SECOND CARNEGIE INQUIRY INTO POVERTY
AND DEVELOPMENT IN SOUTHERN AFRICA

REJOINER TO SIMKINS ON TRENDS IN HOMELAND
INCOME DISTRIBUTION AND POVERTY

Dr. Mike McGrath
and
REPLY TO DR. McGRATH'S CRITICISM

Charles Simkins

Post Conference Series No. 4

Cape Town
March 1985
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1. INTRODUCTION

Simkins's simulations on income distribution in the homelands attracted a great deal of attention during the Carnegie Conference (1984a), and subsequently have even been referred to with approval by the Prime Minister. (Hansard 1984, Col. 5356). At the Carnegie Conference, Simkins's findings were received by the majority of Carnegie participants either with scepticism, or open hostility, although the eminent English economist, Wilfred Beckermann, publicly defended Simkins's methodology. Unfortunately there was insufficient time during the Carnegie week to prepare a careful rejoinder, which could pinpoint the areas of contention in Simkins's paper, and attempt to determine whether its findings are analytically robust. This post-Carnegie paper addresses these two important issues.

2. METHODOLOGY

Simkins uses a technique of simulation to construct a distribution of homeland incomes for 1980, and he then simulates the 1970 and 1960 distributions. Estimates of poverty and of percentile incomes are made from the simulated distributions.

He describes his findings as being 'provisional' (p.14). This should not be interpreted to mean that 'finalised' results will sometime become attainable, for the income survey data which exists for the 1960s and 1970s is inadequate for making estimates of the actual distribution of homeland incomes. Even in 1980 income survey data was available for only four homelands, although this data is probably fairly representative of all the homelands. One must be continually aware when reading Simkins's paper that its results are based on hypothetical distributions of both households and their incomes, and these hypothetical distributions may at best capture a trend. There is, however, no conclusive empirical means of testing the validity of the results. An appraisal of the results of these simulations must, therefore, fall back on an analysis of the assumptions which are made by Simkins. He is cryptic when describing his procedures, and is sometimes completely negligent, or often telegraphic beyond comprehension, when describing his sources.
of data and the origins of his assumptions. This is unfortunate, for the validity of his predictions lies wholly in the validity of the assumptions.

3. THE ASSUMPTIONS

The most important assumptions which can be distilled from his paper are listed below. Assumptions are stated in italics, and comment follows after each.

(1) The file of homeland households which he had generated earlier is adequate as an input in his simulations. (p.2)

The procedure which he followed in deriving these estimates are not discussed, but let us accept that these simulated distributions will not be the source of major biases.

(2) Children under 15 are equivalent to one-half of an adult-equivalent consumption unit, whereas adults over 65 years count as two-thirds. (p.2)

These ratios are stated without any acknowledgement of their sources. They are used to convert the simulated distributions of household incomes into distributions of adult-equivalent incomes per household. Standardisation is necessary to compare meaningfully the incomes of households of different sizes, but the extent of poverty which is measured will inevitably be partly determined by the adult-equivalent scales which have been chosen. The trend in the distribution of incomes would however, not be affected, unless the age composition of households in the various income percentiles had changed over time.

(3) All males, or females, getting a given type of income get the same income. (Used in his treatment of transfer payments, remittances, agricultural incomes and incomes of other sectors. pp.2-9).

It is easy to give examples based on survey data which reveal the dubious nature of this assumption. For example, the Bureau of
Market Research (BMR) reports annual incomes for male earners in Pietermaritzburg in 1976, which ranged from an average of R930 for unskilled construction workers, to R4 138 for a professional in social and personal services. (BMR 1976 Table 12m). Undoubtedly there is wide variation in agricultural output per plot across homelands, and in the earnings of commuters, migrants and homeland public sector employees. BMR survey data exists on the distribution of urban incomes for years close to 1960, 1970 and 1980, and this data could have been used to generate alternative and possibly, more appropriate assumptions about the distribution of commuter earnings. BMR data also shows that remittances received vary considerably across areas. For example, in Venda in 1977 average cash remittances received varied between R161 in Sibasa to R227 in Dzanani. (BMR 1978, Table 3.1).

The relaxation of the assumption that all persons getting a given type of income receive the same would have unquestionably required a great deal of work, but it would have added greatly to the credibility of the results. In each simulated distribution it might have widened inequality, working to increase the proportion of the simulated distribution in poverty.

Inequality in the distributions of commuter incomes increased between the 1960s and 1980 (McGrath 1984 pp.26-27), and over these years migrant incomes (and possibly remittances) may also have become distributed over a wider range, because of expanding job opportunities in the labour market, for those migrants who were able to enter the urban labour market. These changes should have been incorporated into his simulations. They would probably have widened inequality in the 1980 income distribution, and may have depressed the income growth, which he reports, for the middle ranges of the homeland income distribution from 1960.

(4) There is no correlation between individual chances of receiving income from various sources. (p.2)

It is also easy to find survey evidence which brings this assumption into doubt. For example, the average level of remittances
is negatively correlated with average wages in three magisterial
districts in Venda for 1977 (BMR 1978 Table 3.1), and a similar
relationship holds for three of four magisterial districts in
Kangwane (BMR 1980 Table C1). This is not a surprising result
for household incomes are usually the result of a series of inter-
dependent choices. The need to seek employment as a migrant, and
the proportion of earnings remitted, may well be related to wage
levels and employment opportunities near the potential migrant's
home, and on the agricultural output of the household. If these
forms of income are negatively related the actual distributions
will be more equal than has been simulated.

(5) Conditions as regards transfer payments in the homelands bore the
same relationship to conditions in South Africa in 1960 and 1970
as they did in 1980. (p.4)

In the last decade homeland governments have taken over the adminis-
tration of pensions, and there have been reports of an inability of
some of these governments to pay all who made claims. If the pro-
portion of eligibles receiving benefits in the homelands has fallen,
then the Simkins results will understate the levels of income in
earlier years, and overestimate the rate of growth of incomes. Here,
once more, careful research should have been devoted to substantiate
the assumption.

Simkins regards the proportion of people receiving pensions as
shown for 1970 to be too low (p.4 Table 2), although he does not
specify his source of data. He rejects this 1970 proportion and
instead manufactures a number from the average of the 1960 and
1980 observations. His 1960 and 1970 data appears to be drawn
from the official South African Statistics (1980 Table 6.2), and
the reader is surely owed some explanation for his summary rejec-
tion of the published data for the year 1970.

(6) An implicit assumption is that migrants earn the same average wages
as non-migrants. (p.5 Table 3).
For recent years this assumption is also falsifiable, as 'urban insiders' have gained ascendancy in access to jobs. Again we find ourselves asking for a more carefully researched assumption, for it affects both his estimated remittances, and the relative levels of commuter incomes, and could result in an overstatement of growth in incomes of households in the non-metropolitan regions of the homelands, thereby understating the extent of poverty.

(7) Remittances for 1960, 1970 and 1980 flowed in the proportion of:

(i) Spouse to spouse: 35 per cent of earnings with probability 0.75.

(ii) Unmarried children to parents: 20 per cent of earnings with probability 0.5.

(iii) Parents to children lodged with kin in the homelands: 15 per cent of earnings with probability 0.75.

Simkins also assumed that if the remitter were female, remittances would be reduced by one-third and if the remitter were in rural areas outside the homelands the remittances would be reduced by one-half.

No attempt is made to justify individually these critical assumptions, and no indication is given about where they originated. One is tempted to ask why he did not experiment with other probabilities, such as the often made assumption that 20 per cent of migrant incomes are remitted with a probability of 1.

Simkins shows that these assumptions generate a mean level of remittances which is relatively close to the mean found in the BMR's surveys (p.6). The same result could, however, been achieved using many other assumptions about remittances, for there can exist many possible combinations of the proportions of income remitted, and probabilities of making a remittance, which will generate the identical expected value. He notes that the average proportion of remittances (25.6 per cent of income) over the three years of his estimates exceeded the BMR's average of about 20 per cent of income (p.6). Why then did he not adjust his proportions of incomes remitted downwards towards the BMR's proportions?
The 1980 Census estimates of men and women involved in subsistence agriculture is assumed to be underestimated by 100 per cent, and the number of women by 50 per cent. (p.7)

No indication is given of how his estimates of the agricultural workforce in 1980 were made, except for the assertion that:

'The numbers given by the 1980 Population Census are very much lower than the 1960 and 1970 estimates and imply a huge increase in per capita agricultural production in the subsistence sector, for which there is no corroborating evidence. The 1980 figures for the number of men involved has therefore been doubled and the number of women increased by 50%.' (p.7)

(9) He assumes for commuters that:

(i) Male commuters in non-community and personal service sectors earn 90 per cent of the average wage outside agriculture and domestic service.

(ii) Non-commuters earn half of the commuter wage.

(iii) Workers in the community and personal service sectors earn two-thirds of their counterparts in the other sectors.

(iv) Female workers earn two-thirds of the male wage.

(v) The proportions of commuters were the same for men and women, and for service and non-service workers. (p.8).

In a later version of the Carnegie paper he provides evidence and argument based on the 1980 Census to substantiate these assumptions (Simkins 1984b p.141), but makes no attempt to examine whether these relationships have remained unchanged over time. In addition he assumes:

(vi) The proportion of commuters to non-commuters in the 1960 population was the same for 1970. (p.8)

Umlazi was excised into KwaZulu between 1960 and 1970 and in 1970 it constituted the largest urban area in the homelands. The proportion of commuters in the homelands must therefore have increased between 1960 and 1970, and this would understate the growth of
incomes in Simkins's simulations for these years, and would also understate the extent of poverty in 1960.

In Simkins's estimates males are 71 per cent of commuters in 1970 (p.6 Table 6). However, a footnote to the BENS0 table from which his 1970 total of commuters was derived, states that males represented approximately 80 per cent of the total. (BENS0 1976 p.47). In terms of his other assumptions, this would have understated commuter incomes in 1970, and will overstate the growth of incomes above the 30th percentile over the 1970s.

4. TESTING HIS RESULTS

He compares his simulated income distribution for 1980 with an estimate of the homeland distribution made from BMR data, and claims from a casual perusal of these cumulative distributions that their 'correspondence is quite good'. A Chi-squared test, however, shows that they are significantly different at the 95 per cent confidence level. Support for his 1980 findings does not therefore seem to come from the BMR's homeland data.

5. THE ESTIMATION OF POVERTY AND OF GROWTH IN REAL INCOMES

Simkins uses an urban MLL to estimate the proportion of households in poverty. This is a very unsatisfactory approach, for it ignores differences in consumption patterns and living costs between regions and between urban and rural areas. In 1975 the MLL in Durban for an average-sized African household was R1 134 per annum, or R206 per capita. The BMR's MLL for rural areas in 1975 would have been approximately R880 per annum, or under R150 per capita. Simkins's failure to take account of regional variations in the MLL will exaggerate the extent of poverty in all years, but the trend may not be affected.

Alternative estimates, based on the distribution of incomes of African multiple households in 1975, using an urban and a rural MLL, show that approximately 62 per cent of African households in the Black states (excluding the overlapping segments of the Black states) were in poverty. In this distribution household incomes in urban areas, outside the metropolitan regions are included, and if these incomes were excluded
the incidence of poverty in the remaining rural areas of the Black states would be increased. In the same year approximately 30 per cent of African multiple households in urban areas fell below the MLL for African multiple households in Durban (McGrath 1984, p.6).

Incomes are inflated from 1960 and 1970 to constant 1980 prices, based on the South African consumer price index. Some caution should also have been taken here too, for relative living costs (particularly commuting costs) could have changed because of relocations and removals necessitating the use of price indices with appropriate expenditure weights.

6. OTHER VARIABLES

Simkins does caution that the interpretation of his results must be influenced by resettlements, and by the redrawing of metropolitan boundaries over the period 1960-1980, but he provides no estimate of the magnitude of the effects of these changes. This is a severe limitation of his results. One asks how much of the worsening plight of homeland households in the lowest decile of his hypothetical distributions might be due to these removals? On the other hand, the addition of urban metropolitan areas to the homelands is bound to have increased the relative incomes of the upper deciles of the homeland population, for in the mid-1970s the incomes of households in the metropolitan regions was more than twice the incomes of the homelands, after excluding the overlapping segments of the metropolitan regions. Again the reader asks with justifiable frustration: how large an effect have these incorporations had on the distribution and level of homeland incomes?

He also ignores earnings from informal activities outside of agriculture. For 1977 non-market production excluding agriculture in the homelands has been estimated at approximately R82 million, which is 80 per cent of the value of pensions paid in the homelands in that year, and an amount equal to 11 per cent of the value of marketed production of the homelands. This is a substantial amount of income to ignore, especially if it was earned largely by low income households.
Simkins's use of the technique of simulation is innovative, but he should have taken much greater care in selecting and explaining his assumptions, for by his own admission, he is dealing with a highly sensitive topic.

The assumptions about the distribution and interdependence of commuter incomes, migrant remittances, and of homeland agricultural output, all raise quite serious uncertainties, and in total these incomes account for almost 77 per cent of homeland personal incomes for 1977. It is not clear whether reasonable changes in these assumptions would reinforce the trends adduced by Simkins. Uncertainties also lie in the way he has measured poverty, and in the use of the consumer price index as an income inflator. Ideally he should have chosen two sets of assumptions about his variables, grouping together those assumptions which represented plausible extremes, and allowing for changing probabilities through time where these were warranted. The findings of simulated distributions, based on these high and low assumptions, would have enabled him to appraise the sensitivity of his results. If the trends were unchanged, the credibility of the results would have been enhanced.

Over the period 1960-1980 relatively 'hard' evidence can be produced to show:
- that there was an increase in the proportion of the African population in the metropolitan regions (including those which overlap the boundaries of the Black states);
- that African multiple household incomes in the metropolitan regions grew faster than the national average, reducing poverty in these areas, but widening urban-rural inequalities;
- that inequality in the distribution of African multiple household incomes in certain metropolitan regions increased significantly between 1960 and 1980; and
- that the growth rate of African modern sector employment (excluding domestic service) between 1960 and 1980 was slower than the growth rate of the African population. (McGrath 1984, pp.26, 27);

This evidence is not in conflict with the broad direction of Simkins's simulated distribution.
Nevertheless, without any sensitivity analysis on his assumptions, and with all the problems which lie in them, one is forced to conclude that we really do not know what has happened to poverty and income distribution in the homelands since 1960.

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FOOTNOTES

1. Two alternative distributions could be produced, either a distribution of the adult-equivalent income of households by number of persons per household, or by the number of adult-equivalent units. Each of these distributions would be likely to show a different pattern of inequality, although the trend through time and the level of poverty might not be affected.

2. $X^2 = 20.69$ for 6 degrees of freedom.

3. Estimated for a household of 5,5 members from BMR 1975, p.5.

4. Estimated from BMR 1974, p.5. The 1974 figures are escalated by the increase in the CPI from 1974 to 1975.

5. Estimated from BENSO 1980, Table 48.

6. Ibid, Tables 48, 52 and 103, assuming depreciation is 10 per cent of the GDP, and that migrants remit 20 per cent of their incomes.
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BMR 1978, Income and Expenditure Patterns of Black Households in Venda, Report No. 64.


Hansard, 1984, No. 12, Government Printer, Cape Town.


REPLY TO DR. McGRATH’S CRITICISM

C.E.W. SIMKINS

For the most part McGrath has provided a sober and workmanlike discussion of the issues and I appreciate that. It is debate of this sort that clarifies matters and allows us to see more exactly what is at stake. Such debate takes place all too rarely in South African social science, in part because of the rather small number of people involved in research (and, a fortiori, in any branch of it), in part because researchers are often divided into camps corresponding to cultural and political division in the society as a whole, and in part because the critical arts are too little taught in our educational institutions. These difficulties need to be overcome if social science is to make much of a contribution to South African life in the coming years.

Unfortunately, McGrath's critique contains a couple of lapses from the good manners which ought to be observed when scholars criticise one another's work. I should like to deal with these first. The first allegation is that I have been unscholarly (to the extent of 'complete negligence'), in 'describing (my) sources of data and the origins of (my) assumptions'. (p.2). This allegation is untrue; in fact, it is a fixed principle of mine to attempt to make my work as transparent to readers (and therefore to criticism) as possible. When it comes to technical work, this principle sometimes causes considerable difficulty. One wants to discuss the data base (which is itself never unproblematic in the fields of my interest) and to expose the technical apparatus, but this sort of discussion may threaten to obscure utterly the main lines of one's argument and hence the readability of one's presentation. Devices such as notes and appendices help, but in the end a balance has to be struck. I deny that the balance in the article is wrong, let alone negligent. Every single empirical datum introduced into the article (mostly in Tables 1 to 6 and note 1, but also now and again in the text) is referenced by source, either in

* I appreciate being given the opportunity by the Editor of Development Southern Africa to reply to Dr. McGrath's critique of my article 'What has been happening to income distribution and poverty in the homelands?' (Development Southern Africa, Vol. 1 no. 2). This article was originally written for publication in Development Southern Africa.
footnotes to tables or in the text itself. The referencing is sufficient to enable the professional reader to go to the sources and to ascertain for himself whether the information has been correctly extracted and correctly interpreted. The statement of assumptions is equally clear; indeed the whole format of McGrath's critique demonstrates that it is possible to extract them accurately and subject them to critical examination. All this being so, where is the reasonable ground for the complaint?

The second allegation is more interesting. It is also fragmentary and has to be constructed from remarks in different parts of the critique. The charge of carelessness is raised again, this time at the level of research. The alleged carelessness is of two types: that I have used crude assumptions where more detailed research into existing data would have enabled me to represent the situation more accurately and that I have built in assumptions not corresponding to the data I have investigated. I shall deal with the second later. McGrath clearly thinks I have put together estimates and published them too soon, and that I have tried to cover myself in an essentially fraudulent fashion by describing these as 'provisional' (pp.1-2) when in principle 'finalised' results are impossible. This is particularly blameworthy given the sensitivity of the topic (p.9). Fortunately, people are not fools and the imposture has 'generally been received either with scepticism, or open hostility' (p.1).

I shall unravel this by starting with the last point. My experience has been that the article has been received in a number of different ways. There certainly has been scepticism or hostility, but these by no means exhaust the variety of responses. It will suffice merely to list the principal things that have been said in quotation form (some 'quotes' have been composed from several observations making essentially the same point):

1. (McGrathian scepticism) 'Your conclusions are not justified by your arguments'.
2. (Political hostility) 'Your publishing this paper is a political action and you are responsible for the political consequences'.
3. 'This paper is a professional piece of work and if anyone thinks he can do better, let him try'.


4. 'These results are (or may well be) true, but you shouldn't have made them public'.
5. 'If that is what the figures show, you were right to say what you did'.
6. 'I am pleased that you have said it. This enables me to say more easily things I am finding in my research'.
7. 'Sheer media grabbing'.
8. 'But it is possible to piece together a broad picture right now. This means starting with the great Charles Simkins controversy'. (Helen Zille, Frontline, July 1984).
9. 'Others objected because they saw a serious contradiction between Simkins' conclusions and a large body of on-the-spot research telling of increasing poverty and destitution, particularly in the rural areas'. (Zille)

The next issue is: In what sense can my results be called 'provisional' and how can the publication of provisional results be justified? As set out in the thesis from which this article is drawn, I have been engaged for some years in assembling a demographic-economic model simulating the evolution of Black households in respect of several variables both historically and into the future. At this stage I have got the demographic side into final form. And, considering the matter in its historical aspect, the general point that McGrath makes is true: once one has brought the limited available data into account and incorporated them into the model, one sees that there is inevitably a degree of imprecision in one's results and a series of questions one can never answer. Nonetheless, had I been discussing demographic variables alone, I should not have talked about provisional results; rather I should have offered the best account that I could under the circumstances and left it at that.

The case is not the same with the economic variables in either their relation to the demographic variables or to each other. It is here that I have made use of simplifying assumptions, which I know can in due course be replaced by more detailed accounts as research proceeds.
I was not unaware of the issues raised by McGrath at, for instance, paragraphs 3(3), 3(4) and 6; indeed, I am aware of a number of considerations he did not raise. What counted in my decision to analyse the problem in the way I did was the knowledge that:

(i) very often in any field of applied mathematics, be it, for instance, physics or economics, a useful approach to problems is a method of successive approximations. The first approximation is often based on fairly rough but easy to manipulate assumptions; nonetheless, depending on the skill of the researcher, even the first approximation can get one a long way. Second and subsequent approximations incorporate refinements. My results are a first approximation, a first bite at the cherry. Subsequent bites will be taken as circumstances permit and one fully expects that one's detailed understanding of conditions will change as a result. Nonetheless I do not expect that the broad outline of results will be changed. I shall emphasise and develop below certain aspects of my article in order to consolidate this view.

(ii) McGrath sees that the implementation of his recommendations would 'unquestionably require a great deal of work' (p.3); add to it the necessary further tasks which I can see, and one arrives at several years of work. The project is my small equivalent of the tomb of Julius II and progress is liable to be hampered by self-willed diversions or commissions to elaborate on what one had been doing in one small corner. It is also unlikely to be completed according to the full original specification. That does not ultimately matter, as work on each component of the model yields its share of insights. But these considerations taken together add appeal to the successive approximations approach.

And here we are at the heart of the matter. McGrath does not think that my provisional, first approximation results advances knowledge at all. What can I say to convince him otherwise?

The core of my position is that easily the majority of homeland households (over two-thirds) have improved their position rapidly in real terms. Of
the rest half have improved their position in real terms but more slowly, and half have suffered a real deterioration. There are two alternatives to this view. The one is more optimistic and proposes that even the minority absolute and relative deterioration is overstated. But, by and large, people believing me wrong have not wanted to advance this view, so I shall leave it aside. The other is more pessimistic and proposes that the people who have improved their position is real terms are a smaller group than I suggest and even a minority; the remaining households' real position has been static or deteriorating. Can this view be made more plausible than mine?

Table 1 shows points per adult equivalent (cf. Table 8 of the article) in the homelands in 1960, 1970 and 1980. Annual (real) growth rates are reported and are compared with BENSO estimates of growth in real GNP per capita.

<table>
<thead>
<tr>
<th>Date</th>
<th>Points per adult equivalent</th>
<th>Period</th>
<th>Annual real growth (%)</th>
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<td></td>
<td>Points/adult eq.</td>
<td>GNP/capita</td>
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<tr>
<td>1960</td>
<td>1,09</td>
<td>1960-1970</td>
<td>3,5</td>
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<td>1970</td>
<td>1,54</td>
<td>1970-1980</td>
<td>6,1</td>
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<tr>
<td>1980</td>
<td>2,78</td>
<td>1960-1980</td>
<td>4,8</td>
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Sources: BENSO, Statistical Survey of Black Development 1980, Table 50 and BENSO, Statistical Survey of Black Development 1982: Parts I and II.

For reasons discussed in the article the BENSO magnitudes and mine are not the same, but they ought to grow at similar rates and Table 1 conforms this. Table 1 also shows the difficulty confronting the pessimistic thesis. If BENSO and myself are even only approximately right, incomes per receiving unit (however defined) rose fast on average (from a low base, but that is not relevant here) for a period of twenty years, and in particular in the
seventies. Unless this contention is refuted, one is obliged to believe, if one argues that real rises in income have been confined to a minority of households, that relative inequality between households in the homelands has risen to astounding heights. A simple calculation will suffice to demonstrate this.

Contrast the position in 1960 and 1980 and compare the position of the richer 50% of the households with that of the poorer 50%. Measure relative inequality by dividing the income of the richer 50% by that of the poorer 50%. Consider two situations:

(i) the position as derived in my article;
(ii) a hypothetical case where the bottom 50% got no better off on average between 1960 and 1980. This is the kindest limiting case of the pessimistic view.

Table 2 sets out the results of this calculation.

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<tr>
<td>Rich 50%</td>
<td>73,6</td>
<td>79,5</td>
<td>89,6</td>
<td>2,79</td>
</tr>
<tr>
<td>Poor 50%</td>
<td>26,4</td>
<td>20,5</td>
<td>10,4</td>
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Notice that I assert that relative inequality has risen in the homelands, according with several commentators who have said the same thing. The alternative hypothesis, however, requires a truly implausible rise, establishing the case against it by *reductio ad absurdum*.

Are there less statistical, more economic arguments to provide further support for my results? There are demographic grounds for asserting that there are strong links between metropolitan areas and homelands via both migrant remittances and commuter incomes and that through these links numerous homeland
households have participated in rising real metropolitan wages. On top of that I argue in an article intended for the next issue of Acta Juridica that once differences in the sex and industry mix of employed Black people are allowed for, and average educational levels taken into account, the regional (metropolitan, urban, rural, homeland urban, homeland rural) differences in earnings are small, much smaller than they are often taken to be, and that the worst place to be is in rural areas outside the homelands, not in them. If this is so, changes in metropolitan conditions will induce changes elsewhere, strengthening the effects on the homelands.

It is also the case that homeland residents have increased their share of modern sector employment. This has, of course, been partly due to incorporation of metropolitan townships, but not wholly. This increase forms an important component of the total increase in income. Also, real agricultural production has risen and transfer incomes (though always less than 5% of the total) has risen fast. However the case may stand in the 1980s, there is no evidence of significant difference in the treatment of Black people inside the homelands and those outside when it comes to rates and coverage under the categories of payment discussed in the article. In short, there have been rises in the four components of income discussed in the article (transfers, remittances, agricultural production and earnings outside homeland agriculture) mainly because of the integration of the homelands in the wider South African economy and secondarily because of state policy in respect of unrequired payments. Households according to their situation are differentially affected by changes in any one type of income; improvements in all four therefore enhance the chances of widespread diffusion of gains.

There are a great number of detailed points raised against me by McGrath. I shall reply to five of the more important. Silence on the rest does not imply concession on my part but merely the supposition that this reply is already long enough.

The points are as follows:

(i) My remittance assumptions are unjustified. As McGrath points out the hardest information available for calibration are the Bureau of Market Research's surveys which indicate that persons in hostels in metropolitan areas remit on-average 20% of their income. I think that this proportion is somewhat too low for two reasons:
(a) not all persons in hostels are contract workers with important
family links in the rural areas or the homelands. Some are genuinely on their own; others have families in the metropolitan areas themselves. For these groups, remittances are not relevant or are likely to be lower than those for contract workers with homeland links. Accordingly the average figure will be a little lower than the figure for the group of relevance to my article.

(b) The Bureau of Market Research figures capture the flow of money to the homelands. Resources by way of goods are also transferred (especially when migrants return to their families) and it seems wise to allow for this. Accordingly, when my assumptions produced an average of 25.6% of income I saw no reason to make an adjustment. An addition of 28% to the Bureau of Market Research figures seems the appropriate order of magnitude in the light of the considerations above.

It remains for me to state that the individual assumptions are based on my judgement in the light of some of the anthropological literature, results obtained by the Surplus People Project, certain Transkei material, some work in progress at the University of Cape Town etc. Since I was producing a single point estimate, it was necessary for me to produce the best first approximation I could. I am certain that differentiation by sex, by type of relationship and by region of work along the lines indicated is a better first approximation than 20 per cent of migrant income remitted with a probability of 1, as McGrath suggests.

(ii) My agricultural employment estimates are arbitrary. When I wrote the article I had only the Population Census estimates of agricultural employment in non-independent homelands. Assuming that the ratio of agricultural employment to population in the independent homelands is the same as that in the non-independent homelands, a total homeland agricultural employment figure can be derived which has the undesirable properties discussed in the passage cited by McGrath. Hence the upward adjustment, producing an agricultural employment of 413 600 for 1980.
The 1982 BENSO statistical survey has published 1980 census figures for the independent homelands as well as the non-independent homelands. From Part I Table 16 and Part II Table 8 it can be calculated that total agricultural employment is 767 200, which I certainly now accept as the best estimate. My adjustment was in the right direction but too cautious.

Note that the error does not affect the total agricultural product estimate; it merely causes it to be divided into a smaller number of individually larger incomes. To that extent it produces an overstatement of inequality.

A quite interesting political point emerges from all this. Three of the four independent homelands (Ciskei is an exception) continue to have large (in employment terms) agricultural sectors relatively insulated from the metropolitan economy. By contrast, the agricultural sectors in the non-independent homelands have become small (KwaZulu is a partial exception); non-independent homelands are much more bound up with the metropolitan economy. On this account alone, independence can scarcely seem desirable to anyone in them.

(iii) Insofar as my results are testable, they fail the test. In particular a $X^2$-test applied to test the goodness of-fit of my results to a Bureau of Market Research distribution shows the fit is bad. I have two objections to this:
(a) McGrath has made technical mistakes in his application of the $X^2$-test. There are 5 degrees of freedom in Table 7, not 6. Worse, although he has correctly decumulated the table before applying, he has simply taken the percentage distribution that I have derived rather than the raw frequency counts in each cell. This would only be justified if my sample size had been 100. It wasn't - it was much larger.

(b) Nonetheless, a correctly applied $X^2$-test would certainly lead to a rejection of the null hypothesis at the 5% level of significance. But what is the null hypothesis and is it the hypothesis that we want to test?
The null hypothesis that would be involved here is that my synthetic (not natural) sample comes from a hypothetical distribution defined by the Bureau of Market Research results. But we know in advance it does not; it comes from a first approximation model which should roughly reproduce the Bureau of Market Research results (which are themselves based on a sample, therefore subject to sampling error. There are also differences in what is being measured, as indicated in the article). The $\chi^2$-test tells us nothing useful under these circumstances. What is helpful is to describe the relationship between the distribution which I continue to describe as quite good, though not perfect.

If one draws Lorenz curves for both distributions, one finds that my Lorenz curve lies below that of the Bureau of Market Research for a little more than the first quartile of the distribution. It lies above the Bureau's curve for most of the rest of the distribution, the two curves coinciding over most of the last decile. As reported, my Gini coefficient is lower than that of the Bureau. I am inclined to think my methods understate inequality in the middle range to some extent because of my 'equal receipts' assumption at various points. I do not believe that this understatement is nearly large enough to render my article valueless; I also think that better fits will be found when assumptions are refined in a second approximation phase.

The first step in attaining statistical wisdom is to learn when to apply elementary tests correctly; the second step is to learn when not to apply them.

(iv) My application of an urban MLL throughout the homelands is a very unsatisfactory approach to estimating the proportion of households in poverty. This is debatable. I was aware when I wrote the article that the Bureau of Market Research had produced rural as well as urban Minimum Living Levels and that the former were lower than the latter, as McGrath states. I was also aware that the Bureau of Market Research assumes that part of the bundle of goods and services that go to make up the Minimum Living Level has to be purchased on the market in urban areas but is produced by the household itself in rural areas. This
non-market production is not valued for the purposes of calculating the rural Minimum Living Level, so simply to accept the figures McGrath suggests is to conceal an important aspect of the situation. Indeed, were exactly comparable baskets of goods and services to be valued properly in urban and rural areas, these are grounds for believing that the rural basket would cost more, since prices of many essential commodities are higher out of town. For these reasons I decided to apply a uniform standard. This is not to say I think that there can never be a case for departing from it, but merely at the present state of knowledge (including McGrath's critique) the case is not strong enough.

Another point is of relevance here. It makes no sense to ask what the level of poverty 'really' is. It makes no sense because poverty lines are conventionally constructed and within limits one is free to adopt the convention one pleases. For instance, I might have chosen Professor Potgieter's Household Subsistence Level to define my poverty line, or either of the 'effective' levels currently available, the Supplemented Living Level or the Household Effective Level. What is important is that once one has selected a line (as a benchmark, as it were) one sticks to it and measures regional variations and progress or retrogression through time in terms of it.

(v) My results lack credibility because there is no sensitivity analysis. In reply to this, firstly a technical point: in a deterministic model, an analysis of the effects of a set of alternative assumptions involves one set of calculations. In a stochastic model that is not so; starting the process with a different random number seed will produce a somewhat different set of outcomes, the variation in outcomes being controlled by the sampling error in the model. Disentangling the alternative assumption effect from the sampling error effect poses a problem not present in deterministic models. Techniques for tackling the problem exist, but the analysis is complicated.

Given this fact, it seemed to me not worthwhile to carry out a sensitivity analysis or provisional results, but to reserve investigative effort for
a future refining of the economic assumptions of the model. For the
time being, in order to reinforce the credibility of my results against
at least one alternative account, I refer readers to the statistics in
Tables 1 and 2 above and the argument surrounding them.

My aim in discussing these five points has been to show that I have not
been nearly as thoughtless in my work as is suggested by McGrath. I
wish to conclude, however, with a note about the political implications
of my article.

It seems to have been widely assumed by left and right alike that the
article has conservative implications, even to the point of justifying
a whole range of state policies towards Black people. It isn't so.
South African society at present suffers from pervasive discrimination -
not reasonable differentiation, but unjust, hateful discrimination - in
the allocation of state welfare expenditures, in private sector employment
and payment practices, in residential segregation which assigns some
people to squalid and crime-ridden environments and others to well-kept
and orderly suburbs. If future South Africans are to live at peace,
this discrimination must be removed. It will take all our courage and
energy to remove it. It therefore is of great importance that we tackle
real rather than imaginary problems. This defines a task for South
African social scientists: to undertake the innovative, thorough and
sustained work necessary to distinguish the real from the imaginary and
to publish truthfully what they find for the consideration and criticism
of all sections of the community. If our intellectual life has any
vigour, error should come to be seen for what it is; what passes the
test enters the tradition. I hope these views will commend themselves
to a large and growing number of South Africans whatever particular
affiliations; for my own part, the production of work in terms of these
norms is the contribution I can best make to the realisation of the common,
just society I desire.