the book puts forth. Unfortunately the review also contains one blatantly false assertion regarding what the book is alleged to have said and, more diffusely and by implication, attributes other conclusions to the work which it does not make. The result is the creation of a series of straw men which the reviewer then proceeds to demolish with magisterial gusto but which have little if anything to do with the real conclusions of the volume and which are likely to mislead the unwary reader into the assumption that the book presents a perspective and takes a position which, in fact, it does not. The admonition of the review is therefore singularly appropriate to itself — it should be read, but not without qualification, and not uncritically.

Firstly, the gross inaccuracy regarding what the book is alleged to have said. In noting the differences in methodological approach utilized by the two research units involved, the school leaver unit and the occupational structure unit (which the reviewer designates respectively the SLS and the EPS) Professor Irvine comments "Murphree thinks that the difference of approach is essentially the difference between an objective (SLS) and a

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subjective (EPS) method. He errs in this distinction, since both are subjective and empirical". He then goes on to an excursus of how practically any piece of social science research contains its subjective elements, illustrating his point by saying, "The content of a questionnaire, after all, even if distributed to a population of a country (a census form) will reflect the judgement of the people who put it together, using whatever technical aids they think fit."

Now the fact is that I do *not* "think that the difference of approach is essentially the difference between an objective (SLS) and a subjective (EPS) method." Nowhere in the volume do I say this, nor do I at any point make a statement which could reasonably be interpreted as implying this. It would be astonishing had I done so, because my position on this issue is precisely the one that Professor Irvine presents. In fact, by interesting coincidence, I have argued the point with precisely the same illustration as that used by Professor Irvine in my own published work as follows: ". . . it is inevitable that, in the study of such an intricate and complex organism as human society, certain qualitative judgements must be made by the researcher to enable him to order and arrange his data. Particularly is this true with regard to the construction of categories and the placement of data within them. Even in such a relatively "objective" exercise such as the taking of a census is this true."¹

In my distinctions between the two units of study I attempted no comparison of their relative "objectivity". I *did* distinguish between them on the grounds of levels of input of quantitative data (p.10). This implies of course nothing regarding their relative objectivity/subjectivity. I assume that Professor Irvine's scholarship does not lead him to equate quantification with objectivity *per se* or the lack of it with subjectivity. In fact his subsequent section on scale would tend to confirm this assumption, where he says that "No demarcation need be made between the studies on grounds of objectivity. Instead, differences in scale should be noted." But this is of course precisely the position that the book itself takes *passim*, and is clearly implied in the text at a number of points (*vide* especially pp.10, 176, 180).

It is this aspect of the review that I find remarkable, if not novel. One expects a critical review to fasten upon points in the text with which the reviewer finds himself in disagreement and to adduce arguments to the contrary. But here we have an instance in which the reviewer takes a central informing perspective of the book, denies that it is there and then triumphantly produces it out of his own hat. I find this kind of analysis — to use Professor

¹M. W. Murphree, Preface to *The Urban Poverty Datum Line in Rhodesia*. V. S. Cubitt and R. C. Riddell. Salisbury, University of Rhodesia, 1974.

Irvine's own idiom — "a difficult and painful one to record" in the work of any scholar because it is a waste of time — both the writer's and the reader's.

Note that I do not use the qualifying adjective 'senior'. Here I am afraid that Professor Irvine and I will have to agree to disagree on our standards of scholarship in respect to the canons of evaluation we use to assess the work of research scholars. Professor Irvine suggests that the alleged defects of the different sections of the book merit different levels of indulgence on the basis of their respective authors' purported juniority or seniority. With this I cannot agree. The social location of scholarship is, of course, an important focus in the Sociology of Knowledge. The youth of a scholar furthermore may be an important factor in assessing his or her suitability for a particular task. But a book review is surely an evaluation of the work itself — not of those who produced it. The latter is only admissible where it can be demonstrated how, and in what respect, the social location of the authors has influenced the data and analysis produced. This Professor Irvine does not do, and to imply as he does that the alleged juniority, youth or indeed the domestic arrangements of some of the authors have influenced at points the book's analysis and format is to foster an imputation far more tendentious than any Professor Irvine purports to discover in the book.

Turning now to the review's more diffuse tendency to produce convenient windmills at which to tilt, I cite two instances. Professor Irvine slates Messrs. Cheater and Mthobi for their hypothesis regarding the existence of a belief system concerning African work performance and capabilities which inhibits the efficient use of African manpower. Professor Irvine implies that evidence for the plausibility of this hypothesis is not forthcoming and further disputes its causal nexus with the statement, "To impute cause to the belief system, however, is somewhat tendentious. Profit motives seem a much surer cause for African use in low-level positions regardless of educational level . . ." Now in my view considerable evidence to support the hypothesis is to be found on pp.235-245, 269-276, and a particularly apposite illustration is given on p.274. But, to mix the metaphor, the real red herring is in the implications so easily drawn in the statement just quoted that the authors give a monocausal explanation for the effect under discussion and ignore economic factors. This is patently not the case. Cheater and Mthobi argue that the belief system they are discussing plays "a crucial role in rationalizing and maintaining the racial division of labour in the economy." (p.269). They do not argue that it is the only factor producing this effect, nor do they argue that it is primary, assigning it a rationalizing role. (p.276). Furthermore, elsewhere the text explicitly states that "the racially constrained basis for manpower utilization and its resultant inefficiencies is the result of no single factor and is part of a larger pattern of social and power relationships

in the enviroing society." (p.297: see also pp.5-6). It is a refreshing twist to have an educational psychologist arguing that "profit motive seems a much surer cause for African use in low-level positions", but the case for monocausal explanations is not necessarily strengthened thereby. In this connection we all do well to heed the words of Albert Memmi: "Psycho-analysis or Marxism must not, under the pretext of having discovered the source or one of the main sources of human conduct, pre-empt all experience, all feeling, all suffering, all the byways of human behaviour, and call them profit motive or Oedipus complex."²

I cite one other windmill. Professor Irvine claims that a significant part of my analysis takes place within the context of "the sociological equivalent of the nature-nuture controversy." Frankly, I don't know what the "sociological equivalent of the nature-nuture controversy" is; I know of only one nature-nature controversy, the one to which Irvine alludes when he cites Biesheuvel and Jensen. And this controversy, as Professor Irvine should know full well (this is, after all, an area of specific academic interest to him — he has published in a symposium on the subject³) relates to the relative weight that should be given to genetic and environmental factors in differences in performance and ability alleged to exist between different breeding populations. Professor Irvine goes on to assert that all the authors of the book seem to believe that "the nature-nuture controversy about African abilities and skills is resolved, and that this report demonstrates that the balance of evidence is on the nurture side." He goes on to add, "For this reviewer, the nature-nuture controversy is irrelevant."

For me, it is this section of Professor Irvine's review that is irrelevant — irrelevant because it has no relation to the argument of the book itself. My analysis has nothing to say about genetic versus environmental inputs, it deals rather with the attribution of black occupational under-performance to either "cultural deprivation factors leading to a lack of proper motivation and adequate cultural conditioning for effective participation in the entire spectrum of the occupational structure," or alternatively to structural constraints within "the labour market itself, in the recruitment and employment patterns which this market exhibits." (p.293). Professor Irvine calls this the key quote of the whole book. I wish he had read it more carefully, and in the context of the data with which it deals — the select black school-leaving population of Rhodesia at the Form IV and Form VI levels. This is emphasized several times in the immediate context of Irvine's "key quote":

²A. Memmi, *The Colonizer and the Colonized*. Boston: Beacon Press, 1967, p.xiii.

³S. Irvine, "Culture and Mental Ability". *New Scientist*, 1 May 1969, No. 647, Vol. 42, pp.230-231.

“ . . . *African School leavers at the Form IV level . . .* ” (p.393). “Whatever may be said, therefore, of the African work force in general, it cannot be claimed that *the African School leaver population . . .* ” (pp.293-4). And if this is not enough, footnote 1 on p.292 makes it quite explicit that the argument does not proceed along the dimensions of the nature-nurture controversy, alluded to here under the rubric of the ‘Jensen hypothesis’: “This perspective does not however concern us here, since whatever the merits of the Jensen hypothesis may be — and these are in doubt — it refers to comparative statistical averages of various population groups genetically derived while this study is primarily about the African school leavers, a highly select group within a given population group, produced by the attritional process of the formal educational system and of proven academic ability”.

So, whatever is being compared in our analysis, it is not relative heritable components. We are not comparing “black” and “white” age cohorts *per se*, nor are we comparing two school leaving populations in general. We *are* comparing the school leaving populations of two educational systems *in respect to certain specific attributes*, and it is here that I assert that our data implies that they are “comparable” in respect to three things: academic achievement, life-style aspirations and occupational potential. It is with this assertion that Professor Irvine takes issue and it is here that I find his argument, although I do not agree with it, to be of analytical significance. Let me therefore deal with these three assertions *seriatim*, noting that my use of the word “comparable” does not imply identity but rather similarity.

1. **Academic achievement.** Leaving aside Professor Irvine’s red herring of percentages within each age group which I have already dealt with (the analysis is not comparing age groups), his argument against comparability rests on two points: that the subject/quality content of O-level results of the two outputs is different and that the results are the products of two different examining procedures. With regard to the first point, it should be remarked that a number of differences in the order of subject percentage passes and quality of subject passes are clearly noted in the data presented on pages 137-140. If, therefore, we mean by “academic achievement” academic achievement in, say, English language or Mathematics, the results are not necessarily “comparable”. But my assertion related to the aggregate indices covering a range of subjects used by the examining boards themselves, and in this respect the results are “comparable”. As to the second point, which is noted in the text itself, in the absence of concrete evidence to the contrary the two examination systems must be taken to yield results of comparable standard. Contrary to Professor Irvine’s comment, Dr Dorsey’s footnote on this point (p.137) contains no ‘special pleading’, only a statement of fact.

2. **Life-style aspirations.** Here Professor Irvine offers no argument against comparability, so we can pass on to:

3. **Occupational potential.** At this point Professor Irvine complains that I make an inadmissible jump in my analytical equation: academic achievement of a given standard + life-style aspirations of a given standard = appropriate motivation = occupational potential of a given standard. He has a point. It is indeed, as he says, "a long scientific stride from aspirations and goals to a whole complex theory of achievement motivation for Mashona [sic] or Ndebele society." But this is of course true for any theory of achievement motivation for any society, let alone a Shona or Ndebele one. And, given the gaps in our theories regarding achievement motivation generally, my argument is that the similarities evidenced regarding academic achievement and life-style aspirations are at least presumptive evidence that the occupational potential will also be similar, at whatever level. Professor Irvine appears to consider this 'advocacy' and not 'science'. I think it is more than that. For one thing, the evidence in the volume regarding satisfactory post-school occupational performance under appropriate conditions by blacks is stronger than Irvine credits. For another, the analysis is presented in sets of hypotheses: ". . . *implying* appropriate motivation . . ." ". . . *implying* an intellectual development and occupational potential . . ." (p.245). Of course the issue is "an open, empirical question, not an ideological one", and of course hypotheses are the "basis for further scientific enquiry." Such enquiry can and should take several forms. Further studies should be conducted regarding the components of successful occupational performances by Africans; one such is in fact being currently conducted by one of the authors of the volume under review. If the educational psychologists can come up with evidence regarding which elements in formal schooling correlate most closely with occupational efficiency of given types they will sharpen our insights and analysis. And experiments in occupational management isolating and identifying variables must be conducted. This is what, in fact, our analysis specifically suggests (pp.187, 304-306). This may be 'advocacy', but it is also 'scientific', in that it places the focus of much experimentation where it is appropriately located — in the work place.

Fortunately, many 'laymen' in commerce and industry are more highly motivated for such an enterprise than the 'scientists' credit. Professor Irvine, understandably, complains about the size of the volume and the inclusion of extensive tables, commenting that "Much of the material that is of interest to judges of academic merit has no place for the general reader" and suggesting that the SLS findings should have been "severely summarized". Readers should note that it was the laymen sponsors of the project themselves who opted for the inclusion of this material, on the grounds that it should be available for others to adduce different analyses if appropriate.

Let me conclude with a more positive note regarding Professor Irvine's review. He draws valuable attention to two tabular errors. One is the result of a typographical error in which one digit in a column addition is omitted, the other an error in transposition from draft copies. As editor I of course accept full responsibility for these inaccuracies. I hasten to add, however, that in neither case do these errors invalidate the analyses given to the tables concerned any more than — if I can pull his leg for a moment — does Professor Irvine's arithmetical gaffe ($XIX + 478 = \text{five-hundred-odd pages}$) invalidate his contention that the volume is a very large one. On this point Professor Irvine and I are in complete agreement. It is a long book, and it may well be "doomed, in its present form, to the fate of a Solzhenitsyn novel. Everyone will talk about it, but few will be able to stay awake long enough to read it at bed-time." But then, as everyone knows who has read him, Solzhenitsyn's novels were not written to entertain. Neither was this book.



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