

## Measuring Poverty in Ireland: A Reply

T. CALLAN, D.F. HANNAN,  
B. NOLAN and B.J. WHELAN

*The Economic and Social Research Institute, Dublin*

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Dr Barrett's comments on our recent report on *Poverty and the Social Welfare System in Ireland* (1988) provide a welcome opportunity to deal with some of the points which have been raised in reaction to that report. While some important issues are involved, his comments do not show a clear understanding of them and are for the most part misplaced.

Before turning to the substantive issues, it is worth emphasising that much of the discussion about our report has *not* been based on what it actually says. Unfortunately Barrett, while bemoaning selective reading of our results, himself adds to the confusion in his first paragraph talking about "particular emphasis . . . on the poverty of 33.5 per cent of the population", with a page reference to the publication. He fails to distinguish here between our report which contains no such conclusion, and the commentary by the Combat Poverty Agency which accompanied it. Ironically he later accuses us of failing to clarify precisely this distinction.

We will attempt to clarify the issues raised in Barrett's comments under five headings.

### 1 *Methodology of Poverty Measurement*

In a country such as Ireland, being able to "keep body and soul together" – avoid starvation, have a roof over one's head – is not sufficient to avoid being in poverty. It is widely accepted that poverty has to be seen in the context of the actual society in question, and the standard of living generally considered adequate there. What is considered adequate will thus change over time and differ across countries – we do not apply the standards of the 1880s

to the 1980s, or of Switzerland to Greece. Even what many currently conceive as “absolute” needs, when closely examined, turn out to reflect prevailing standards – or perhaps those of 10-20 years ago – rather than invariant requirements for subsistence.

A person may be considered to be in poverty when, due to lack of resources, he or she is unable to participate with dignity in the life of the community. How then are we to measure poverty? The notion itself is imprecise, and no unique satisfactory measurement approach has been developed, as we reviewed in Chapter 3 of our Report (Callan *et al.*, 1988). One useful benchmark is provided by looking at the numbers falling below relative income thresholds such as half average income. These have been widely used in, for example, making comparisons across countries (see, for example, O’Higgins (1988), Ringen (1988), Buhman *et al.* (1988)). They are not to be confused, as Barrett does, with measures of inequality; this is illustrated by the fact that it is possible to have *no one* below half average income and yet have a great deal of inequality. In measuring inequality, the frame of reference is in some sense a perfectly equal distribution: in measuring poverty, our reference point is a society where everyone has access to “ordinary” or “normal” living standards. To say, as Barrett does, that this is to measure inequality rather than poverty is simply incorrect, and is based on a failure to recognise the implications of the relative nature of the phenomenon.

While crude, such relative income thresholds can produce strong results. In making comparisons across countries, for example, Ireland has about twice as high a percentage of the population below half average income as Britain,<sup>1</sup> which in turn has a much higher percentage than Sweden (or Switzerland, to use Barrett’s example). There can be little doubt that this reflects real differences in the extent of poverty, and few would quarrel with this poverty ranking of these countries. Similarly in making comparisons over time, the conclusion which we emphasised for Ireland in the 1980-87 period, quoted by Barrett but then ignored by him, is that the percentage of persons falling below relative thresholds rose irrespective of whether 40 per cent, 50 per cent or 60 per cent of mean equivalent income was used. (We elaborate on this finding in a paper in this issue of the *Review*.)

The selection of a particular relative threshold is arbitrary, which is why we used this range and did not concentrate on a particular one in presenting our results. This is the *strength* of the method: it allows the sensitivity of the results to the precise cutoff to be assessed, and those which hold across all three then have much greater force. This approach was *not* “imposed by the

1. The distinction between relative income poverty and income inequality is illustrated by the fact that overall income inequality measures do not show anything like this dramatic difference between Ireland and the UK.

EC", as Barrett repeatedly asserts. Rather, it was adopted precisely because it produces results which do not depend on the poverty line chosen, and gives a much more complete picture than simply focusing on the number under a particular threshold.

## 2 *Equivalence Scales*

Barrett suggests that our finding that "Households with children were found to have a relatively high risk of poverty in 1987 with a substantial increase in the risk since 1980" was due to the equivalence scale used, and that this scale is more generous to children than for example that used in Murphy (1984) and in a NESC study (Rottman and Reidy, 1989). He is referring here to a scale which, taking the household head as 1, allows 0.7 for the "needs" of each extra adult and 0.5 for each child. He seems unaware of the fact that our report contains a detailed analysis of the effects of using different scales. We also employed scales allowing 0.6 for each extra adult/0.4 for each child, and 0.66 for each extra adult/0.33 for each child. The former allows about the same, and the latter considerably less, for children than the scale used by Murphy and Rottman/Reidy. The conclusion quoted holds across this entire range of scales, as was clearly stated in the report.

## 3 *Income Concept and the Role of Non-cash Benefits*

The analysis in our report is based on disposable income — income from work and property plus cash transfers less income tax and social security contributions. Barrett argues that a more appropriate basis is "final" income — where the value of non-cash benefits such as free or subsidised health care or education is added to, and indirect tax paid subtracted from, disposable income. He cites the studies by the CSO (1980, 1983) and Rottman and Reidy (1988) of the redistributive effects of taxes and transfers as supporting this view.

We agree that in assessing the inadequacy of particular cash income levels, the free or subsidised services made available must be taken into account. The disposable income thresholds presented in our report — such as the 50 per cent threshold of about £40 per week for a single adult — are to be assessed against the background that education and health care are for the most part provided free to those at these income levels. Likewise, they are to be seen in the context of the actual prices that people face, which reflect *inter alia* indirect taxes. It does not follow that simply including the costs of such services as a benefit to recipients, and subtracting estimated indirect taxes paid, gives a better measure of command over resources and is more appropriate for comparing standards of living and measuring poverty. A number of issues are relevant here. First, in-kind transfers obviously do not represent command over resources in the same way as cash income: simple utility theory shows that

any in-kind transfer will be valued less by a consumer than the corresponding cash amount, because there is no choice about its allocation. This is apart altogether from the fact that the benefits are attributed on the basis of the cost of provision – so an increase in teachers' pay shows up as a benefit to those receiving free education.

Second, a major part of the in-kind benefits from health care is tied to inherently undesirable contingencies. Compare two persons with the same final income. For the first, much of that income comes from the attributed benefits of a long hospital stay. The second has had no such stay, and all of the income is cash. In Barrett's terms, they enjoy equal standards of living, but this contradicts common sense.

With respect to the other major type of non-cash transfer, education, most of the benefit received by low-income groups relates to compulsory education. If this is to be included as a benefit for households with children, then the equivalence scales must be adjusted to incorporate the legal requirement to educate children up to 15 years of age. The most appropriate way to do this is to add the cost of educating a school-age child to the family's needs. But this simply cancels out the addition of the attributed benefit, in the same way as excluding education from both sides of the equation (needs and non-cash income), which is what we have done.

Non-cash benefits from housing, and the imputed income from owner-occupation, are the main other items of non-cash income. We have re-analysed our sample on the basis of disposable income net of housing costs to see the impact this would have on the percentage of households below relative thresholds. Table 1 shows that income net of housing costs does not in fact give a very different picture.

Table 1: *Proportion of Households Below Relative Poverty Lines Using Alternative Income Concepts, 1987*

<i>Equivalence Scale:</i>	<i>1, 0.66, 0.33</i>		<i>1, 0.7, 0.5</i>	
	<i>Disposable Income</i>	<i>Disposable Income Net of Housing Costs</i>	<i>Disposable Income</i>	<i>Disposable Income Net of Housing Costs</i>
40 per cent line	7.5	7.5	10.0	9.4
50 per cent line	17.4	16.6	18.9	18.7
60 per cent line	29.5	28.6	29.0	27.7

The treatment of indirect taxation implied by Barrett's suggested concentration on final income is also open to question. Whereas disposable income can be interpreted in the light of actual price levels, final income must be evaluated at a set of indirect-tax-free prices. This is a rather unsatisfactory counterfactual for comparing living standards, and would have to take into account the different consumption patterns of those at different income levels.

If non-cash benefits provided by the state are to be included, then those available from other sources are also relevant. Benefits associated with employment such as company cars, subsidised loans, share options, etc., are concentrated towards the top of the income distribution, and influence relative living standards.

Even if all these complexities were ignored and final income treated as if it were the same as cash income, Barrett's use of the final income data in Rottman and Reidy (1988) is misleading. First, he uses average household income figures which do not take into account differences in household size and composition – i.e., they are not equivalent incomes.<sup>2</sup> Second, he treats the bottom decile ranked by disposable income as if it were the same as the bottom decile ranked by final income, ignoring re-ranking.<sup>3</sup> To carry out the comparison which he is trying to make, a full analysis of final equivalent incomes is necessary – which we are pursuing on the basis of the 1980 HBS data tapes.

Finally, Barrett's discussion of the studies by the CSO and Rottman/Reidy fails to see the fundamental difference between the objectives of these studies and our own. They are focused on measuring the redistributive impact of state taxes and transfers of various kinds, on tracing the flows involved. Thus there is no conflict between the conclusion he quotes from Rottman and Reidy that the *redistributive impact* of state intervention increased between 1973-80 and (tentatively) 1980-87, and our results on the increase in numbers falling below relative thresholds. The key is what is happening to market incomes, as Rottman and Reidy make clear; given the sharply increasing inequality in these incomes, a greater redistributive impact is quite consistent with an increase in poverty.

2. While the data cited by Barrett from Rottman and Reidy (1988, p. 205) are based on households ranked by equivalent income decile, the income figures themselves are *not* equivalent income. It is therefore not valid to merely calculate a poverty threshold as 50 per cent of the overall average, and compare the average income of the decile with this threshold.

3. Thus, when he concludes that the bottom decile has 50.9 per cent of average final income this in fact refers to the bottom decile ranked by disposable income. It may be the case for example that most of these remain in the bottom 10 per cent by final income, but that the non-cash benefits are concentrated on those who are re-ranked into higher deciles.

The role of non-cash benefits is an area of great importance, and our survey was designed with precisely this in mind, gathering a great deal of information about utilisation of the various services. In-depth analysis of this information, together with the results of the CSO's redistribution exercise using the results of the 1987 Household Budget Survey, will provide a much clearer picture of the impact of those services. Teasing out the implications for the analysis of poverty and anti-poverty policy will have to take into account the complexities we have described. Barrett's comments fail to recognise the conceptual complexities and the empirical procedures required.

#### 4 *Reliability of the Data*

Barrett asserts that our survey considerably understates incomes. He uses the ratio of expenditure to income in the Household Budget Survey to suggest that understatement of income in such surveys is concentrated in the lower income groups. This is not a valid inference: there are a number of other explanations for the difference between measured incomes and expenditures. Most fundamentally they are obviously two quite different concepts. Expenditure may be financed from borrowings or running down savings, for example, or non-recurring receipts. Direct investigations of the reliability of similar surveys internationally have found that understatement or underrepresentation tends in fact to be concentrated at the *top* of the income distribution (see for example Atkinson and Micklewright (1983), Adler and Wolfson (1988), Bound and Krueger (1988)). If this is the case, relative poverty measures calculated on reported incomes would tend to underestimate the numbers below relative poverty thresholds. For our own survey, comparisons with independent aggregate data, for example the percentage below the Medical Card means test threshold, suggest that it reflects the national situation well.

Barrett states that low estimated levels of take-up of the means-tested SWA and FIS schemes are evidence of substantial under-reporting. These levels of non-take-up are not out of line with those observed elsewhere, where in-depth studies have revealed the importance of factors such as lack of information, complex application procedures and "stigma" in discouraging those below the relevant income levels from claiming. Also, the report notes that there is *less* incentive to under-report in a survey such as ours than in the actual means test.

He also points to the variation in style of living/deprivation indicators for those at particular income levels. He emphasises the relatively favourable position of "Unemployment Benefit households". The households involved are in fact *headed* by a person in receipt of Benefit. This does not mean that they are solely or even largely dependent on it, since other household members may be at work. Unemployment Benefit is not means-tested, and many of these households are not towards the bottom of the income distribution (even

using equivalent rather than an unadjusted incomes). Also, the style of living of households must be seen in the context of other factors such as their stage in the life-cycle. Overall comparisons at an aggregate level are not particularly revealing, and we intend to devote a good deal of analytical effort to improving our understanding of the factors influencing style of living and the relationship between income and deprivation indicators.

### *5 Policy Relevance of the Report*

In discussing the impact of our report, Barrett states that although it is "scrupulously neutral" between the range of poverty thresholds, "it is a matter of some regret that the ESRI failed to clarify its position when the sponsoring agency chose to highlight the highest number". We utterly reject this charge: the report itself makes clear the position of the authors, and the views of the Agency are quite clearly presented as such. The format of the report is identical to that used by the NESC in presenting consultants' reports together with a separate commentary by the Council (see for example Rottman and Reidy, 1988). As we pointed out at the outset, Barrett himself fails to make this distinction clear for much of his comment, and this is compounded by his referring throughout to "the CPA/ESRI report". No one who has actually read the report could need clarification about our position.

Barrett also states that "the report could have done serious harm to the Irish economy:", but "did not, however, influence the 1989 Budget", since "the welfare increases were selective and small overall". This entirely misses the point that our report provided a rational basis for targeting the increases in welfare payments towards those most in need. A number of the groups we pinpointed were singled out for particular attention in the Budget, notably the long-term unemployed and those in work on low pay. Further analysis of the extent, nature and causes of poverty in Ireland should allow anti-poverty policy to be made considerably more effective.

### *6 Conclusion*

While some important issues are raised, the main thrust of Barrett's comments is quite misplaced. The poverty line methodology he criticises is the one widely employed by economists in all developed countries; his attack on the reliability of the income distribution is based on inappropriate evidence; his assessment of the effects of including non-cash benefits is severely flawed, and he confuses measurement of the overall redistributive impact of state policy and measurement of poverty.

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