

1 Introduction¹

There is remarkably little 'hard' data on how living standards in Africa have changed during adjustment. In particular, there are few countries with nationally representative household consumption surveys conducted at two or more points of time during the period.² This helps explain the cautious language used in a recent high profile World Bank report: 'the poor are probably better off and almost certainly no worse off' as a result of economic reforms (World Bank 1994). Here we use the example of Uganda to show the problems of comparability that may arise even where two such surveys do exist. Where surveys have very different designs, they are unlikely to be comparable without adjustment and, in the case of Uganda, we question whether they can be reliably compared at all. It is not surprising that survey design matters: what is striking about the Ugandan example is just how sensitive the results seem to be.

The problem in the Ugandan case can be explained simply. There are two large official household surveys available: the Household Budget Survey (HBS) of 1989/90 and the Social Dimensions of Adjustment Integrated Household Survey (IHS) of 1992/93.³ Official survey reports estimate that mean household consumption rose by 56 per cent in nominal terms between the surveys (Republic of Uganda 1994a). However, this increase is much less than the 135 per cent rise in the CPI during the same period. Together, the figures imply a 34 per cent fall in real household consumption over three years. By contrast, perhaps the best estimate made before

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² A World Bank workshop on poverty monitoring in Africa identified only four besides Uganda: Côte d'Ivoire, Ghana, Kenya and Nigeria.

³ The HBS covered around 4,500 households; the IHS around 10,000.

Problems of Measuring Changes in Poverty over Time

The Case of Uganda 1989–92

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comparison of the surveys is provided by the national accounts. These put real private consumption per capita in 1992 at 3 per cent higher than it was in 1989 (Republic of Uganda 1994b). Clearly something is wrong: either the Ugandan economy suffered a previously unobserved collapse or there are major problems in comparing the surveys.

Central to our interpretation is that comparability problems may have arisen because of the very different objectives of the two surveys. The HBS was a detailed expenditure survey, aimed at providing weights for the CPI for individual items. Only minimal non-consumption data was sought. By contrast, the IHS was a general purpose survey that also covered health, education, time use, income, fertility, mortality, assets and anthropometrics. Although the IHS was part of the World Bank Social Dimensions of Adjustment survey programme, it appears to have been seen as a benchmark survey for future Monitoring Surveys. Comparability with the HBS was apparently not raised as an objective at the planning stages. After its execution, there seems to have been a change in donor perceptions with the Bank and others viewing the main (or most immediate) use of the survey as being to measure changes in poverty over time by comparison with the HBS. Perhaps the main lesson of the Ugandan experience is that comparability of surveys over time is important, but cannot be guaranteed unless considered at the design stage.

The rest of the article has three sections. Section 2 details various adjustments proposed by the survey collectors to reconcile the two surveys. The adjustments bring mean living standards in the two surveys much closer but still imply a 6 per cent fall. We also show what the adjusted figures imply for changes in poverty and inequality. Section 3 appraises these adjusted figures, beginning by surveying what little other evidence there is. It then shows that the adjustments point to disturbing discrepancies in survey estimates arising from differences in household size, recall periods and budget shares. It is argued that without knowing more about the causes of these differences, it is not clear whether the adjustments are warranted. Moreover, even if warranted, they are unlikely to be sufficient to ensure comparability. The concluding Section 4 identifies some general lessons provided

by the Ugandan experience for those designing or analysing household consumption surveys to measure living standards and poverty.

2 Adjusting the Data

2.1 Implications for mean living standards

The official report for the IHS was published even though the data implied a rise in nominal consumption since the HBS was far below the increase in the CPI. Subsequently, various adjustments to the IHS figures have been proposed to ensure comparability (Gupta 1995). This situation is revealing because it makes public the kinds of modifications often made before the release of official statistics. In Section 3 we consider the implications of the adjustments for the reliability of the data. Here we show how their extent and implications for living standards, poverty and inequality.

We start with mean monthly consumption per household as calculated in the IHS official report (Table 1 refers). The first adjustment is for the difference in sample coverage. Seven districts in the North and Eastern regions were excluded from the HBS due to insecurity. From now on we restrict our comparison to those districts covered in both surveys (we have no information on how living standards have changed in the other districts). Since the seven districts omitted from the HBS were generally poorer, excluding them from the calculation for the IHS increases mean monthly consumption per household (by 6 per cent). The next two adjustments are for the omission from the IHS figures of certain items included in the HBS. The first is non-consumption expenditure, such as weddings and funerals for non-household members. Including this and the 'other expenditures' item in the IHS raises consumption by around 3 per cent. Due to an oversight, the IHS omitted an item code for public transport fares. One adjustment is to assume that this led to 1.64 per cent of consumption being omitted (this being the budget share of public transport fares in the HBS).

The official survey reports used 30 day recall estimates of purchases of food, drink and tobacco. However, purchases were also recorded for shorter recall periods. For the HBS, these (seven-day recall)

Table 1 Adjusted comparison of mean consumption

	HBS 1989/90	IHS 1992/93	% Change
Mean consumption per household (USh./month) calculated as in official report	34,428	53,490	55
Incremental Adjustments:			
1. excluding seven districts	" "	56,451	64
2. including non-consumption expenditure	" "	58,293	69
3. adjusting for public transport fares	" "	59,264	72
4. with short recall estimates of food purchases	34,319	64,155	87
5. revaluing home consumed food at retail prices	36,032	65,569	82
6. per capita	7,800	16,674	114
Memo Items:			
Household size	5.45	4.77	-12
CPI	100	235	135
Fisher price index	100	227	127

data do not differ much from the 30 day recall figures. For the IHS, the shorter recall period was the time between the first and second visits by the interviewer. On the assumption that this interval was seven days, this data gives a 25 per cent higher estimate of purchases of food, drink and tobacco than the 30 day recall data. Using this estimate raises total household consumption by 8 per cent.

Much food is consumed out of home produced stocks but this was probably valued at farm-gate prices.⁴ To aggregate them with purchases, they need to be revalued in retail prices. Estimates of farm-gate and retail prices can be obtained from the unit values for home consumed food and purchased food respectively. Revaluing at retail prices increases the value of home consumed food by 10 per cent in the IHS and by 16 per cent in the HBS. Such a narrowing of the gap between retail and farm-gate prices is to be expected given the improvements in security and transport during the period. The revaluation is the only adjustment that raises consumption in the HBS relative to IHS.

The largest adjustment comes from looking at per capita rather than per household consumption. Reported mean household size fell markedly between the surveys, by around two-thirds of a person. After all the adjustments made previously, mean consumption per capita rose by 114 per cent between the surveys.

This increase in nominal consumption is still less than that of the CPI. However, the CPI draws on prices collected only in major urban centres. Since around 90 per cent of Ugandans live in rural areas, this may not be the appropriate deflator. To explore this, food price indices were constructed from unit values for purchases recorded in the surveys. Different prices were calculated for each of eight regions at two monthly intervals of each survey. This allows for regional price differences and for inflation within the periods of the surveys (which was substantial). A similar exercise was not possible for non-food items, so the CPI non-food price index was used for these and assumed to apply to all regions. A resulting Fisher 'ideal' price index implied a 127 per cent increase in prices between

⁴ According to the Statistics Department, field-workers were instructed to value home consumed food at farm-gate prices, but this instruction is not in the survey documentation.

the surveys (see Appleton, 1995, for details). Comparing this with the rise in nominal consumption noted earlier suggests a 6 per cent fall in mean real consumption per capita between the surveys. This is not negligible, but is far from the 34 per cent fall previously discussed.

2.2 Implications for poverty and inequality

What do the adjusted data imply for changes in poverty between the surveys? To show this, we calculate poverty indices for the two surveys using the adjusted consumption data. We set a poverty line of US\$6,000 per person per month in HBS average prices.⁵ The Fisher price index discussed above was used to adjust nominal consumption for regional

differences in food prices and for inflation between and within the periods of the surveys. Table 2 reports the $P\alpha$ class of measures (Foster, Greer and Thorbecke 1984) for the two surveys.⁶ These are defined as:

$$P\alpha \equiv 1/n \sum_{i=1,n} \{\max[PL - C_i, 0]/PL\}^\alpha$$

where PL = the poverty line; n = population size; C_i real consumption per capita of person i and α is a measure of inequality aversion.⁷

The P0 index for the HBS implies that 56 per cent of the population were poor in 1989/90.⁸ The poverty gap (P1) – one estimate of the cost of eliminating poverty – was 22 per cent of the poverty line per head.⁹ The P0 figure for the IHS implies that a

Table 2 Poverty indices for Uganda

2.1 HBS 1989/90					
Location	Population Share	Real PCC	P0	P1	P2
national	100.0	6852	55.6	22.3	11.87
central, rural	28.0	7179	54.3	20.8	10.26
central, urban	6.9	9473	38.8	13.0	6.32
east, rural	21.2	5749	66.0	27.8	15.95
east, urban	1.6	12833	22.1	6.5	2.85
west, rural	30.4	6822	50.8	20.0	10.64
west, urban	1.5	10746	28.2	10.2	4.53
north, rural	9.6	5016	73.4	32.8	18.31
north, urban	0.9	6358	59.0	20.4	8.65
2.2 IHS 1992/93					
Location	Population Share	Real PCC	P0	P1	P2
national	100.0	6086	66.1	28.0	15.25
central, rural	24.6	5656	69.0	28.5	15.23
central, urban	8.6	13403	25.2	7.3	3.10
east, rural	23.2	5019	73.3	32.0	17.69
east, urban	2.3	8610	41.7	13.6	6.26
west, rural	27.6	5102	70.7	30.6	16.85
west, urban	1.6	9893	39.8	12.2	5.21
north, rural	11.6	4966	73.5	33.8	19.00
north, urban	0.5	7149	58.9	23.1	11.46

Notes Real PCC = real per capita consumption (averaged over all individuals). In both cases, the poverty line is 6,000 US\$. per person in HBS prices, adjusted for regional and seasonal variation.

further one in ten Ugandans have fallen into poverty. The rise in the P1 index to 0.28 suggests that the cost of eliminating poverty has increased by over a quarter. The P2 index has also worsened.

This apparent increase in poverty masks substantial spatial variation. Urban areas of Central region appear to have prospered between the surveys: the proportion in poverty has fallen from 39 per cent to 25 per cent while mean living standards have risen by 41 per cent. According to national accounts estimates, non-agricultural sectors grew more than agricultural sectors during the period 1989-1992. This, together with the increase in aid, may have benefited residents in the capital and nearby urban areas most. Western region appears to have fared particularly badly: the proportion in poverty rises from 50 per cent to 69 per cent. This may reflect the short term effects of poor weather: the first harvest of 1992 was officially described as 'very poor' in the West but elsewhere was 'average' (Republic of Uganda 1994b).¹⁰ Of the four regions, the North (more specifically, those parts included in both surveys) appears to have witnessed the smallest deterioration.

The Gini coefficient for (adjusted) real consumption per capita rises between the surveys from 0.38 to 0.4, implying increasing inequality. This is due to increasing inequality within urban areas (the urban Gini coefficient rises from 0.375 to 0.425) and to a widening urban-rural gap. Within rural areas, the Gini coefficient falls from 0.372 to 0.356, implying greater equality. The latter result reflects

the more prosperous rural areas suffering a greater decline in living standards than the poorer North.

3 Appraising the Data

3.1 What we don't know: Evidence from other sources

We now consider how much confidence can be attached to the comparisons of living standards and poverty based on the adjusted data from the household surveys. We begin by assessing other sources of data. One striking feature is how little compelling evidence there is. Between 1989 and 1992, national accounts show the beginnings of Uganda's economic recovery, with a 12.7 per cent increase in real GDP. This translates into a modest 2.7 per cent increase in private consumption per capita (Republic of Uganda 1993, 1994b). However, these estimates involve considerable judgement. Fairly hard information does exist on the industrial sector, where regular surveys of manufacturing establishments record a 31.7 per cent rise in real industrial production between the surveys. This buoyant picture is in contrast to that provided by the surveys (unadjusted). Table 3 draws comparisons from four items of expenditure that are largely domestically consumed and produced. There is disturbingly little correspondence between production estimates and the consumption estimates from the surveys. Moreover, the production estimates rise by far more than the (unadjusted) household expenditure figures (and the CPI). These findings suggest that the surveys understate the growth in consumption. But

⁵ The poverty line is one selected by the World Bank (1993). Adult equivalent scales were not used. Using a much lower poverty line (USh. 3000) gives qualitatively similar results about changes in poverty between the surveys.

⁶ Note that, for comparability, the 1992 figures exclude the seven districts omitted from the 1989 survey.

⁷ If $\alpha=0$ the index gives a head-count: the percentage of people in poverty. If $\alpha=1$ then the index gives the poverty gap: the mean shortfall of consumption below the poverty line as a proportion of the poverty line. With $\alpha=2$, the weight given to shortfalls from the poverty line increases with the shortfall.

⁸ The P0 figure for the country is very similar to that in World Bank (1993, Table 1.2, p 6). There are differences in the estimates by region since World Bank (1993) does not correct for regional price differences in food prices. The price indices used here are for a national food basket with different food prices by region. The spatial pattern of poverty may alter further if regional food baskets were used (see Jamal 1994) or allowance made for differences in non-food prices (Grootaert and Kanbur 1994).

⁹ World Bank (1993) reports a P1 of 0.03 based on the HBS data. This suggests that poverty in Uganda could be eliminated at a cost of only 3 per cent of the poverty line per capita. This probably reflects a computational error.

¹⁰ The second season was described as 'excellent'. The first season in 1989 was described as 'excellent' and the second as 'very good'.

Table 3 A comparison of estimates from industrial production and household expenditure surveys nominal figures (bn. US\$.)

	Production Estimate			Consumption Estimate		
	1989	1992	% Change	HBS	IHS	% Change
Ugandan sugar	9.0	51.9	477	23.7	62.6	164
Washing soap	9.7	31.7	227	16.0	35.0	119
Sodas	9.5	26.1	175	5.9	5.4	-9
Cigarettes	27.0	56.7	110	11.2	12.1	8

Source Statistics Department, Entebbe, personal communication.

it should be noted that the four items discussed are rather special and account for around only 4-5 per cent of total consumption.

For many other, larger, sectors such as agriculture there is no hard evidence such as that provided by the survey of industrial production. The Ministry of Agriculture provides estimates of food crop production but these are not based on reliable surveys. They imply that production has not kept pace with population growth: the national accounts recorded a 3.9 per cent fall in food crop production per capita between the surveys. As previously discussed, poor weather was an additional negative factor in 1992. Indeed, the key role of the weather in developing economies is a further limitation to comparisons of living standards at two points in time. Even if living standards were lower in Uganda in 1992 than 1989 this may simply be a short term effect of the weather, rather than a long term reflection on economic reform. In the absence of reliable agricultural measurements, one cannot be sure how accurately the available statistics reflect this. Coffee procurement figures suggest that some effects may have been dramatic: for some months in the second half of 1992, coffee procurement was half the level reported in 1991. Annual procurement fell by 20 per cent. The national accounts imply that the poor performance in agriculture was offset by strong growth in industry and services. However, measuring output in service sectors is difficult and sometimes not attempted. For example, real health spending since 1985 is assumed to have grown at a constant rate of 2.7 per cent per annum in line with population growth. This is despite such

important events as the onset of the AIDS epidemic and the restoration of security.

In the absence of decisive objective evidence, what do people believe? Most Ugandans questioned by the author believed that poverty did increase between the surveys, or at least that conditions in many rural areas had got worse.¹¹ Besides the weather, they cited instability on the Rwandan border, AIDS and the 'structural adjustment policies' as negative factors. By contrast, expatriates and outsiders working in Uganda tended to endorse the more optimistic picture provided by the national accounts. Among the factors that *a priori* should have raised living standards are the restoration of transport, improved internal security, increased aid inflows and the cumulative effect of a longer period of peace and liberalisation. This lack of consensus and of other evidence increases the potential importance of the data from the two household surveys. However, before the comparison can be relied upon it is necessary to appraise the major adjustments proposed to reconcile the two surveys.

3.2 Paper ghosts? Interpreting household size

The largest adjustment proposed in Section 2 was to make comparisons on a per capita not per household basis. This matters because mean household size is very different in the surveys: around two-thirds of a person less in the IHS than the HBS. Given the close relation between household size and household consumption, discrepancies over the former raise doubt over the latter. Only if the change

¹¹ Those contacted were mainly academics, survey field-workers and NGO workers.

in household size was genuine would comparisons of consumption per capita be clearly appropriate. The 1991 census recorded mean household size at 4.8, corroborating the IHS figure. Although household size is on a downward trend in Uganda, such a large fall between the HBS in 1989/90 and the census in 1991 seems rather implausible.¹² Instead, the HBS figure may be an overestimate.

The HBS may have overestimated household size due to its looser approach to defining household members.¹³ It may have included some who would not be counted as household members under the census or the IHS; for example, household members who live away from home for most of the time. In such cases, the consumption of such 'ghost' members may not be included in the household total. Hence, comparisons of consumption per household are likely to be more valid than those per capita. Some evidence against this interpretation is provided by the demographic composition of the households in the two surveys. It seems more likely that 'ghost' household members are of a particular age and gender - for example, men rather than infants. In fact, the discrepancy in household size between the surveys is proportionately the same in both these age-gender groups. Moreover, the relationship between household size and household consumption is somewhat stronger in the HBS than IHS.¹⁴

An alternative interpretation is that the HBS over-sampled large households. How such a large error could have arisen is not clear from descriptions of the sampling procedure. However, the sampling in the IHS is likely to have been superior. The Statistics Department could draw on the 1991 census,

was better resourced and more experienced. If the problem with the HBS was sampling, then simple per household comparisons are not appropriate. Larger households have lower consumption per capita (in both surveys). Consequently, if the HBS over-sampled large households, it will have underestimated consumption per capita in the population. Following Demery and Grootaert (1993), this can be corrected for by re-weighting the sample according to household size. In particular, we can sum HBS mean consumption per capita for each household size weighted by the proportion of households of the same size observed in the IHS. Using the adjusted figures (and excluding households with over 20 members¹⁵), this procedure gives an estimated mean per capita consumption of 8,247 USH. per month. This represents a 102 per cent increase in nominal consumption per capita but a 11 per cent fall in real terms.

Uganda is not the only case where household size appears to have changed implausibly between surveys. Mean household size in the Côte d'Ivoire Living Standards Study (CILSS) surveys fell even more dramatically from 8.3 in 1985 to 6.3 in 1988. Demery and Grootaert (1993) attributed this to poor sampling procedures in the earlier surveys. As in the Ugandan case, correction for this implies substantially higher consumption per capita in the earlier data. This had important implications for the comparison over time. The unadjusted CILSS data implied a fall in living standards between 1985 and 1986 that disappeared after correcting for the sampling errors. Conversely, after 1986, the rise in poverty was greater than appeared from the unadjusted data. Clearly sampling matters for accurate

¹² Some fall in household size might be expected with the return of security to some areas, but this is likely to be confined to a few districts.

¹³ The two surveys used the same 'cooking pot' definition of a household, with identical wording in the manuals of instructions for field workers. However, the HBS questionnaire required the listing of household members only. The SDA questionnaire also listed other individuals 'associated with the household' to be listed and then subsequently coded them as 'usual' household members or others. The 'others' included 'regular members' who resided elsewhere for more than 6 months and are not included in statistics on household size. Nonetheless, even when this group are included, the mean household size for the IHS is just 4.96.

¹⁴ One might expect a weaker relationship in the HBS if the consumption of 'ghost' members was not included in the household total. This test is not decisive, however, because household spending would increase with the number of 'ghosts' if those non-residents sent remittances.

¹⁵ Households of over 20 members are rare in both surveys and usually unobserved in one or the other.

measurement of living standards and their changes over time. Mean household size – with a census bench-mark – provides one simple gauge for assessing the representativeness and comparability of surveys.

3.3 People forget: The importance of recall periods

The other sizable adjustment in Section 2 is the use of short recall estimates of food purchases. Both common sense and experimental evidence suggest that under-reporting of consumption increases with the recall period. What is surprising is how much difference the choice of recall period appears to make¹⁶. Section 2.1 noted that the seven-day recall data in the IHS gives 25 per cent higher estimates of food, drink and tobacco purchases than the 30-day recall data. If the discrepancy in the IHS is due to recall error, then the shorter recall period will give more accurate mean statistics.¹⁷ However, before this adjustment is accepted a number of difficult questions must be addressed.

First, there is the puzzle of why recall periods appear to matter for estimates of food purchases in the IHS but not the HBS. One explanation is that interviews were conducted differently in the two surveys because of questionnaire design.¹⁸ The consumption section of the HBS questionnaire has 12 pages with rows for different items, headings for different subgroups of items printed on each page and columns for different recall estimates. Given this lay-out, interviewers are likely to have

proceeded systematically down the list of sub-groups of items, asking about consumption during both recall periods. By contrast, the IHS has only a single page to record food consumption and does not have sub-groups headings on different rows.¹⁹ As with the HBS, interviewers were supposed to work systematically down the list of items in their code book. However, with 74 items, this is likely to have been time consuming, cumbersome and disruptive of the flow of the interviews. Given that the columns for the 'between visits' questions precede those for the 30-day recall, the IHS interviews may often have begun by asking respondents to list all the items they had bought since the first visit. Interviewers could then have to selectively prompt for possible omitted purchases.²⁰ Under this procedure, some items purchased in the last 30 days but not between the visits are likely to have omitted.

Without experimental testing of the different questionnaires, this hypothesis cannot be directly tested. Nonetheless, there is some indirect evidence in favour of it. There are fewer cases in the IHS of items being reported as consumed in the last 30 days but not the last seven days. For the 45 food, beverages and tobacco that are directly comparable, only 23 per cent of reports in the IHS were of this kind compared to 27 per cent in the HBS. More generally, the HBS reports more individual items being consumed than does the IHS. Of the 45 comparable items, the number recorded as consumed by respondents in the HBS averages 12.5. For the IHS, the figure is considerably lower at 9.3²¹. With the seven-day recall data, the disparity is far less:

¹⁶ In an experimental study of Ghana, Scott and Amenuvegbe (1990) found that moving from a one day to a one week recall period leads to an under-estimate of expenditure of the order of 20 per cent. No further bias was incurred from moving from one week to two weeks.

¹⁷ For distributional issues, short recall periods have a cost in being more subject to transitory variation unrelated to longer term economic welfare.

¹⁸ Note that the short recall periods for the IHS was 'between visits' whereas in the HBS, there was only one visit and respondents were simply asked to think back seven days. Recalling 'between visits' may be more likely to give actual recall of expenditures, whereas the seven day recall may lead to 'normative' recall, that is to say reporting typical weekly expenditures. There is some evidence that actual recall may produce higher figures than normative recall (Scott and Amenuvegbe, 1990). Moreover, monthly normative recall may not be

independent of weekly normative recall - respondents may often simply estimate the latter as four times the former. This explanation is plausible but the pilot for the HBS survey experimented with both approaches. No significant differences were found, which is why the cheaper seven day technique was used for that survey.

¹⁹ The relatively condensed lay-out of the IHS sections on expenditures was to reduce printing costs. The draft questionnaire proposed to the Statistics Department by the World Bank ran to 72 pages.

²⁰ Some IHS field-workers questioned by the author said they followed this procedure; others said they did as they were supposed to.

²¹ This figure is weighted by the population multipliers in the survey and excludes from the IHS those districts which were not covered by the HBS.

Table 4 Decomposition of difference in purchases between the survey: Directly comparable food, beverage and tobacco items

	30 day estimates (%)	7 day estimates (%)
% change in real purchases per household	-31	-18
Difference explained by:		
1 difference in number of items mentioned	-18	-7
2 difference in spending conditional on item being mentioned	-16	-12
3 residual	3	1

5.2 items mentioned per week in the HBS compared to 4.8 in the IHS. The importance of this can be shown by decomposing the mean change in purchases of comparable food, drink and tobacco items between the surveys. Table 4 shows how much of the total change is due to fewer items being mentioned in the IHS (given the HBS mean purchases conditional on purchase of an item), how much is due to less being spent on items when they are purchased (given the proportion of items purchased in the HBS) and a residual.

With the 30-day recall, over half the fall in mean purchases is due to fewer items being mentioned. With the seven-day recall, this accounts for just over a third.

A second problem concerns non-food expenditure. For most cases, both 30-day and yearly recall estimates were obtained in both surveys. Unlike the case of food purchases, choice of recall period does not appear to matter greatly in the IHS. However, according to the computerized HBS data made available to the author, the estimates of most subgroups of non-food spending are almost twice as high using the 30-day recall figures as they are using annual recall data.²² Moreover, there is an apparent inconsistency in the comparisons between the surveys using both the unadjusted and adjusted figures. The non-food estimates for the HBS are taken from the annual recall data whereas those for the

IHS are taken from the 30-day recall data. This is contrary to the *prima facie* argument for comparing like with like. Furthermore, if shorter recall estimates are thought more accurate for food, the same could also be claimed for non-food. These issues have not been addressed either in the official survey reports or the discussions about suitable adjustments.

One final problem with the adjustment to shorter recall food purchases is what it implies for shares of consumption. The food share is virtually the same in the unadjusted data from the two surveys, as is the share of home produced food. Data on home consumed food were only collected on a 30-day basis. Consequently, switching to a shorter recall for food purchases implies increases in both the food share and the marketization of food between the surveys. Given the poorer weather in the second survey, both implications may be untrue. The national accounts unit at the Statistics Department in Entebbe routinely assumes that a smaller share of food is marketed during poor weather, because households tend to meet their own needs before taking the surplus to market. Indeed, during their most recent revisions, the national accounts unit made the assumption that the 30-day figures for home consumed food were underestimated to the same extent as the 30-day figures for food purchases.²³ If this further adjustment were made in addition to those reported in Section 2, the surveys

²² This is true for the following sub-groups: rent and water, fuel and power, water, transport, health, education, miscellaneous semi-durables and services. Curiously, the discrepancies are much less for clothing and footwear, and for glassware. It is possible that there is an error with the computerized data, although if there is, it is not apparent under scrutiny.

²³ There may have been greater omission of home produced items in the IHS than of purchased ones. Of the 45 comparable food, drink and tobacco items, households reported consuming an average of 4.1 out of home production in the HBS but only 1.8 in the IHS.

Table 5 Shares of total consumption (percentages)

	HBS 1989/90	IHS 1992/93
Food	60.82	61.00
Beverages & tobacco	7.44	3.59
Restaurants	0.79	1.35
Clothing & footwear	5.91	4.72
Other goods	9.86	6.54
Rent, fuel & power	6.00	12.09
Transport & communications	2.81	0.70
Health	1.87	4.35
Education	2.23	5.36
Other services	2.27	0.30

Source: Republic of Uganda (1991, 1994a).

would show a modest rise in consumption per capita. However, this would imply a further increase in the food share. This seems counter-intuitive given the weather and the poorer performance of agriculture between the surveys noted in Section 3.1.

3.4 Hidden inconsistencies: consumption shares

More generally, a comparison of consumption shares (with the unadjusted figures) throws doubt over the comparability of the surveys. Table 5 provides this comparison using the unadjusted figures. Although the food share remains fairly stable, there are rather more dramatic movements in the shares of spending on different types of non-food items than would be expected after a three-year interval. Section 2.1 mentioned the missing code for public transport fares. However, adjusting for this item (1.64 per cent of consumption in the HBS) would do little to create a similar pattern of consumption in the two surveys. While it might be possible to explain the changes as real phenomena, a consideration of survey design suggests more obvious explanations.

One potentially important factor is that the HBS provided a much more exhaustive set of codes for different expenditure items. For example, in the case of clothing, the HBS listed 104 different items. The IHS gave only 15 categories, including 'other

men's clothes', 'other women's clothes' and 'other children's clothes'. Given this difference in detail, it is unremarkable that the HBS records higher expenditure on clothing. However, the magnitude of the difference is surprising: nominal expenditure on clothing and footwear per household rises by only 25 per cent, despite the CPI having more than doubled over the period.

A second difference, already discussed in Section 3.3, is the layout of the questionnaire. The more condensed and open format style of the IHS questionnaire (especially the absence of subgroup headings) may have led to greater omissions of small, idiosyncratic or infrequently purchased items. This could account for why beverage and tobacco have lower shares in the IHS and why production estimates rise so much more than the household consumption figures. Take the example of sodas (fizzy soft drinks including cola). In the HBS, non-alcoholic drinks were among the subgroups printed on the questionnaire and consequently, an interviewer would be almost certainly ask about this item. In the IHS, there is no such prompt and the interviewer has to write down this item in a blank table along with a possible 74 other food, drink or tobacco items. Greater omissions are thus a plausible explanation for why 10 per cent of households in the HBS reported purchasing sodas compared to only 4 per cent in the IHS.²⁴

²⁴ Particularly given that mean purchases of those households for which the item was recorded rose more than the CPI.

These explanations for declining shares for clothing and for beverages and tobacco may also apply to the categories 'other goods' and 'miscellaneous services', both of which also comprise many infrequent or small purchases. As previously discussed, omission of items due to the more open format of the IHS questionnaire may also account for much of the apparent fall in food consumption (at least under the 30-day recall estimates).²⁵

The cases where budget shares rise between the surveys are also revealing. Rent, fuel and power are all items where purchases are likely to be relatively large and regular. Consequently, consumption of these items is less likely to be omitted: indeed, for most households, rent was imputed by the interviewer. The IHS may have picked up more health and education spending by inquiring about them in separate dedicated sections of the questionnaire. These sections asked about the health and education spending on each household member whereas the HBS only requested a total for the household. The former is likely to give a more comprehensive estimate which may explain the substantial rises in health and education budget shares between the surveys.

As previously noted, explaining the discrepancies between the surveys in terms of survey design is speculative. Only experimental testing of different questionnaire designs can decide the issue. Indeed, one of the main implications of the Ugandan experience is the importance of pilot testing alternative questionnaire designs. This is especially where it is intended to compare a survey with an earlier one. Such pilot testing has been done under long standing survey programmes such as the National Sample Survey Organization in India and the SUSENAS in Indonesia. Note that in Uganda, comparability with the HBS appears not to have been an objective in planning the IHS. However, unless the very different composition of household consumption in the two surveys is plausible, it is hard now to place much faith in a comparison of the totals. Otherwise, the

change in mean household consumption will only equal the true figure if the measurement errors of the different components fortuitously cancel.

4 Conclusions: General Lessons

There is currently much interest – and ignorance – about how the poor in Africa have fared during adjustment. In such circumstances, there is a temptation to compare living standards using whatever surveys are available. The Ugandan experience implies that such exercises should be undertaken with care unless the surveys were designed to be comparable. The apparent dramatic fall in living standards suggested by the initial report of the Ugandan IHS is almost certainly spurious. We demonstrated four checks which can be made when making such comparisons. The simplest is to look at mean household size. This can usually be compared to census results but in Uganda (and other cases) surveys can report implausibly large discrepancies. A second, more informationally demanding, control is to compare consumption estimates with available data on production and international trade. For the few commodities where such a comparison could be made in Uganda, there were alarming discrepancies. A third check is to examine whether the recall periods are comparable. The Ugandan results (and experimental data) suggest that recall periods matter more than is commonly realized. Fourthly, the composition of household consumption provides an internal consistency check. Where consumption **shares** change implausibly between surveys – as in Uganda – doubts arise over comparisons of **levels**.

It was argued that differences in survey design were responsible for many of the discrepancies between the Ugandan surveys. Such sensitivity to survey design has implications for analysts: they should be interested in where their numbers came from. It is even more important for those collecting survey data to measure living standards and poverty. Comparability with earlier surveys is important both to

²⁵ The possibility that the open format of the IHS questionnaire led to greater omission of consumption items was implicitly recognised by the Statistics Department even before the inconsistencies with the HBS became apparent. In particular, the 1993 Monitoring Survey implemented to follow-up the IHS explicitly listed a large number of both food and non-food items in its questionnaire.

address the substantive issue of changes over time but also as a check on data quality. Inevitably, however, new surveys will differ from earlier ones either because of new objectives or proposed improvements in design. Consequently, pilot testing should include new and old questionnaires to see if the changes make a significant difference and, if so, to provide correction factors to ensure comparability. One lesson of the Ugandan experience is that donors sponsoring new surveys should be much more sensitive to potential comparability problems with previous surveys. A fairly small investment in proper pilot tests could have a large payoff.

More generally, the Ugandan case suggests that quantitative data from household surveys are more relative than might sometimes be thought. Estimates can vary markedly between surveys at fairly close points in time and appear to depend strongly on their design. This is perhaps not surprising: questionnaires in household surveys do not take objective measures in the manner of natural sciences. They simply code what people say to questions asked. What is surprising is just how sensitive consumption estimates appear to be to questionnaire design and the nature of the interview. Being made up of many irregular and often small purchases, consumption data are particularly prone to errors

of omission - either by the respondent, interviewer or survey designer. Such errors raise further doubts over measures of absolute poverty, quite aside from the difficulty of specifying the poverty line. Similarly, it may be asking too much of household surveys to measure small changes in living standards - for example, the 3 per cent suggested by the Ugandan national accounts.

Household surveys should still provide useful information about relative poverty and distributional issues. While the absolute consumption levels of households may vary markedly with survey design, their place in the distribution at any one time may be more robust. For example, there is evidence that: 'the ranking of households [by] monthly per capita expenditure [differed little as between] a detailed enquiry [and] a one-shot question' (Bardhan 1989: 208).

Indeed, the relative pattern of changes implied by the Ugandan surveys - Central urban areas faring best and the Western region faring worst - is plausible. However, more research is required on this and doubtless the quality of such relative inferences would also improve with greater attention to the accuracy and corroboration of survey data.

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