



# POVERTY

ASSESSMENT PAPER

*Benefits and Costs of the Poverty Targets  
for the Post-2015 Development Agenda*

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# Benefits and Costs of the Poverty Targets for the Post-2015 Development Agenda

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Post-2015 Consensus

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# HIGHLIGHTS

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Within the area of poverty, the target with the best benefit cost-ratio is:

- *Eliminate extreme poverty* which has a benefit-to-cost ratio of 4-6.

The remaining targets are relatively ineffective, or there is large uncertainty in the benefit-to-cost ratio.

- Cover x% of people who are poor and vulnerable with social protection systems
- Build resilience of the poor and reduce by x% deaths and economic losses related to disasters
- Ensure equality of economic opportunity for all women and men, including secure rights to own land, property and other productive assets and access to financial services for all women and men
- Achieve full and productive employment for all, including women and young people
- Every country will monitor the wellbeing of its citizenry with improved measurements and reporting of life satisfaction

## SUMMARY OF RECOMMENDATIONS

TARGET	Benefit Cost Ratio	Rating
Eliminate extreme poverty	4-6	GOOD
Cover x% of people who are poor and vulnerable with social protection systems	<1	POOR
Build resilience of the poor and reduce by x% deaths and economic losses related to disasters	<1	POOR
Ensure equality of economic opportunity for all women and men, including secure rights to own land, property and other productive assets and access to financial services for all women and men	Varies by sector	UNCERTAIN
Achieve full and productive employment for all, including women and young people	<1	POOR
Every country will monitor the wellbeing of its citizenry with improved measurements and reporting of life satisfaction	<1	POOR

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## INTRODUCTION

How much does poverty cost? It is an easy question to ask but a surprisingly hard one to answer. The modern literature on measuring poverty dates to the late 19<sup>th</sup> Century, in the work of Charles Booth and Seebohm Rowntree, but there has been an explosion of poverty measurement in the last two decades prompted by the growing availability of household survey data. For example, the global poverty counts by Chen and Ravallion (2013) rely on a household survey database of almost 900 surveys from 125 low- and middle-income countries fielded between 1979 and 2011. Yet in this century of measurement, the main output of researchers has been measures of poverty in its own terms but not in broadly economic terms that let tradeoffs be considered, where the costs and benefits of reducing poverty are balanced against each other. Instead, modern studies typically report the incidence, depth, and severity of poverty using unified measures due to Foster, Greer and Thorbecke (1984). These measures tell one the proportion of a population who are below the poverty line, how far below the line they are on average, and the inequality amongst the poor revealed by poverty statistics that put more weight on those furthest below the line. More rarely, poverty measurement is given a time dimension; for a given poverty line and a given distribution of income or consumption below that line, how many years would it take at a constant and uniform growth rate for everyone to escape poverty (Morduch, 1998).

Amongst these typical ways to measure poverty, only the depth of poverty statistic that gives the average proportionate shortfall from the poverty line can be interpreted in monetary terms. If an all-seeing, all-knowing government perfectly targeted transfers to the poor, giving them just enough to close poverty gaps, and if such transfers had no administrative cost, created no disincentive effects, and the information for perfect targeting was freely available, then the sum of all the poverty gaps might be interpreted as a measure of the total cost of poverty. Indeed, Clunies-Ross and Huq (2014) do just this, to calculate a global poverty gap of \$82 billion, for \$1.25 per day poverty (in 2005 PPP terms). An implicit cost-benefit argument is also made by these authors, who argue that social protection systems could be used to eradicate poverty since the required transfers are equivalent to just two-thirds of official development assistance. Yet this way of measuring the cost of poverty is neither conceptually sensible nor practically useful. At the conceptual level it is tautological – the cost of being poor is just that you are poor – when in fact there are likely many more effects of poverty on education, health, life expectancy, and perhaps even macroeconomic performance that are ignored. At the practical level, perfectly targeted transfers amount to marginal tax rates of 100 percent since every dollar of extra income earned by a poor person reduces their transfers received by the same amount, and transfers are administratively costly, and targeting is neither perfect nor costless.

The work of researchers in measuring poverty in its own terms may reflect a normative view that poverty is such a bad thing that it should be eradicated irrespective of costs. Under such a view, calculating what poverty costs is unnecessary since the arguments for eliminating poverty go beyond economic calculation. If so, such a judgement is a relatively recent way of thinking. Ravallion (2011) uses textual analysis to examine changing awareness of poverty over three centuries; at the beginning of this period (and in earlier

times) there was an acceptance of the inevitability, and perhaps even the necessity, of poverty. But beginning in what Ravallion has termed the ‘first Poverty Enlightenment’ of the late 18<sup>th</sup> Century there is growing discussion of the ideas that poverty is not the result of some natural ordering, and that a minimum acceptable standard of living should be available for all members of society. A similar timing for this shift in thought is given in Alfred Marshall’s *Principles of Economics* in an Appendix on the growth of economic science. According to Marshall (1920, p. 626), while the mid-18<sup>th</sup> Century French Physiocrats under Quesnay’s intellectual leadership did not create a school of thought that produced much of direct value, they had a great indirect influence on neoclassical economics (that dates to Marshall himself) because of their motivations:

“...[t]he chief motive of their study was not, as it had been with most of their predecessors, to increase the riches of merchants and fill the exchequers of kings; it was to diminish the suffering and degradation that was caused by extreme poverty.”

Perhaps if Mercantilism had stayed the dominant economic school of thought, a literature on ‘optimal poverty’ may have developed with empirical tools that let a researcher calculate the rate of poverty that would most ‘increase the riches of merchants’. But instead, the neoclassical school dominates, and even though a weighing of costs and benefits at the margin is a hallmark of the neoclassical approach, such cost-benefit calculus evidently is not extended to poverty. Instead, with many poverty researchers, and other well-meaning individuals and groups, motivated to reduce ‘degradation caused by extreme poverty’ it seems almost uncouth to give an instrumental value to reducing poverty. The lack of such valuation poses a severe challenge to an initiative like the Copenhagen Consensus that attempts to “give priority to targets that yield the largest return for human development.” Many people would consider that poverty reduction is human development, and as such it is not something to be traded off against progress on other targets. In other words, poverty may be incommensurable with monetary measures of costs and benefits in ways that many other desirable development outcomes are not. For example, a laudable but less prominent development goal like improved sanitation does not lack for estimates of economic payoff; even though claims that ‘cleanliness is next to Godliness’ may have convinced children to wash, hard-headed policy makers want to see dollar values from investments they make in this area and even sponsor randomized control trials (e.g., Cameron, Shah and Olivia, 2013). But the situation with poverty is different – reducing poverty is typically seen as an end in itself, so estimates of the economic payoffs from such a reduction are almost entirely absent in the literature.

Of course effects of inequality on economic growth are widely studied and one could use such studies to adopt an instrumentalist view to choosing the optimal level of inequality that yields the greatest economic payoff. The literature in this area suggests that it is important to disentangle the responses to inequality (such as the urge to redistribute) from effects, and after so doing it appears that lower inequality is correlated with faster and more durable growth (Ostry, Berg, and Tsangarides, 2014). There is no parallel set of studies where the effects of poverty on growth are examined, and instead it is typically the reverse question that is examined, of why some growth processes are more pro-poor in certain settings than others, with evidence taken at both the cross-country level (e.g. Ravallion, 2011a) and the sub-national level (e.g. Ravallion and Datt, 2002). Even if the

reverse regression, of growth on poverty, was common, it would be hard to make causal claims since the identification of causal effects would require some factor that affected poverty but not economic growth and it is difficult to think of any such factor.

Consequently, there is very little in the large literature on poverty that can directly inform an initiative like the Copenhagen Consensus. Perhaps some of the targets proposed as part of the post-2015 development agenda that relate to poverty have high benefits relative to costs, but there is little credible evidence currently available to support such claims. Since the evidence does not inform, posterior beliefs should be similar to prior beliefs. The prior revealed by the formation of the Millennium Development Goals (MDGs) was that the most important target related to poverty was to reduce extreme poverty, defined as \$1 a day in terms of the PPP dollars of the time. Most of the other proposals being made as part of the post-2015 agenda did not make the cut when the MDGs were being formed and in the absence of compelling evidence it is unclear why they should make the cut now. Conversely, absent rigorous evidence that causes beliefs to be updated, a conservative principle is to continue doing what one already is doing. In this context, that principle suggests that the optimal strategy for the post-2015 period is to continue to monitor extreme poverty, defined at the level of \$1 a day in the PPP terms that were current when the MDGs were negotiated. As discussed below, the progress in reducing extreme poverty from 1990-2010 is unlikely to be repeated in future, so a zero target of eliminating this \$1 a day poverty by 2030 probably will not be met and likely distracts from more urgent tasks of doing a better job of monitoring what poverty reduction actually does occur.

Notwithstanding these conclusions, based on logical reasoning rather than a full review of the literature, the remainder of this document loosely follows the process that the Copenhagen Consensus requires. The targets to be considered are described in the next section. Following that, there is a note on the perspective taken in this review, which is from the point of view of someone involved in designing, implementing and analyzing surveys for measuring poverty at country level. Next, the background and context section provides some simple intuition for why the nature of poverty changes, as a country escapes mass poverty, and what that might mean for the choice of future poverty targets and the likelihood of achieving those targets. This section provides a defense of the indicator used for the MDG poverty target, which was extreme poverty at the \$1 a day level (in terms of PPP values when the MDGs were set), and also suggests why the rate of poverty reduction from 1990 to 2010 is unlikely to be matched in the 20 years to 2030. Consequently, a zero target is unlikely to be met. A theme of this background section is that we know much less about poverty than we think we do. Therefore the section ends by discussing the survey information gap in Africa, and the broader problem of relying on surveys that are poorly designed to measure living standards in an era of rising affluence and urbanization. The final two areas of ignorance discussed are that seemingly promising alternatives from non-monetary approaches to measuring life satisfaction and happiness are likely to be a mirage, and that we have a poor understanding of the complex responses of households when presented with new sources of cash, such as from social protection transfers whose expansion is one of the proposed targets that will be evaluated.

In Section V the methods of valuing the benefits of reduced poverty are discussed, with particular attention given to a method for valuing human capital that may be appropriate

for considering some of the broader effects of poverty reduction. An advantage of this method is that it is already being used by statistical agencies in some (rich) countries to value human capital as part of an extended set of national accounts, and so it is a method that is potentially feasible to extend to poor countries as one way to begin valuing poverty reduction. The evaluation of the various groups of targets is covered in Section VI, and some broadly indicative cost-benefit comparisons are made. These could generously be described as back of envelope calculations, which have been made specifically for this project since the literature does not contain such comparisons. The conclusions of the review are given in Section VII.

## **THE TARGETS CONSIDERED**

A dizzying array of goals and targets are proposed by the various groups debating the post-2015 development agenda. The attention here is restricted to those related to poverty. The specific proposals that are evaluated are of three overlapping types: six targets from the May Session of the Open Working Group; four targets from the High Level Panel (UNHLP, 2013); and, one target and one indicator that caught the interest of the author from amongst the plethora of suggestions made by other participants in the post-2015 consultation process.

### ***Open Working Group Targets***

These targets are from Focus Area 1 “Poverty Eradication, Building Shared Prosperity and Promoting Equality” and the wording is based on a working document for the 5-9 May, 2014 session of the Open Working Group.

- i. eradicate extreme poverty by 2030 by bringing the number of people living on less than \$1.25 a day (in 2005 PPP terms) to zero
- ii. reduce the proportion of people living below national poverty lines by 2030
- iii. by 2030 implement nationally appropriate social protection measures, including floors, with a focus on coverage of the most marginalized
- iv. build resilience of the poor and reduce by x% deaths and economic losses related to disasters
- v. achieve full and productive employment for all, including women and young people
- vi. ensure equality of economic opportunity for all women and men, including secure rights to own land, property and other productive assets and access to financial services for all women and men

### ***High Level Panel Targets***

The High Level Panel of eminent persons on the post-2015 development agenda suggested four targets under the “Ending Poverty” goal:

- a) Bring the number of people living on less than \$1.25 a day to zero and reduce by x% the share of people living below their country’s 2015 national poverty line
- b) Increase by x% the share of women and men, communities and businesses with secure rights to land, property, and other assets
- c) Cover x% of people who are poor and vulnerable with social protection systems
- d) Build resilience and reduce deaths from natural disasters by x%

Clearly (a) from the High Level Panel corresponds to (i) and (ii) from the Open Working Group, while (c) corresponds to (iii), and (d) to (iv). These three will be covered under the headings of “poverty”, “social protection”, and “disaster resilience” in what follows. The correspondence of (b) and (vi) is not as strict, while target (v) regarding full employment does not feature in the proposals from the High Level Panel. These targets that are less agreed upon by the two sources will be denoted “economic rights”, with special attention to the gendered aspects of those rights, and “full employment” in what follows.

### ***Proposals of Idiosyncratic Interest***

A target proposed by Jeffrey Sachs is that:<sup>1</sup>

- “Every country will monitor the wellbeing of its citizenry with improved measurements and reporting of life satisfaction.”

Although just the proposal of an individual, this target captures the gist of many others that aim to go beyond standard indicators like GDP because of the discontent over current approaches to measuring economic performance and social progress. Independent of the post-2015 agenda, the demand for this type of life satisfaction measurement as an alternative to conventional economic statistics gained prominent support from the ‘Stiglitz Commission’ set up by French President Sarkozy in 2008 due to his dissatisfaction with the present state of statistical information about the economy and society. In the report by this commission, Stiglitz, Sen and Fitoussi (2010, p.12) note that “the time is ripe for our measurement system to shift emphasis from measuring economic production to measuring people’s well-being” and this fits well with the proposal made by Jeffrey Sachs. The evaluation given below suggests that such a recommendation is premature because there can be no meaningful cross-country (or cross-group or over time) comparison based on typically used subjective wellbeing indicators, and worse, these indicators could even be used to support policies that perversely reduce welfare, such as restrictions on human mobility.

An indicator (without a target) suggested by the Centre for International Governance Innovation is the Multidimensional Poverty Index (MPI). The MPI is one of six indicators for inclusive growth, income poverty and inequality under the broad goal of “Inclusive Growth for Dignified Livelihoods and Adequate Standards of Living” (Carin and Bates-Eamer, 2013). There has been considerable enthusiasm for using the MPI as either a supplement or a replacement for conventional poverty analysis in academic circles (see, e.g., Alkire and Santos, 2014) and also in some international organizations other than the World Bank. This author does not share that enthusiasm, for two reasons. First, in the same way that the MDGs are multi-dimensional, so too will be the post-2015 development goals, so having a multi-dimensional index as one part of an overall multi-dimensional monitoring process seems unnecessary. Second, the MPI involves tradeoffs made by researchers rather than by poor people, in the way that different components of an index add together. In contrast, the cost-of-basic-needs (CBN) poverty line, which is the preferred method that many countries

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<sup>1</sup> As noted in the North-South Institute post-2015 tracking tool database, with a source given as: <http://jeffsachs.org/wp-content/uploads/2012/06/From-MDGs-to-SDGs-Lancet-June-2012.pdf>

use, is derived almost entirely from choices made by poor people rather than by affluent researchers. With the exception of the normative calorie target used to anchor the food poverty line, everything else in the CBN poverty line depends on how poor people allocate their household consumption budget.<sup>2</sup>

## **A NOTE ON PERSPECTIVE**

The discussion of post-2015 development goals and targets has attracted much attention from the research community. In particular, there is already a large literature on poverty forecasts, with a particular focus on the likelihood that the world as a whole, and various regions, will reach certain poverty targets by 2030. Amongst the studies already completed are some by the Brookings Institute, the Center for Global Development and the World Bank (papers by Chandy, Edward, Olinto, Ravallion, the World Bank, and Yoshida in the references are examples). Some of these studies will be referred to at various points, but no attempt is made here to replicate or extend them. Since there is open access to the poverty data used in these studies (mostly they use the World Bank's Povcal Net database), and also since these other studies are the work of larger research teams, it is fair to assume that whatever nuggets of truth are to be found in those data will already be known and nothing more could be contributed by the current study.

Instead, the perspective taken here is that of someone who is involved in designing, implementing and analyzing surveys for measuring poverty at country level, in countries that vary widely in the success they have had to date in reducing poverty and in their capacity to undertake basic socio-economic measurement and analysis. From this point of view, many of the proposals made under the guise of the post-2015 agenda appear to be unhinged from the reality of statistical measurement in poor countries. Too many proposals show unwarranted confidence in the existing data and inadequate thought about constraints to improving the data infrastructure. In this regard, the one existing study that pays attention to these data issues, and in part goes beyond what is covered here, is the book by the World Bank (2014) *A Measured Approach to Ending Poverty and Boosting Shared Prosperity*. That book is highly recommended.

It is also the case that too many of the proposals are insufficiently critical of the ability of governments, and especially the United Nations system, to credibly deliver even basic statistical measurements and reporting, let alone having a higher level ability to implement policies designed to achieve the lofty development goals that are set. One example, from the High Level Panel, will suffice to make this point. The panel recommends that:

“Starting in 2015, the UN could produce a single Global Sustainable Development Outlook, jointly written every one or two years by a consortium of UN agencies and other international organizations.” (UNHLP, 2013, p.22)

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<sup>2</sup> The calorie target in CBN poverty lines used by statistics offices surveyed by the UN *Handbook* project described below ranged from a low of 2000 calories per person per day to a high of 3000. The variation did not appear to be related to climate, latitude or any other easily observable feature of countries so likely reflects either historic accident or idiosyncratic variation introduced by the methodological preferences of the first researcher or consultant to establish CBN poverty lines in each country.

Leaving aside the issue that many international organizations like the IMF and the World Bank already produce similar outlooks that could easily be expanded to cover the post-2015 goals, this recommendation reflects a naïve over-confidence in the capabilities of the United Nations agencies.

Evidence for this claim comes from the failure of the United Nations Statistics Division to complete a relatively simple task, which was to publish a *Handbook on Poverty Statistics*. A series of six meetings by a UN-sponsored expert group on poverty statistics, held between 1997 and 2003, preceded a series of regional workshops in 2004 with country-level statistical officials. This activity culminated in a set of authors (including myself) being commissioned to write chapters for the *Handbook on Poverty Statistics: Concepts, Methods and Policy Use*. At the same time, the United Nations Statistics Division launched a “Global Survey on Poverty Measurement” that collected metadata on aspects of household surveys and poverty measurements from over 100 national statistical agencies. The overall aim of this project was to produce a *Handbook* that could assist countries to design and implement a system of poverty measurement that would satisfy their national policy needs and also help with the international poverty comparisons that were increasingly demanded. In particular, by describing best practice in poverty measurement and analysis in the commissioned chapters, and by using the metadata survey to document the diversity of country-level practice, it was anticipated that the *Handbook* may help nudge statistical agencies into adopting better, and more harmonized, practices of poverty measurement and analysis.

The need for such a nudge is because, in contrast to most economic statistics where there are detailed procedures to ensure conformity, and these are supported by publications such as the *Handbooks of National Accounting* prepared by the United Nations and the *Handbooks of Balance of Payments Statistics* prepared by the IMF, there is no such conformity in methods for the surveys of household incomes and consumption expenditures that provide the source data for poverty measurement. Much of the diversity in methods is of idiosyncratic origin rather than the outcome of systematic trial and error, and contributes to great uncertainty (of a non-sampling type) in poverty comparisons across countries and over time. The idiosyncratic variation in household survey methods also contrasts with the collection of data on fertility and maternal and child health; cross-country and temporal comparisons made with such data rest on far firmer foundations that do poverty comparisons, due to the standardized approach to measurement by the Demographic and Health Surveys. There is no similar standardization of surveys used to derive poverty measurements, with particular approaches in particular countries at particular times reflecting accidents of historical practice and the influence of particular donor country agencies and even of individual consultants. Moreover, it is the poorer, smaller, and less significant countries that are more beholden to donors to fund surveys (introducing potential non-comparability as they switch from one donor’s survey method to another). Such countries also lack domestic capacity and are unlikely to attract interest of foreign researchers to either advocate for use of consistent methods over time or to help form *ex post* adjustments to try to restore comparability to non-comparable poverty statistics (by way of contrast, consider the local capacity and foreign interest revealed in the contributions to Deaton and Kozel (2005) that deals with debates about Indian poverty that were caused by changing survey methods).

The limited evidence from experiments is that poverty estimates are very fragile due to these differences in survey methods. The firmest evidence is from Beegle et al (2012) who compare eight methods of surveying household consumption; each method is commonly used in poor countries. These eight methods were randomly assigned to different households in the same towns and villages in Tanzania so that any difference in the resulting poverty estimates is due just to the methods (the random assignment ensured that characteristics relevant to poverty status balance between all of the samples). The results show huge effects; the poverty headcount rate varies from 48 percent to 67 percent depending on which survey design is used. A follow-up study by De Weerd et al (2014) uses data from the same survey experiment to highlight the fragility of hunger estimates (which use estimates of food quantity and the variance of calorie availability that can come from these same types of household consumption surveys). Depending on the survey method used, anywhere from 19-68 percent of the sampled population would be classified as being hungry. In the context of Tanzania, variation due to survey method causes a difference in hunger counts of more than 23 million people and across all of Sub-Saharan Africa (where hunger is highest according to FAO monitoring for the MDGs), these differences amount to hundreds of millions of people potentially misclassified as either hungry or not hungry.

While this experiment informs us about cross-sectional fragility, temporal comparisons are also vulnerable to changing survey methods. For all countries except those with very fast and sustained rates of poverty reduction (e.g. Vietnam), the extent of the ‘noise’ in poverty statistics due to these survey design issues and due to the failure to maintain comparable methods over time and space is likely to be similar to the magnitude of the ‘signal’ in the data from the trend in true but unknown poverty rates. For example, when India replaced the previously used 30-day recall period for measuring household expenditures with a 7-day recall for food and a one year recall for infrequent purchases the changes seemed to raise reported consumption by about 17 percent. Since there were so many Indians close to the poverty line, this 17 percent increase was enough to reduce the measured headcount poverty rate by a half, removing almost 200 million people from poverty. As Deaton (2002, p.215) drily noted about the change in survey methods: “this must be the most successful poverty-reduction program in the world!”

But in spite of the threats to monitoring efforts like the MDGs that are posed by these findings, and the benefit that could come from countries adopting better and more harmonized approaches, the United Nations never managed to publish the *Handbook on Poverty Statistics*. Instead, this book has sat in draft form on the UN website for the last nine years. While the complete manuscript of the *Handbook* is on the website, it is designated as “currently being edited and not to be quoted” and so it is effectively out of reach for the country statistics offices that it was designed to help. Similarly, the detailed metadata survey that was carefully completed by statistics officials around the world lies unused and is now probably unusable because it is out of date.<sup>3</sup> This experience raises the question of how one can expect useful statistical monitoring from an international agency that has

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<sup>3</sup> In fact, a new metadata survey that covered many of the same questions about survey design but focused especially on food consumption modules had to be fielded in 2013 by a World Bank-led consortium (Dupriez, Smith, and Troubat, 2014).

revealed itself to be so incompetent, and which apparently prefers to deal in platitudes about poverty but when given the opportunity to make a small but useful improvement in our ability to consistently measure poverty it fails. The proponents of a greatly expanded role for the United Nations in monitoring an expanded set of post-2015 goals and targets should reflect on this example when suggesting greatly expanded monitoring and reporting tasks.

## **BACKGROUND AND CONTEXT**

### ***Why reducing extreme poverty remains a hard target to achieve***

The first, and most prominent, Millennium Development Goal is to eradicate extreme poverty and hunger. The first target under this goal is to halve, between 1990 and 2015, the proportion of people whose income is less than \$1 a day. The MDG goals and targets were not fully specified in the Millennium Declaration of the United Nations and instead concordance with an earlier set of OECD International Development Goals (IDGs) was finalized in 2001 (Hulme, 2009). The IDGs had provided a basis for the World Bank's 1997 *Global Monitoring Report* and so tracking of the first IDG, that "the proportion of people living in extreme poverty in developing countries should be reduced by at least one-half by 2015" was done using a World Bank approach to measuring global poverty that the MDGs then inherited.

This World Bank approach built on Ravallion, Datt and van de Walle (1991), who used local poverty lines for a group of poor countries that averaged US\$23 and US\$31 per person per month in 1985 PPP prices. Notably the catchy '\$1 a day' term was not used in this initial study, despite the similar monetary values.<sup>4</sup> Poverty rates were calculated using grouped distributional data from consumption surveys where available, and otherwise income surveys, to fit a parametric Lorenz curve that was then combined with the poverty lines and the means of the living standards distributions. The same approach, on a larger scale, is used now to form the World Bank's Povcal Net database. The MDG poverty target reflected diverse influences and so there is some incoherence; the target is in terms of income but progress is measured mostly with consumption data. Economists make the target appear more coherent by describing it as cutting in half the proportion of people living below \$1 a day (Besley and Burgess, 2003), which incorrectly implies a consumption target. Similarly, \$1 a day is no longer the benchmark; it was first updated to \$1.08 in 1993 PPP prices, then reset (by re-calculating from the national poverty lines of 15 poor countries) to \$1.25 a day in 2005 PPP prices, and will be likely reset again once the 2011 PPP results from the International Comparison Program are agreed upon.<sup>5</sup>

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<sup>4</sup> Similarly, there is no mention of \$1 a day in the 1990 *World Development Report (WDR)* which focussed on poverty, despite subsequent media comment linking the debut of this definition of poverty to the *WDR*. See, for example, "On the Poverty Line" *The Economist*, May 22, 2008.

<sup>5</sup> The use of a small reference group of countries whose national poverty lines are averaged to form the global line is criticized by Deaton (2010) because it violates monotonicity. A poor country with a low poverty line, such as India, may sufficiently reduce poverty that it graduates out of the group of 15 and its replacement causes the global poverty line to rise. In describing the revisions to global poverty counts from resetting the global line in 2005 PPP prices, Deaton (2010, p.16) wryly notes that "India and the world have become poorer because India has become richer!" The revisions to the PPP values from the 2011 International Comparison Program (ICP) also suggest that the entire time series of poverty estimates based on the 2005 ICP may have been too high (Deaton and Aten, 2014) although there is a lot of current debate on this issue.

The point of this brief history is that the MDG poverty target was not formed *de novo* and instead reflected an existing set of contradictory influences. For example, compared to the gradualism of the IDGs, United Nations agencies and summits in the pre-MDG period had more ambitious goals of total poverty eradication, but with only weak monitoring since each country could individually set the target date to achieve this (Hulme, 2009). Given that there is nothing sacrosanct about a target based on a \$1 a day definition of poverty it should be justified on either empirical or logical grounds. Not being inherently special, it has to stand on its own merits. The aim of this background section is to provide some defense for the \$1 a day poverty line and to explain why reducing poverty at this level of living will remain a hard target to achieve.<sup>6</sup> Many features of global poverty reduction in the last two decades are unlikely to apply in the future, so it would be naïve to assume that the same poverty reduction will occur with business as usual.

The context for this defense of \$1 a day is that the current opportunity to consider new goals and targets for the post-2015 period has elicited a wide range of proposals – by one count, almost 900 different goals and targets.<sup>7</sup> Many proposals dilute the focus on \$1 a day poverty by advocating for other poverty targets. This push to dilute the focus on \$1 a day is present even in proposals made by researchers. For example, Ravallion (2013) calls for the monitoring of \$1 a day poverty to be supplemented with a measure of ‘weakly relative’ poverty calibrated to how national poverty lines vary across rich and poor countries. Relatedly, Klasen (2013) calls for a process of internationally coordinated national poverty measurement, where the same methods are used to set poverty lines in each country and the global poverty count is simply the sum of each country’s poverty count.<sup>8</sup> Pritchett (2013) argues for supplementing the low global poverty line with a high poverty line set at \$10-\$15 per person per day, based on poverty lines in rich countries. Alkire (2013) puts forward a view supported by many NGOs, that a multidimensional poverty index (MPI) should be used in addition to the existing \$1 a day poverty target.

In part this advocacy for other targets reflects the apparent success in achieving the MDG poverty target. In developing regions the share of the population whose living standard (mostly in terms of consumption) was below \$1.25 in 2005 PPP prices was 47% in 1990 and this share had fallen to 22% by 2010 (UN, 2013). In other words, the target of halving extreme poverty appears to have been met five years ahead of target.<sup>9</sup> This progress corresponds to 700 million fewer people living in extreme poverty than in 1990. Reaching the MDG target early may call into question the relevance of \$1 a day in a world where many people seem to have escaped this level of poverty. Indeed, optimistic forecasts are

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<sup>6</sup> Throughout this report, the phrase “\$1 a day” will be used (along with the term “extreme poverty”), even when referring to the current value of \$1.25 in 2005 PPP terms. The value will change again once the new ICP results are used to express the value in 2011 PPP terms, so it is convenient to refer to it as the \$1 a day line throughout.

<sup>7</sup> A chart which shows a cumulative count of the number of goals and targets proposed is available at: “Finding the Needle in the Post-2015 Haystack” <http://www.ipsnews.net/2014/04/finding-needle-post-2015-haystack/>

<sup>8</sup> Unifying the methods of setting national poverty lines will not give consistent poverty measures across countries if methods of surveying living standards continue to vary, as shown by the Beegle et al (2012) experiment with different consumption survey designs in Tanzania.

<sup>9</sup> Some researchers such as Pinkovskiy and Sala-i-Martin (2014a) suggest that not only was the poverty target met much earlier when considering developing countries as a whole but even in Africa, which has done the worst of all regions. According to these researchers, all countries in Africa other than the Democratic Republic of the Congo will reach the MDG poverty target one year ahead of schedule.

that if the developing world maintains the historically fast rate of growth in average living standards (as captured by surveys) that it has experienced since 1999, and there is no worsening of inequality, a further one billion people would be lifted out of \$1 a day poverty by 2027, leaving just 200 million people left in extreme poverty (Ravallion, 2013).

There is debate about these forecasts, with some studies suggesting that extreme poverty will be harder to reduce. Edward and Sumner (2014) note that if countries achieve only half their IMF growth forecasts and inequality continues to rise, more people may be in extreme poverty in 2030 than in 2010. Chandy *et al.* (2013) show that growth rates need to always exceed forecasts, by two percentage points per year, to cut \$1 a day poverty to 3% by 2030, which was the target set by the World Bank in 2013 separately from the MDGs. This study also shows that global poverty will increasingly concentrate in Sub-Saharan Africa, where it is less responsive to growth than in China or India, and that nearly two-thirds of the poor in 2030 will live in fragile states. Yoshida *et al.* (2014) relax the uniform growth rates assumption of Ravallion (2013) and use country-specific growth rates from 2002-10, and forecast 9% poverty in 2030; this exceeds what Ravallion forecasts because the poorest countries have slower growth rates. Conversely, Pinkovskiy and Sala-i-Martin (2014) argue that poverty reduction is understated and the world is already close to eradicating extreme poverty because surveys understate growth compared with national accounts; they use a third source of information – satellite-detected luminosity – to show that an optimal proxy for true growth gives surveys a weight of just 18% (and 82% on national accounts) and that progress in reducing poverty is faster with the optimally weighted proxy than it appears with the survey means in the World Bank's Povcal Net database.

These papers that forecast future global poverty counts are useful for highlighting the range of uncertainties that exist, but they provide little explanation as to why some aspects of poverty reduction in the past are unlikely to be present going forward. One could belittle many of these studies by describing them as 'playing around with Povcal Net'. There is free access to Povcal Net, and indeed Dykstra, Dkystra and Sandefur (2014) made 23 million queries to the database to make public the distributional data from each survey, so any insights to be gleaned from this source are likely to be already known. Thus it is unlikely that the world needs another paper based on Povcal Net and a lengthening of the list of mechanical exercises that say 'what if growth was  $Y$  rather than  $X$ ' while ignoring reasons for why growth in fact was  $X$ .

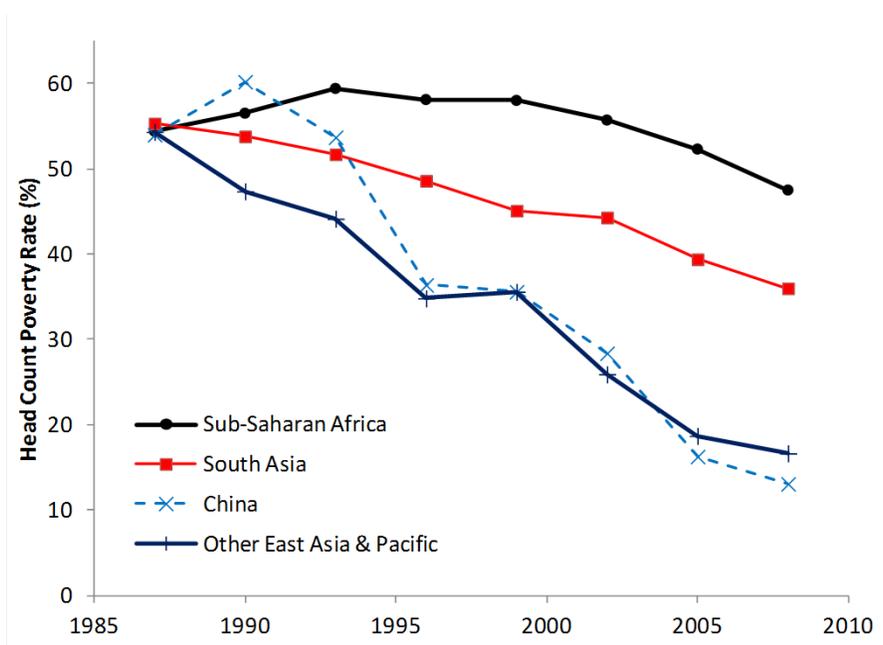
Instead, the goal of this background section is to discuss four features of poverty reduction that make past progress too sunny a guide to what can be expected in future: the role of one-off institutional reforms in East Asia whose effects are unlikely to be replicated elsewhere; the characteristics of rice as an ideal food for the poor, which gives East Asia another advantage in poverty reduction; how the escape from mass poverty reduces the effectiveness of growth and raises sensitivity to inequality, and what this implies for using surveys to monitor progress in a world of rising affluence; and, how the nature of poverty changes in a sociological sense, with the poor becoming less like the majority in terms of location, ethnicity, caste or religion. Each of these factors is likely to make poverty reduction going forward much harder than it was for the two decades from 1990 and therefore cautions against diluting the focus on \$1 a day poverty as a target. These factors

also suggest research questions that may be more fertile than simply giving new wrinkles to the theme of using different simulations of forecast growth rates and inequality in order to see what the time path of future poverty reduction might be.

### **The Record on Global Poverty Reduction**

The record on global poverty reduction over the last three decades is summarized in a number of papers by Shaohua Chen and Martin Ravallion (2001; 2010; 2013). These studies rely on a survey database that has grown over time to now include almost 900 surveys, from 125 countries fielded sometime between 1979 and 2011. With the exception of the Middle East and North Africa, which contribute less than one percent to the global poverty count, these surveys now cover more than three-quarters of the population of each region, and overall cover 90% of the developing world. The trends in the headcount poverty rate (the percentage of the population below the \$1.25 PPP 2005 poverty line) for Sub-Saharan Africa, South Asia and East Asia and the Pacific (EAP) are shown in Figure 1. The remaining regions of the world contribute less than four percent to global poverty and so are ignored here. The source of the figure is Chen and Ravallion (2013), who present the estimates for every three years from 1981. It is useful, however, to begin the discussion from 1987 when the poverty rates for these three regions were all within one percentage point of each other. The other difference from the original source is that China is divided out from the rest of EAP – typically in the global poverty counts the reporting is for China and also for all East Asia and the Pacific that includes China. Such reporting obscures that other parts of East Asia have been just as successful, with more continual poverty reduction than for China (e.g. compare 1987 to 1993), and so this moves the question from ‘what is special about China’ to ‘what is special about East Asia’.

Figure 1: The Uneven Escape from Extreme Poverty Around the World: Africa Lagging



Source: Chen and Ravallion (2013)

The trend annual rates of decline in the headcount poverty rate in Figure 1 are -7.3% for China, -5.7% for the rest of East Asia and the Pacific, and -2.0% for South Asia. All three trends are precisely estimated, with robust *t*-statistics that vary from 5.9 to 12.6. In contrast, one cannot rule out the hypothesis that there was no trend decline in poverty rates for Sub-Saharan Africa, with a coefficient on the time trend variable of -0.006 and a robust *t*-statistic of 1.4. Allow for the moment the possibility that there is something fundamentally different about the rate of poverty reduction in East Asia compared with in Sub-Saharan Africa. In 1987, 48% of the poor were living in EAP (including China) and only 15% were in Sub-Saharan Africa, but by 2008 these proportions had partly reversed; just 22% of the poor were in EAP and 30% of the poor were in Sub-Saharan Africa. The great success of East Asia in reducing poverty becomes less and less relevant to global poverty counts going forward, since few of the poor will be found there, while the evident lack of success in Sub-Saharan Africa increasingly matters to the likelihood of meeting global poverty targets. Moreover, several factors that contributed to poverty reduction in East Asia are not present elsewhere, as discussed in the next section.

### What Was Special About East Asia?

Many of the countries in East Asia can be described as highly capable states, with long histories of centralized government activity and social organization, but with recent periods of following misguided policy. China before Deng Xiaoping's reforms and Vietnam before *Doi Moi* are good examples but Cambodia under the Kymer Rouge, Myanmar under the generals and phases of despotic rule in Indonesia, Philippines and Thailand can also be described in these terms. The contrast between North and South Korea is perhaps the starkest example of how misguided policies and institutions can stymie the escape from mass poverty. In contrast, many poor people elsewhere live in countries that have always had very limited state capacity. It is easier for highly capable states to stop doing the wrong

thing than for weak states to start doing the plethora of right things needed across the economic and political spectrum. Hence, one-off institutional reforms in East Asia may have triggered an escape from mass poverty that is simply not replicable, so poverty reduction elsewhere will be harder than it was in East Asia.

There are many examples from the East Asian experience that can illustrate this point, but one will suffice; the abandonment of collective farming and the adoption of the Household Responsibility System (HRS) in China. The commune system that operated from 1958 to 1978 had resource allocation decisions made by commune authorities and their agents, the leaders of production brigades and production teams, leaving farm households with no control over capital, land and output (Wen, 1993). For the key input of labor, the commune authorities could allocate workers to tasks, and the production teams even used a work points system that was supposed to reflect the quantity and quality of effort that each team member performed (Lin, 1987). But a key difficulty in using factory-style work organization in agriculture is the difficulty of monitoring effort, because of the spatially dispersed nature of production, and the need for workers to make on-the-ground decisions due to heterogeneity of the other inputs, such as land and climate. The infeasibility of monitoring meant that shirking was common since the incremental income from an extra unit of effort by the work team member is their share of communal output, which was just a small fraction of the marginal product of their effort (Lin, 1987).

The Household Responsibility System solved the monitoring problem by letting peasants once again become the residual claimant so that they had every incentive to apply optimal levels of effort and no need to divert resources to monitoring. While land under the HRS was still owned by the village, it was contracted out to individual households who had control rights over cultivation decisions and who could keep all of their output after they delivered a certain quota of grain to the state and made payment to village authorities for rent, taxes and contributions to the public welfare fund. The origins of the HRS was as an illegal experiment started by villagers in Anhui province in late 1978, who changed the institutions of farming to pre-empt risk of famine (Anhui was the epicenter of mortality in the Great Leap Famine, 20 years earlier).<sup>10</sup> The official view at the time was that household farming was the antithesis of the socialist principle of collective farming and was prohibited. Lin (1987, p.410) notes that “the HRS was worked out among farmers initially without the knowledge and approval of the central government.” In other words, eventual adoption of this reform involved the state not doing something that damaged the livelihoods of the poor, by ending a misguided policy of collective farming.

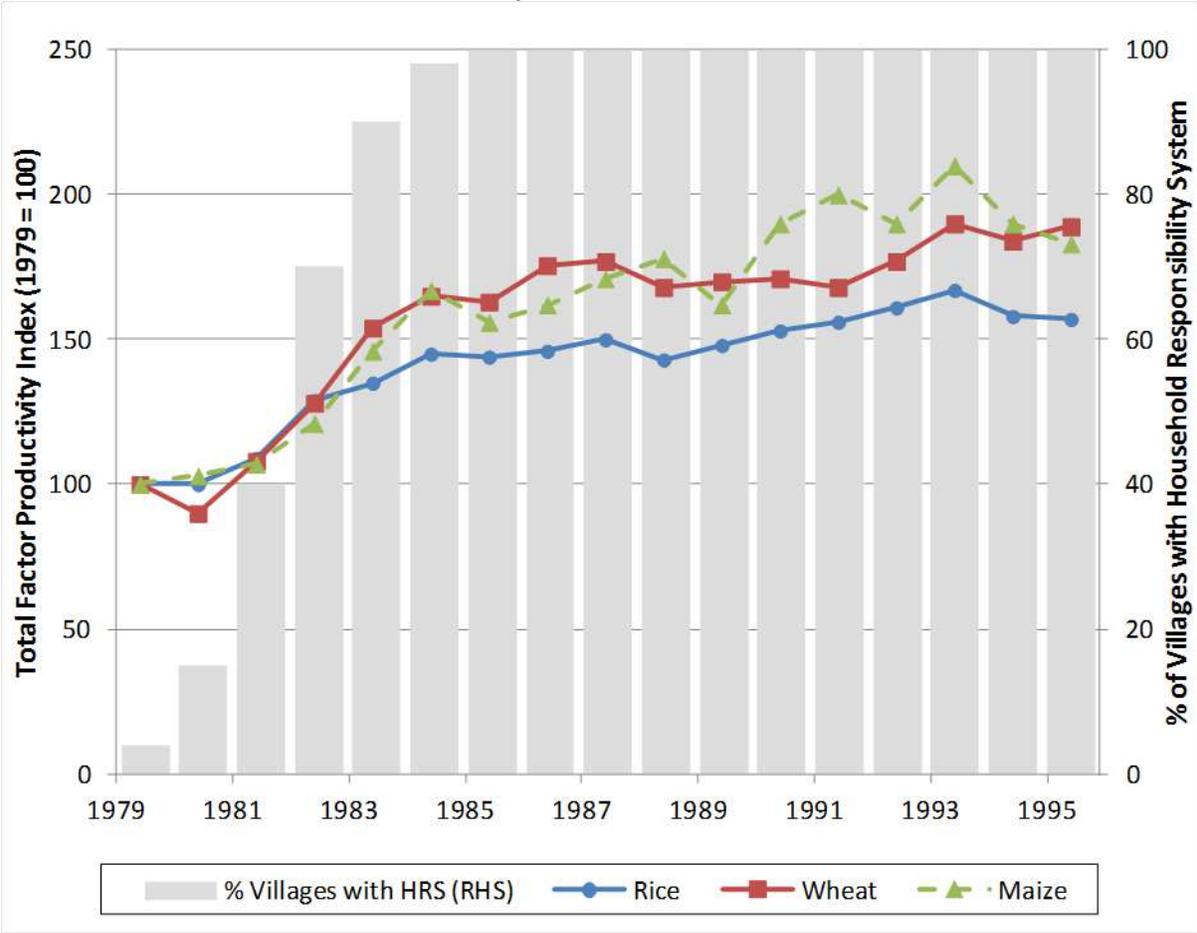
Between 1979 and 1984, when the Household Responsibility System went from being used in almost no villages to being used in almost all villages, agricultural productivity increased tremendously. Figure 2 shows trends in Total Factor Productivity (TFP) for the three main crops of rice, wheat, and maize, based on Jin *et al.* (2002). For this six-year period of institutional change, the trend annual rate of productivity increase was 8.4% for rice, 10.7% for maize and 12.2% for wheat; this rapid rise in output relative to the weighted sum of inputs stands in stark contrast to the pre-reform period where TFP had stagnated over much of the communal farming era. In fact, just prior to the beginning of the HRS era,

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<sup>10</sup> Yixin (2010) reports 6.3 million famine deaths in Anhui from 1958-61, equivalent to 18.4% of the population.

Total Factor Productivity was about 20% lower than it had been in 1957 when an earlier experiment with rural cooperatives ended (Wen, 1993). The rapid growth in productivity from 1979-84 also contrasts with the subsequent period, from 1985-97, when annual rates of TFP increase were much more modest, at 1.1% for wheat, 1.2% for rice and 2.2% for maize. Productivity growth in African agriculture is also low, but there is no single thing that African governments can do (or stop doing) that will raise TFP in the remarkable way that China experienced by abandoning collective farming.

Figure 2: Agricultural Productivity Jumps After Household Responsibility System Reforms in China

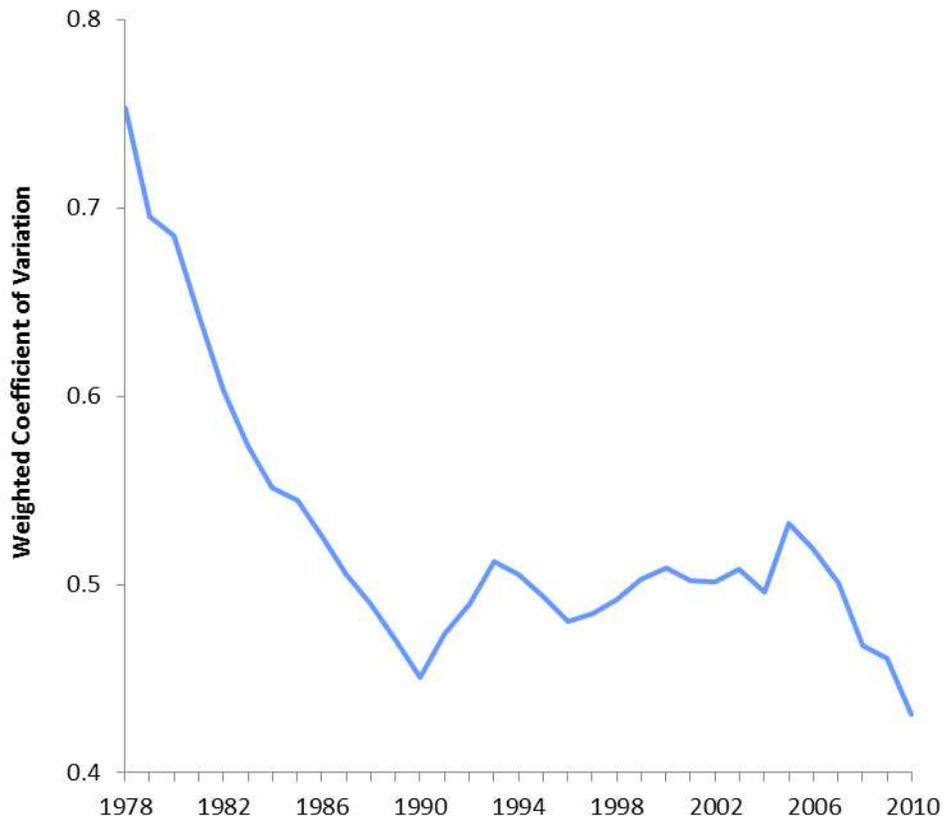


Source: Jin et al (2002)

Rural China was overwhelmingly poor at the time of the HRS reforms; Ravallion and Chen (2007) estimate that 76% of the rural population was below the poverty line in 1980. Yet by 1985, when the HRS had been fully adopted, the rural poverty rate had fallen to less than one-third of this level, at just 23%. It is not overstating the case to argue that the abandonment of collective farming in China is the greatest anti-poverty intervention in history. This one-off institutional reform, which has some parallels elsewhere in East Asia (e.g. the *Doi Moi* reform in Vietnam), had impacts which are unlikely to be replicated by any future reforms in countries where most of the poor now live.

A further feature of the institutional reforms in East Asia was their positive impacts on reducing inequality; this matters because as countries escape mass poverty the scope for further poverty reduction depends more on inequality than on growth (see below). Again using China to illustrate, Figure 3 shows how one form of inequality – that between areas – fell sharply during the HRS reform period. The figure shows the (weighted) coefficient of variation in provincial GDP per capita, but the pattern is the same if inequality is measured with the Gini coefficient or Theil index.<sup>11</sup> Inequality fell at a trend rate of 5.2% per annum (with a robust *t*-statistic of 34.2) from 1978 to 1984 (and thereafter showed no long-run trend).<sup>12</sup> The reforms helped rural incomes to close some of the gap with urban incomes, and this showed up at the provincial level as poorer provinces caught up to richer ones. Indeed, at the beginning of the reform, the coastal provinces south of Shanghai that were soon to be the epicenter of China’s boom were quite poor; they ranked in the second and third lowest quartile in terms of GDP per capita.

Figure 3: Falling Inequality As China Abandoned Collective Farming



While it would be ideal to examine similar trends with a time series of inter-personal or inter-household inequality, such data are unavailable. But this should not matter because in the early period of China’s reform, where one lived (and the type of *hukou* registration) was

<sup>11</sup> The coefficient of variation is:  $CoV = \sqrt{\sum_{j=1}^m (p_j/P)(y_{wj} - \mu)^2} / \mu$  where  $m=30$  provinces,  $p_j$  and  $P$  are the  $j^{\text{th}}$  province’s and the overall population,  $\mu$  and  $y_{wj}$  are the  $j^{\text{th}}$  province’s and the overall population-weighted mean GDP per capita. The corrected resident population series developed by Li and Gibson (2013) is used in the calculations.

<sup>12</sup> This is estimated from a semi-log regression on time, using Newey-West heteroscedasticity and autocorrelation consistent (HAC) standard errors with a single lag.

a key determinant of living standards. It was only as reform proceeded that inequality became less of a geographic issue as the market increasingly rewarded persons who had valuable characteristics (Benjamin *et al.* 2005). Similarly, the effects of local inequality on growth also faded over time (Benjamin *et al.* 2011). The key thing to note from this account of inequality is that the shared prosperity occurred because the government stopped following (some) misguided policies, which also raised productivity. In the countries where the poor are now concentrated, the tradeoffs between equity and efficiency are likely to be rather more severe.

### *The Role of Rice*

In addition to the impact of one-off institutional reforms, another key advantage for East Asia is from food; rice is more important in the diets of the poor in East Asia than in South Asia, and more important again than in Sub-Saharan Africa. In East Asia, rice provides almost 60% of calories in the diets of the poor.<sup>13</sup> In contrast, for South Asia rice gives just over one-third of the calories for the poor and is not much more important than wheat, which provides one-quarter of calories.<sup>14</sup> What is special about rice as a food for the poor is its negative income elasticity and different long-run price trend than the other two major staples – maize and wheat. Timmer (2014) notes that rice has been an inferior good at the global level since the mid-1990s and it was earlier shown to be an inferior good in East Asia from the 1980s (Ito *et al.*, 1989). In terms of price trends, the long-run rate of decline in rice prices is twice that of maize and ten times faster than the fall in wheat prices.<sup>15</sup> Thus, “even if maize and wheat prices remained stable in real terms, rice prices would be lower by more than 40 percent after a century” (Timmer, 2009, p.26).

There is some debate about whether low food prices are good or bad for the poor, with dispute due especially to general equilibrium effects on wages (Jacoby, 2013). But most studies see high food prices as poverty-increasing since there are many more net buyers than net sellers. Thus, simulations that multiply agricultural incomes and household food spending by relative food price rises typically find net effects of higher poverty (de Hoyos and Medvedev, 2011), even with wage changes counted (Ivanic and Martin, 2008).<sup>16</sup> Effects of long-run falls in staple food prices helping poverty reduction will likely be even more important in future since the poor urbanize faster than developing country populations as a whole (Ravallion, Chen and Sangraula, 2007), and the urban poor unambiguously benefit from lower grain prices (Wright, 2014). Thus to the extent that falling prices of the dominant food staple are a helpful factor in reducing poverty, the downward trend in rice prices gives East Asia an advantage over other regions of the world where maize, wheat, other grains, and root crops are the dominant staple.

The tendency for rice prices to fall faster than prices of other staples is no accident, and

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<sup>13</sup> This average, of 58% of calories, is based on cost-of-basic-needs (CBN) poverty line food baskets if available, and otherwise from reports on the composition of the diet broken down by income groups and/or sector. The calculation only covers rice as an ingredient, rather than the rice eaten as purchased meals and other ready-to-eat foods.

<sup>14</sup> The comparison uses World Bank developing country groups and is restricted to countries with populations greater than 5 million (but excludes North Korea).

<sup>15</sup> Timmer (2009) reports rice prices declined at -0.53% per annum over 1900-2008, conditional on prices of maize and wheat. For the other two staples, the conditional trend rates of decline were -0.27% (maize) and -0.05% (wheat).

<sup>16</sup> One of the few developing countries that may gain, in the aggregate, from higher prices of a food staple is Vietnam (Ivanic and Martin, 2008), but even there gains are more concentrated than losses, so higher rice prices make most households worse off (Linh and Glewwe, 2011).

reflects features of rice which make it close to a perfect food for the poor. As people escape poverty they quickly switch from rice and consume more income-elastic cereal products like bread, and also dairy, fats, and meats.<sup>17</sup> Rising consumption of these other foods does not raise indirect demand for rice, which is rarely used for animal feed. In contrast, a “food versus feed” competition arises with other staples as middle class demand expands – in terms of the animals eaten by the middle class (Yotopoulos, 1985). Thus, people escaping poverty in countries where wheat or maize is the main source of calories may put pressure on prices of these staples because those crops also feed animals. Humans have limits of 150-200 kilograms for the amount of grain they can eat per year as food but indirect demand through grain-fed meat and milk may approach 1000 kilograms per year since grain-to-meat conversion is so low. A further (tragically misguided) pressure on prices is biofuels policy; Wright (2014) shows how fuel blending requirements shift the output of calories from the food market (with low demand elasticities) to the price-elastic market for motor vehicle fuels, causing large price rises in the first market, little price change in the second market, and massive global wealth transfers from (poor) consumers to (rich) farmers. While maize (and oilseeds) is a primary feedstock for biofuels there is no direct biofuel demand for rice.

The fact that the main staple in the food poverty line has falling real prices and a negative income elasticity of demand gives East Asian countries an advantage in their escape from mass poverty. In other countries, rising staples prices due to middle class demand may trap some of the poor below the rapidly rising poverty line. This effect is readily apparent for nationally measured poverty using cost-of-basic-needs food poverty lines; these are just a Laspeyres index with a fixed basket of foods and if the main food in the basket (in terms of calories) has rising prices, more people will be found to live below the rising poverty line. But for \$1 a day poverty the effect is a little more subtle; more expensive food will lead to higher wages (Jacoby, 2013) and will tend to push up prices of non-traded goods. The PPP exchange rate will therefore be higher and the \$1 a day line in international prices will cut at a higher level in local currency prices, so more people will be found to fall below this poverty line.

### **Escaping Mass Poverty, and Rising Inequality-Sensitivity**

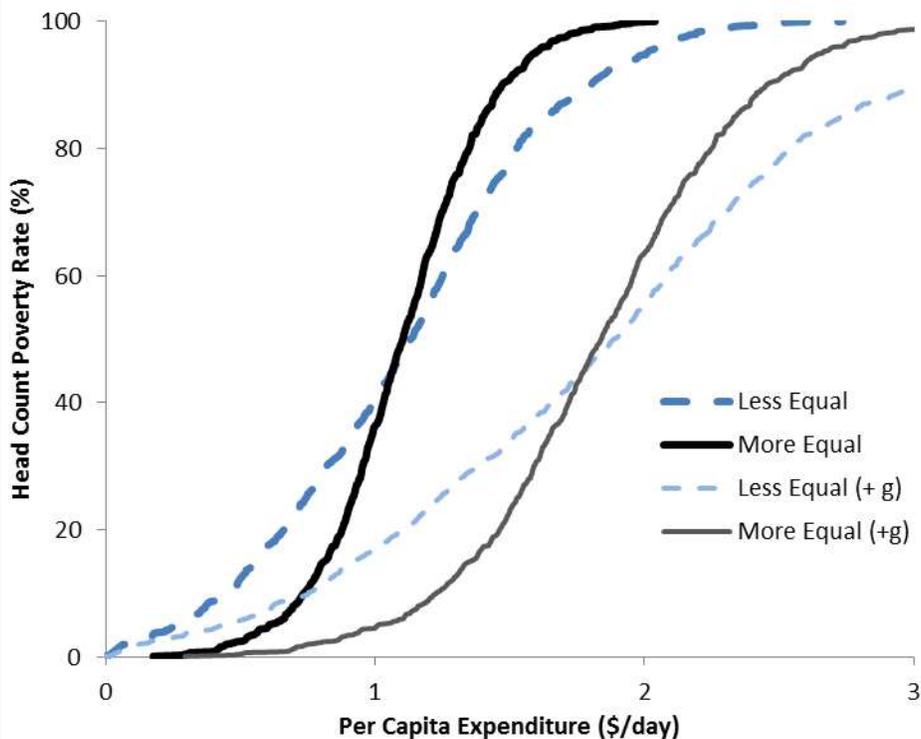
The poverty rate can be derived with three types of information: the mean of the living standards indicator, inequality around that mean, and where the line dividing the poor from the non-poor is located. This lets one perform thought experiments on how poverty changes as the mean is changed, holding inequality constant, and how it changes as inequality is changed holding the mean constant (Datt and Ravallion, 1992). The sensitivity to inequality and growth can be measured from such experiments and also may be estimated from variations over time and space in poverty rates, growth rates and inequality (Bourguignon, 2003). The evidence from a variety of settings is that as countries become less poor, the poverty rate becomes more sensitive to inequality and less sensitive to growth (Olinto *et al*, 2014).

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<sup>17</sup> For example, in Indonesia the poorest 40% in rural areas increased rice consumption by about 1% per year over four decades to 2006, the next two quintiles had static consumption and consumption of the richest rural quintile fell by about 1% per year (Timmer, 2014). For the poorest urban quintile, rice consumption was almost static, and demand fell monotonically for each richer quintile, with a fall of 1.6% per year for the richest quintile.

The intuition behind the rising inequality-sensitivity of poverty can be seen with a simple diagram. Figure 4 shows the cumulative distribution function (CDF) for a more equal and a less equal distribution with means of \$1.1 per day. The CDF is also known as the poverty incidence curve because one can directly see the head count poverty rate from where the poverty line (set at \$1 a day) cuts the CDF. When poverty is widespread, the poverty line cuts the CDF near the point of inflexion, where the CDF is almost straight, and the head count poverty rate is not very sensitive to inequality (seen in the curvature of the CDF). But the head count is sensitive to the location of the overall distribution (the mean) relative to the line because the CDF is steeply sloped in that region. In the example, the gap in the poverty rate between the two distributions is just five percentage points (41% and 36%) even though the coefficient of variation (CoV) of the less equal distribution is almost double that of the more equal one (0.46 and 0.27).

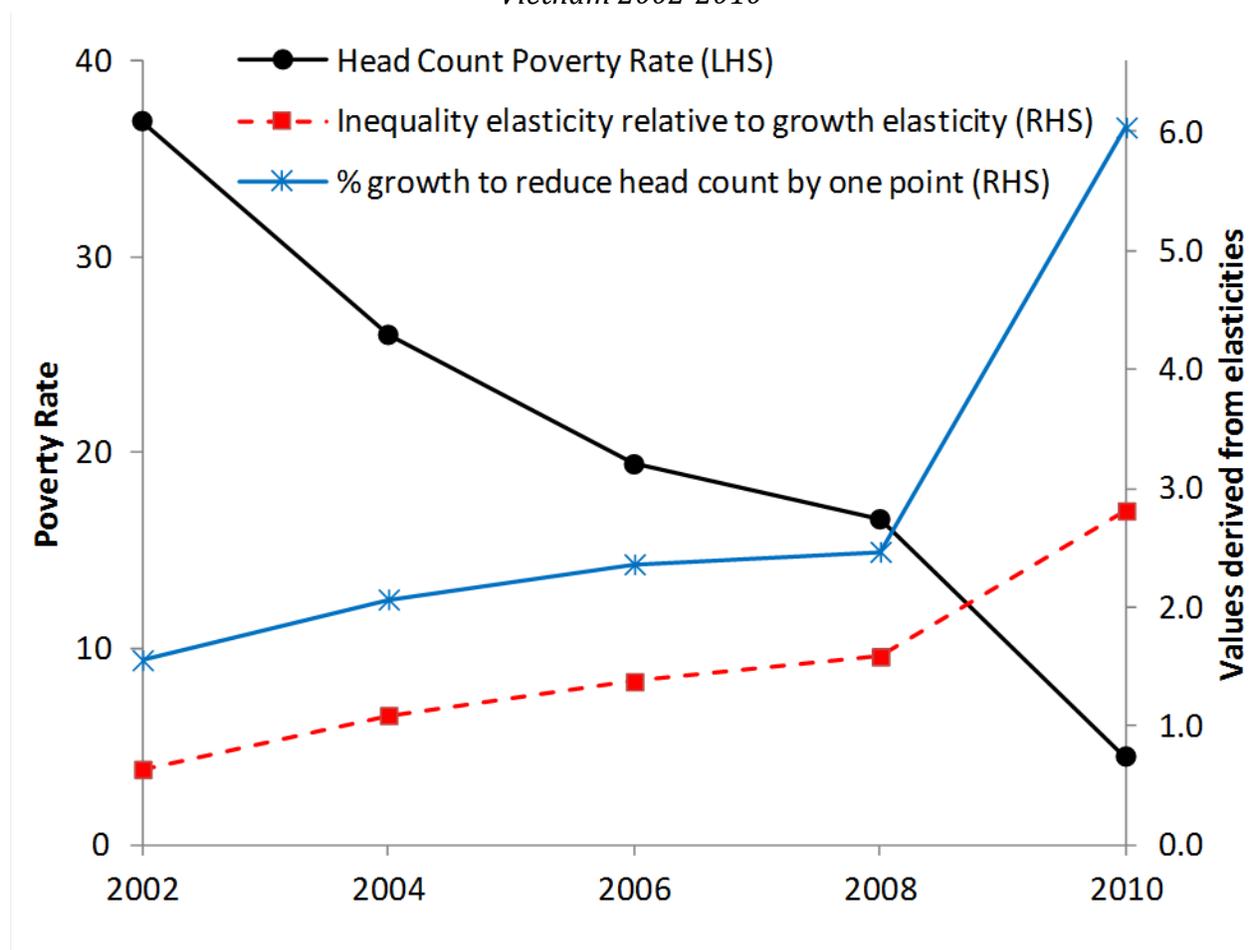
Figure 4: Inequality Sensitivity of Poverty Increases as Countries Grow Less Poor



The remaining curves in Figure 4 show the situation after a period of uniform growth, assuming that everyone's consumption rises by two-thirds. A big gap in poverty rates between the more equal (head count=5%) and less equal (head count=18%) distribution opens up, even with an equal growth rate. One could assume, instead, a form of pro-poor growth where the absolute rise in consumption was the same for everyone, so the growth rate was higher for the poor. This just shifts the curves in parallel and the large gap in the poverty rates under the less equal and more equal distributions still occurs. For example, adding \$0.5 to the consumption of everyone gives a head count rate of 12% with the less equal distribution and just 2% with the more equal distribution, making the gap in the poverty rates twice what it was before growth.

The poverty rate's heightened sensitivity to inequality and reduced sensitivity to growth as countries escape mass poverty is illustrated by the experience of Vietnam. At the \$1 a day global poverty line, the headcount rate in Vietnam fell from 64% in 1993 to 17% in 2008 (World Bank, 2012) and was down to about 5% by 2010.<sup>18</sup> Figure 5 shows this progress (using the left axis) and also what happened to the sensitivity to growth and inequality (using the right axis). When the poverty rate was high, the elasticity with respect to inequality was just over half the elasticity with respect to growth, but as the poverty rate fell the relative size of the inequality elasticity rose to almost three times that of the growth elasticity.<sup>19</sup> Another way to show the fall in growth-sensitivity is to consider what growth rate in mean consumption is needed to achieve a one percentage point fall in the headcount poverty rate; when the poverty rate was high in 2002, a growth rate of 1.6% per annum was sufficient to drop the poverty rate by a percentage point, but by 2010 it took an annual growth rate of 6% to achieve the same drop in the poverty rate.

Figure 5: Declining Effectiveness of Growth on Reducing Poverty as Poverty Falls, Vietnam 2002-2010



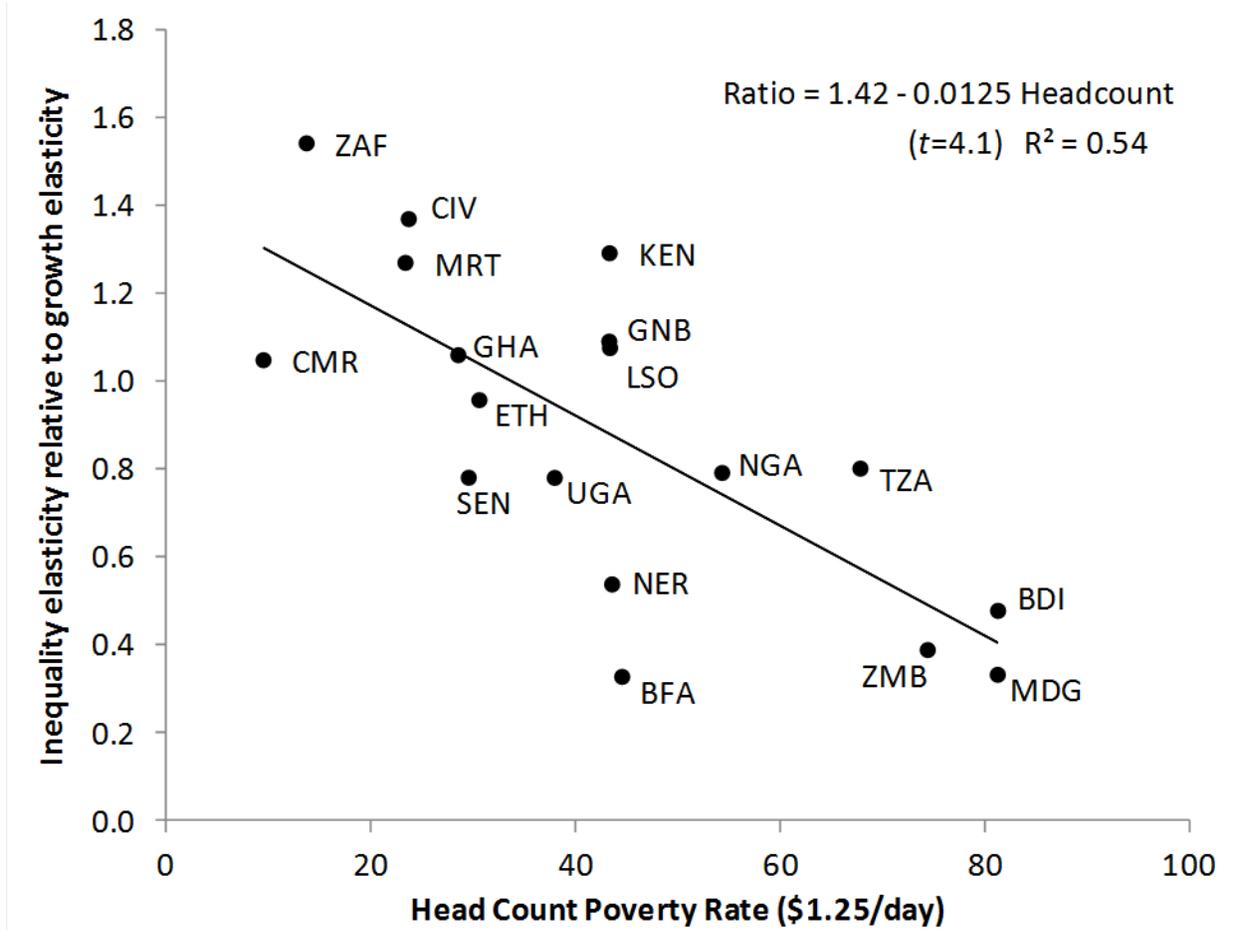
Source: Author's calculations from VHLSS data

<sup>18</sup> The uncertainty for 2010 is because the survey living standards indicator was revised then, to reflect Vietnam's rising affluence. Using the national CBN poverty line, (which was below the \$1 a day line) the poverty rate fell from 58% in 1993 to 15% in 2008. After being reset in 2010, the new welfare indicator and national poverty line gave a poverty rate of 21% in 2010.

<sup>19</sup> These elasticities are calculated from a Beta Lorenz curve, using the *Povcal* software of Chen *et al.* (2000).

A final example of the greater sensitivity to inequality and lower sensitivity to growth as countries get richer comes from Sub-Saharan Africa. Figure 6 shows the relationship between the head count rate, at \$1 a day, and the ratio of the inequality elasticity to the growth elasticity for several Sub-Saharan African countries. The elasticities and poverty rates were originally reported by Fosu (2014), who used an unbalanced panel of 123 countries over 1981-2007 to apply the method of Bourguignon (2003) but with a focus on Africa.<sup>20</sup> In countries with very high poverty rates, like Burundi, Madagascar and Zambia, the inequality elasticity is less than half the magnitude of the growth elasticity. Conversely, countries like Cameroun, Côte d'Ivoire and South Africa with much lower poverty rates have inequality elasticities whose absolute value exceeds the growth elasticity. The relative size of the inequality elasticity has a statistically significant trend decline (robust  $t=4.1$ ) when moving from less poor to poorer countries.

Figure 6: Relative Effects of Growth and Inequality on Poverty, Selected African Countries



Source: Author's calculations from estimates reported in Fosu (2014)

That poverty rates become more inequality-sensitive and more growth insensitive, as countries become less poor, matters to both policy makers and practitioners. The difficulty for policy makers is that reducing inequality, in order to eradicate poverty, can create

<sup>20</sup> The poverty rates are for the latest year, which ranged from 2002 to 2011. The countries where results were described by Fosu (2014) as 'perverse' (because of negative inequality elasticities) are omitted from the figure.

distributional conflicts and may be viewed as a zero-sum game. In contrast, economic growth can be presented as a positive-sum game and so growth-oriented policies may be subject to fewer rent-seeking losses than occur with attempts at redistribution. The difficulty for practitioners is that surveys may be doing a poor job of measuring the true level of and trend in inequality. But there is little attention in the economics profession to this situation, in contrast to debates about discrepancies in the growth record told with surveys versus national accounts (Deaton, 2005). While some studies experiment with different assumptions about whether survey means or national accounts better measure true growth (Chen and Ravallion, 2010; Pinkovskiy and Sala-i-Martin, 2009, 2014) there is uncritical reliance on inequality data from the surveys, and especially on the derived indicators of inequality in Povcal Net.

The first problem is that Povcal Net is a mish-mash of income surveys and consumption surveys, with consumption recorded in diaries or by recall, for varying commodity detail and periods, and reported at either the individual level or the household level. Thus the inequality data should not be expected to be comparable over space or time (since countries may change methods). For example, in a consumption survey experiment in Tanzania the Gini varied from 0.42 to 0.54 when eight different survey methods were randomly assigned to households (Beegle *et al*, 2012). These differences exceed the average difference between income survey and expenditure survey Gini coefficients in the widely used database of Deininger and Squire (1996). Methodological gaps in apparent inequality are likely to change over time. For example, Attanasio *et al.* (2004) show that trends in consumption inequality in the United States depend on the type of survey data used; the Consumer Expenditure Survey (CEX) has two completely separate surveys and according to the diary survey, there was a substantial increase in inequality from 1986 to 2001. In contrast, the interview survey showed no change in inequality over time.

Even with greater harmonization of survey methods, or a reliable way to convert between inequality estimates from various methods, the survey record on inequality would be deficient. The problem is that surveys do a poor job of capturing income-elastic consumption that becomes more important with rising affluence. Instead, the surveys pay much attention to food, which is of falling importance over time; for example, a long-run analysis of U.S. household expenditure surveys shows that food shares went from being ten times the share for recreation in 1900 to being smaller than the share for recreation by 1994 (Costa, 2001). The consumption perhaps most poorly measured by surveys is housing (including owner-occupied rents); with rising affluence this may become the largest component of household budgets.<sup>21</sup> Yet surveys do such a poor job of measuring housing that this form of consumption is often dropped from analyses (e.g. Deaton and Dupriez, 2011), or else is assumed to have a constant budget share (World Bank, 2012) so as to impute it from the parts of the budget that are better measured by surveys.<sup>22</sup> Both of these approaches have the equivalent effect of ignoring the disproportionate growth in housing

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<sup>21</sup> A weight of 44% is put on housing costs for poor people in a cost-of-living index in the U.S. (Jolliffe, 2006).

<sup>22</sup> For example, poverty analyses in Vietnam from the 1990s though until 2008 had assumed that the consumption of housing kept a constant proportion of other nonfood consumption over time, which had the effect of making housing appear to be just 6% of the total budget (World Bank, 2012). When this assumption was relaxed, and consumption was based on the fraction of self-reported dwelling values given by the rent-to-value ratio of renters, the share of housing in household budgets rose to 15%. Housing was 27% of the budget for the richest quintile and just 8% for the poorest, so relaxing the implausible assumption about constant housing shares greatly increases inequality.

consumption, which accrues mostly to the better-off, and so they will tend to understate rising (nominal) inequality.

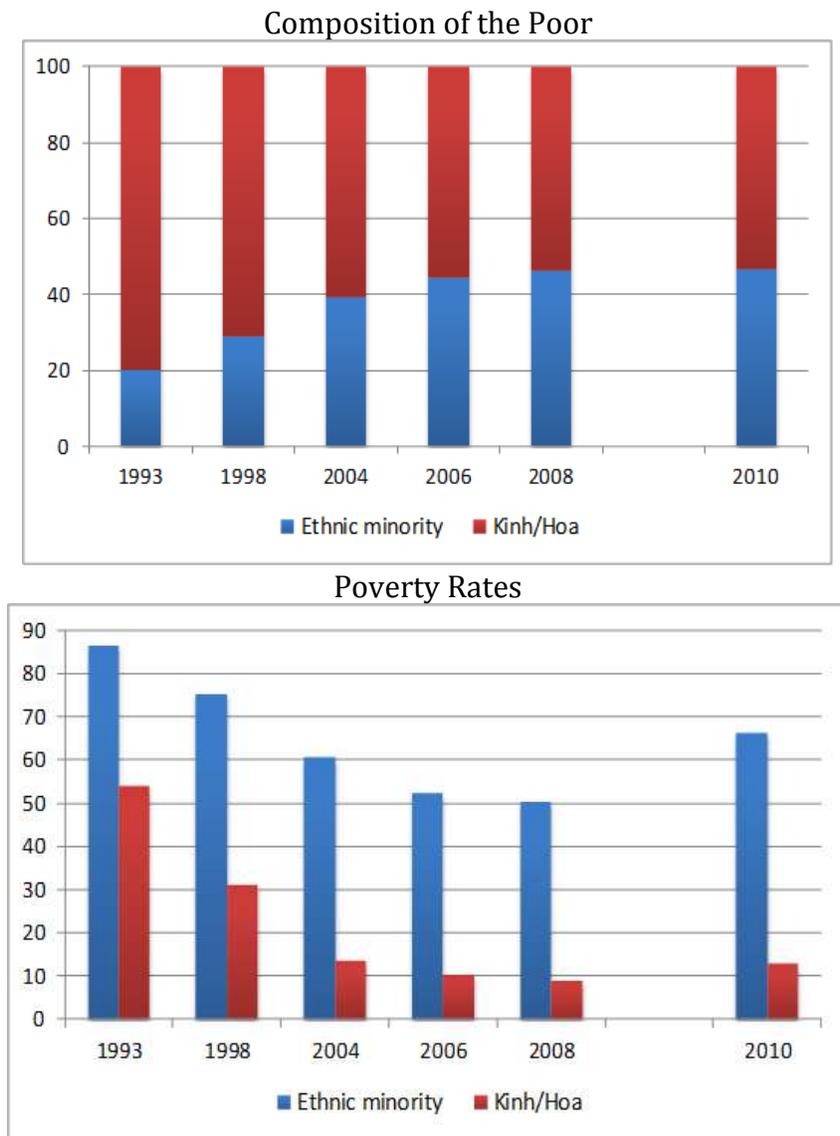
A further challenge to measuring trends in real inequality is that many poor countries have an almost total absence of spatially-disaggregated price data (Gibson, 2013). Without price data it is unclear if rising nominal inequality simply reflects increasing spatial price differences. Improvements in market integration with rising affluence may reduce spatial variation in prices of traded goods but for non-traded goods, like housing, the Balassa-Samuelson effect leads one to expect higher prices in areas with higher nominal incomes, so that real inequality is less than nominal inequality. For example, nominal inequality in urban China is overstated by up to 35% if no account is taken of the differences in costs of housing between provinces or cities (Li and Gibson, 2014). Particularly for China, because of the absence of housing markets under central planning, but probably more generally throughout rapidly urbanizing developing countries, these spatial cost of living differentials caused by urban housing markets are likely to have grown from a low base. Thus, some of the apparent rise in inequality found in many countries may just be a growth in spatial price differences rather than a rise in inequality of real consumption.

### **Changing Composition and Stubborn Persistence of Poverty**

When a country is mired in mass poverty, the average poor person has characteristics that will not be dissimilar to either the average person, or to the people making political and bureaucratic decisions about poverty. Indeed, decision-makers are likely to know people in their extended family and kin networks who are poor. But with the escape from mass poverty the poor increasingly begin to differ from the mainstream and therefore also from the decision-makers. In particular, the poor may live in different (often isolated) places, speak a different language, practice a different religion, or come from a different caste or ethnic group. The same policies and interventions that worked for the majority may not work as well for these minorities, and there also may be less political will to eradicate poverty if the poor are increasingly different from the mainstream decision-makers.

Consider the example of Vietnam, which has successfully escaped from mass poverty. When poverty was first measured, in 1993, four-fifths of the poor were either from the majority Kinh group or were Hoa (Chinese) and the remaining one-fifth were from ethnic minority groups (Figure 7a). But by 2010 the composition of the poor had greatly changed, with ethnic minority groups almost one-half of the total poor. The reason for this shift in composition is that the poverty rate for the Kinh/Hoa majority fell from over 50% to around 10% but for ethnic minority groups the headcount poverty rate never fell below 50% (Figure 7b).

Figure 7: Changing Composition of Poverty as Countries Escape Mass Poverty, Vietnam 1993-2010



Source: World Bank (2012)

The point of Figure 7 is that the poverty reduction that is yet to occur in Vietnam will need to be quite different to the poverty reduction that has already occurred. The factors that prevent ethnic minorities from participating in the general escape from poverty made by the majority will need to be dealt with. The experience of ethnic minorities in rich countries suggests that dealing to the factors that prevent some minorities from joining the mainstream may be a much harder public policy problem than just removing some of the impediments to economic growth, which triggered the escape from mass poverty. In this respect Vietnam is not atypical; for example, the analysis of poverty trends in India by Panagariya and Mukim (2014) shows that the headcount poverty rate of scheduled castes and scheduled tribes was 32% above the all-group poverty rate in 1983, but by 2009/10 these minority groups (comprising one-quarter of India's population) had a poverty rate 44% above the average. Although poverty fell for all groups, the composition of poverty was shifting more towards these disadvantaged minorities.

Moreover, a characteristic of many poverty-stricken ethnic minority groups is their higher fertility and younger age structure. For example, the 2001 census for India reveals a total fertility rate for the scheduled castes of 15% above the all-group fertility rate and for scheduled tribes of 25% above the all-group rate (Bose, 2001). In China, this factor is exacerbated by the exceptions to the one-child policy granted to ethnic minorities; the 2010 Census shows that while minority groups make up only 7.9% of the working age population they are 11.3% of the youth and child population (<15 years). If ethnic minorities are a growing share of future populations and if they have structurally higher poverty rates the overall poverty rate will exhibit a stubborn persistence.

These structural gaps may take generations to close, since they appear in early life in the form of schooling achievement gaps. For example, surveys of primary schools in rural China (Shaanxi, Gansu and Qinghai provinces) show non-Han minority students whose mother tongue is not Mandarin score more than 0.6 standard deviations below Han students in standardized exams (Yang *et al*, 2013). These achievement gaps will lead to lower educational attainment, worse occupational outcomes, lower relative incomes and a persistence of poverty amongst these minority group members. All of these factors make a target of getting to zero poverty likely to be harder to attain than it was to transition from a society of mass poverty to one of low poverty. Since zero poverty will be hard to achieve, changing the goal posts by shifting the focus away from \$1 a day poverty seems unwise; if a different type or level of poverty is targeted it won't be clear if the originally targeted poverty in the MDGs is yet eliminated, especially in light of our ignorance about poverty that is discussed below.

### ***We Know Less Than We Think About Poverty***

Many of the studies on global poverty targets use as their data the information available from Povcal Net on mean levels of consumption or income and on various measures of inequality from the surveys collated there. In some sense these are not really data at all, and instead are a processed end-product whose starting point was actual data, but data that came from some very disparate surveys that often do a poor job of measuring living standards and poverty in the countries where they are fielded. One consequence of the over-reliance on Povcal Net is that too little attention is paid to these survey problems, since analysts are not confronted with how truly messy are the data available for measuring poverty and for monitoring progress in raising living standards. The aim of this section is to discuss some of the areas where the evidence that is needed for monitoring suggested post-2015 targets related to poverty is weak and could be improved.

### **The Survey Information Gap in Africa**

The evidence from the global poverty counts is that Sub-Saharan Africa is the region where the trend rate of poverty reduction is least apparent (Figure 1). This region also has some of the weakest statistics for monitoring poverty. The literature already notes that survey coverage is worse for Africa (including North Africa and the Middle East -- MENA) than elsewhere. For example, the survey database used in the poverty counts by Chen and Ravallion (2013) covers 62 percent of the population of Sub-Saharan Africa over 1987-2008 (and 61 percent for MENA), versus 93 percent of East Asia and the Pacific, and 98% coverage for South Asia. These global poverty counts assume that poverty rates in

countries without surveys are the same as the average poverty rate for their region, as established from the countries with surveys. This assumption of missing at random is more important for Africa than elsewhere since extrapolation from countries with surveys to those without is needed for more countries in this continent than elsewhere (Garcia-Verdu, 2013). A natural assumption would be that countries without household surveys are doing worse than others (for example, the list includes Somalia, Sudan, and Zimbabwe) since weak statistical systems are one byproduct of weak economic performance. If so, reliance on a missing at random assumption may be a source of bias in the global and regional poverty counts that include the extrapolated figures.

It turns out that this survey coverage issue is probably not the main problem for the survey information gap for Africa. The available proxy data suggest that African countries that are not in the Chen and Ravallion samples because they lack household surveys have not grown at a slower rate than the African countries with household surveys (the available data can say nothing about inequality, or more directly, about poverty). As evidence for this point, data on satellite-detected luminosity are used to proxy for economic growth, following an approach that has recently become popular in economics. Specifically, national level stable lights for each country and each satellite year reported by Elvidge et al (2014) are used in the analysis. It should be noted that the night time lights that are detected are mainly from urban areas and enclave developments (Small and Elvidge, 2013) and so this is not necessarily a very good proxy for rural growth (which is why the use of luminosity data by Pinkovskiy and Sala-i-Martin (2014) to supposedly settle debates about the rate of poverty reduction may be inappropriate since most poverty is rural). Nevertheless, this proxy is both timely and widely available, and so is hopefully informative about potential sample selection biases in the African countries with poverty data.

Let the (log) of the sum of lights  $L_{its}$  for country  $i$  in year  $t$ , as measured by satellite  $s$ , be modeled with a fixed effects regression, as follows:

$$\ln L_{its} = \beta_0 + \beta_1 T + \lambda(T \times CR_i) + \delta_s D_s + \gamma_i D_i + \varepsilon_{its}$$

where  $T$  is a time trend,  $D_s$  is a set of fixed effects for each satellite,  $D_i$  is a set of fixed effects for each country, and  $\varepsilon_{its}$  is a random error. The estimated coefficient of most interest is  $\hat{\lambda}$  because this indicates whether the time trend in lights is the same for African countries in the Chen and Ravallion sample ( $CR_i=1$ ) compared with the countries not in the sample ( $CR_i=0$ ). The regression controls for the fixed characteristics of countries (the  $D_i$ ) and also the fixed characteristics of satellites (the  $D_s$ ) which each have their own idiosyncratic measurement error processes. Both sets of fixed effects are relevant but are not reported here to save space. The regression results are:

$$\ln L_{its} = 0.045 T - 0.019 (T \times CR_i) + \delta_s D_s + \gamma_i D_i \quad R^2 = 0.98$$

(0.004)      (0.003)

where standard errors are in ( ). The trend annual rate of growth in national-level lights for African countries not in the Chen and Ravallion samples is 4.5 percent. For countries in their samples the trend growth rate is only 2.6 percent and the difference of 1.9 percent is

statistically significant ( $t=5.9$ ). In other words, the African countries with household surveys available are not necessarily growing faster than those without surveys, and may in fact be growing slower. So sample selection bias is probably not a major contributor to the survey information gap and uncertainty about poverty trends in Africa.

Instead of a problem of the countries with household surveys being a potentially non-random subset of all African countries, the bigger problem is with price data. A preliminary analysis by Sandefur (2014) suggests that the Consumer Price Index (CPI) estimates for many African countries understate the true degree of inflation and thereby overstate real economic growth. This source of bias matters for poverty counts because the CPI data influence the PPP values used to convert \$1 a day international poverty lines into poverty lines in local prices that are then compared with survey estimates of nominal consumption. If the CPI is understated, the PPP conversions will not inflate the local values of the \$1 a day poverty line enough in subsequent years, and progress on poverty reduction will misleadingly seem to be better than it is. As an alternative to the CPI measure of price changes, which is largely based on urban surveys even though the population in Africa is mostly rural, Sandefur uses a time series of CBN poverty lines for several African countries to construct alternative PPPs. These alternative PPPs imply lower rates of poverty reduction than what is suggested by the deflators that World Bank poverty counts are based on. The downward bias in the official CPI measures for African countries may not be an accident; there is evidence that official statistics in other areas (education and health) in Africa are biased as a result of what Sandefur and Glassman (2014) claim is systematic distortion due to incentives to overstate development progress. Obviously the understatement of inflation also has the effect of overstating real economic growth. With limited information available for comparing prices over time, especially in areas outside of major cities, this uncertainty about real economic performance and poverty reduction in Africa is likely to persist.

### **The Problem (for surveys) of Rising Affluence**

The data workhorse for poverty analysis in developing countries is a Household Consumption Expenditure Survey (HCES).<sup>23</sup> While income surveys are widely used for poverty analysis in rich countries, amongst developing countries it is only in Latin America where income surveys are used more than HCES. The design of a HCES can differ along several key dimensions, such as the method of data capture (diary versus recall questionnaires), the level of respondent (individual versus household), the reference period for which consumption is reported (anywhere from one day to one year), and the degree of commodity detail (from less than 20 items to over 400 items).<sup>24</sup> Despite these differences, the design of most HCES reflects an implicit goal of capturing the living standard of the members of a household who eat meals from a common pot that the householders themselves have cooked from ingredients that they acquired by either purchase, receiving as a gift or payment, or self-production.

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<sup>23</sup> I use the generic term HCES to refer to a range of household survey efforts to capture total household consumption expenditures, which includes budget surveys, income and expenditure surveys, living standards surveys, and others. Gibson (2006) discusses the comparative strengths of each type of survey for poverty analysis.

<sup>24</sup> See Beegle *et al.* (2012) for a fuller discussion of these design variants and for evidence on the sensitivity of poverty estimates and poverty profiles (the characteristics of the poor) to variation in HCES design.

In recognition of this type of target household, the food modules of these surveys are organized according to lists of ingredients such as rice, wheat flour, maize flour, sweet potatoes, and so forth. The important staples may warrant several lines on the list, such as for different rice varieties according to quality, season of production, stickiness and so forth. At the end of the list for each group of ingredients of a particular type (such as 'meats', 'vegetables', or 'fruits'), there will be a catchall category for all of the other examples of that type of ingredient, such as 'all meats not otherwise specified'. These residual categories are one place where more consumption is found with rising affluence, since diets diversify away from the more limited set of ingredients that were used to cook most meals when people were poorer. What is recorded for these catchall items is almost always just monetary values of the amount paid (for purchases) or the imputed values (for what is consumed) with no quantities recorded (since the individual goods in the catchall are too heterogeneous to allow easy recall of quantity in standardized units and too diverse to allow easy processing of the data, e.g. they may vary widely in calorie content). Thus it becomes harder over time to construct the unit values (expenditures over quantity) that are often used as proxies for food prices because the share of food consumption with unit values available is falling over time. Consequently, calculating the real value of food consumption by taking account of spatial and temporal price variation becomes more difficult, and what may appear as rising inequality could just be shifts in the diet towards residual category foods that have rising prices (due to shifts in demand for these foods caused by the diversification of diets) that spatial (if existing) and temporal price indexes are not able to capture.

The other problem for surveys created by rising affluence and by the related structural shift to urban livelihoods is that the basic design of HCES questionnaires is no longer suited to the population being surveyed. Asking about ingredients makes sense for people who prepare their own meals, and made sense at baseline (1990) for the MDGs when most of the poor were still rural and eating as a family. But this approach to surveying makes less sense for people who obtain food independently of other family members, in the form of prepared meals, whether as street foods, in restaurants or purchased to heat and eat at home. The poor urbanize faster than developing country populations as a whole (Ravallion et al, 2007) so an ingredients-based, common-pot method of measuring consumption will be increasingly ill-suited to measuring poverty for the period from 2015-30 that new global poverty targets will cover.

Consider first the problem of independent eating, or more broadly the issues for surveying that arise from the fact that in urban areas family members may only sleep together but not work together and eat together as they did in the countryside. In a survey experiment in Tanzania, Beegle et al (2012) compare two types of HCES diary surveys; one has each adult recording their own commodity acquisitions while the other has a diary filled in by one respondent on behalf of the whole household. For rural households there is no difference in the consumption recorded with one type of diary or the other. But urban households report 29 percent lower consumption, if surveyed with a household-level diary rather than having each adult keeping a diary. The large error when using a HCES design that seemed to work fine in the countryside (the household-level diary) shows the challenge facing HCES methods if they are to accurately measure living standards in future as poverty urbanizes and as common-pot measuring techniques become less relevant.

Similar challenges face the ingredients-based approach. Many urban people have no idea what ingredients are in the food they eat, since they buy meals rather than raw ingredients. Yet, in many HCES, meals eaten outside of the home are little more than an afterthought, being recorded with just one or two questions that ask only about expenditures and not about quantities or numbers of meals. A failure of most HCES to pay sufficient attention to eating out contributes to several puzzles and also is likely to affect trends in poverty. For example, India has had a substantial debate about the apparent rise in under-nutrition during its recent period of rapid economic growth and declining poverty.<sup>25</sup> Over two decades from 1987-88, mean calorie consumption per capita appeared to fall by 10 percent and the under-nourishment rate grew from one-quarter to more than one-third of the population, all while India was recording some of its fastest ever economic growth rates and while the poverty rate fell from 54 percent to 42 percent (Smith, 2013). Hypotheses put forward for the puzzle include relative price changes, rural impoverishment, declining calorie needs with urbanization, dietary diversification, and a squeeze on the food budget due to rising expenditures on non-food essentials (Basu and Basole, 2013). Yet the most likely explanation is much simpler – and more troubling – the HCES evidence that is relied on for much of the debate increasingly understates calorie consumption because it misses the rising share of calories coming from eating out. A similar data problem occurred in China; in the 1990s the per capita quantity of meat consumed appeared almost static, despite rapid growth in incomes and rapid growth in meat supply (e.g., pork supply appeared to be 45 percent higher than pork demand), and despite rapid rises in meat consumption in other East Asian countries during their periods of similar high rates of growth in income. At least part of this gap was due to the failure of consumption statistics based on HCES data to account for the meat (especially pork) that was indirectly consumed as meals during eating out occasions (Ma, Huang, Fuller and Rozelle, 2006).

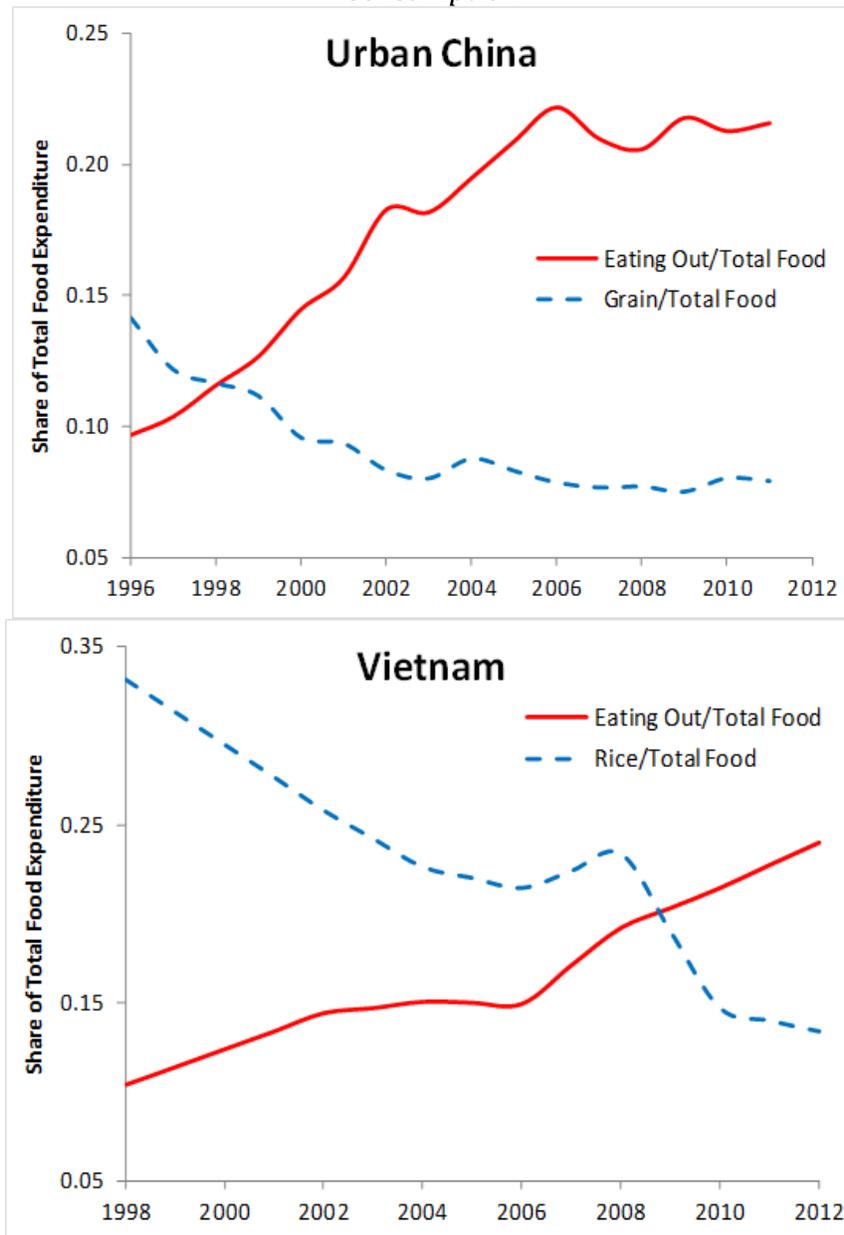
The driving force behind these puzzles, and the source of the rising error with HCES approaches that emphasize food ingredients, is the rising share of meals eaten out of the home. Meals out are highly income elastic and so their budget share rises with rising affluence, and they quickly become the most important category of food expenditure. But one would not know this from looking at the design of survey questionnaires which have very many questions devoted to ingredients and few devoted to meals. Figure 8 reports trends in the share of total food expenditure devoted to eating out, for urban China and national Vietnam. The trend in this ratio is compared with the trend in the share of the food budget spent on the major ingredient – grains in urban China and rice in Vietnam. In urban China, the total for all grains, such as rice and wheat flour (in terms of what is acquired as ingredients rather than what is consumed) has fallen from one-seventh of all food spending in 1996 to just one half of that level by 2011. In contrast, over the same period eating out has risen from under one-tenth of the food budget to now being almost one-quarter of total food spending. These two budget shares crossed back in 1998 and by 2011, when the series stops, spending on eating out was almost three times that of spending on grains. A similar pattern is apparent in Vietnam, where spending on rice went from being one-third of total food spending in 1998 to just one-eighth by 2012, while in the other direction spending on meals went from 10 percent to 24 percent. The crossing point occurred later than in urban

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<sup>25</sup> See Deaton and Drèze (2009) and Patnaik (2010) for the beginnings of this debate.

China, in part because the results for Vietnam use national level data, but the implication is the same. A sensible approach to measuring food consumption, both for the purpose of monitoring the time trend in poverty and also for helping to measure hunger, would pay much more attention to eating out and other types of meals that households don't produce themselves and less attention to the detailed recording of food ingredients acquired.<sup>26</sup>

*Figure 8: Changing Importance of Ingredients and Eating Out in Household Food Consumption*



<sup>26</sup> The rapid rise in the share of food expenditure on eating out also introduces uncertainty into cost of basic needs (CBN) poverty lines (but not the \$1 a day international line). These CBN poverty lines price a basket of food that delivers a specified level of calories (typically, around 2000 per day) and have to make assumptions about the cost per calorie from meals eaten out (and from all of the other foods that do not have quantities measured in the survey). These assumptions may not have mattered much when spending on eating out was a minor part of total food spending, but they matter much more now that the share of the food budget on eating out exceeds one quarter in many countries.

Yet the evidence is that this problem for surveys of dealing with rising affluence is not being dealt with. The detailed metadata survey of Dupriez et al (2014) summarizes various relevant dimensions of HCES design related to food acquisitions and consumption (requiring a 22-page form to cover all variants) from a sample of 100 low- and middle-income countries. A majority of these surveys use the interview method, where a single respondent reports on the household's consumption and/or expenditure activities over some prior reference period(s). Amongst the interview surveys, the average number of groups in the food list is 110. Just three of these 110 groups are for meals and other forms of food eaten away from home. In contrast, traditionally important ingredients categories like cereals or vegetables each have an average of 14 groups listed, where for each group there will be a prompt about whether any example of the particular group (such as rice, wheat flour, bread, potatoes, onions) was acquired and/or consumed in the reference period. Moreover, most of the food-at-home groups also have data on the quantity purchased, self-produced or consumed. In contrast, a majority of the surveys that ask questions about food eaten out of the home do not have any quantities of this consumption reported. Thus it is impossible to know how many calories come from eating out of the home. In summary, HCES design is increasingly ill-suited to gathering the data needed for assessing the food consumption of a more affluent and more urban population and this hampers our ability to consistently compare poverty over time.

The other form of consumption that is poorly measured, and whose high income elasticity makes it of growing importance, is housing. Ideally, surveys would let the consumption of housing services be calculated for both renters and owners. Similarly, variation in the price of housing services over time and space should be measured so that comparisons of consumption (and poverty) are in real terms. While most household surveys will measure actual rents paid, in many poor countries renters are a small minority of the sample and it is difficult to extrapolate from rents paid by these renters to imputed rents for others. Moreover, most surveys do not collect needed information on either replacement costs for dwellings, or historical costs, life expectancies and depreciation rates that could enable the flow of services provided by the dwelling to be valued. Consequently, consumption of housing services is often dropped from analyses (e.g. Deaton and Dupriez, 2011), or else it is assumed to have a constant budget share so that it can be imputed as a ratio of other parts of the consumption budget that are better measured (World Bank, 2012). In both cases, the implicit assumption is that patterns over time and space in housing are the same as they are for other consumption, which ignores the high income elasticity and growing share of housing in the consumption basket.

This treatment of housing by either dropping it or treating it as a fixed ratio to other forms of consumption also ignores the implications of the Balassa-Samuelson effect, that prices of non-traded goods are higher in richer areas. Since housing is the quintessential non-traded good, it would be expected that richer areas within a country would have higher housing prices, and that these spatial price differentials will grow over time as an economy urbanizes and becomes more affluent. Consequently, it is unclear how one should interpret inequality estimates that come from surveys that do a poor job of measuring housing consumption and housing prices. In these circumstances it is somewhat puzzling that some researchers (e.g. Pinkovskiy and Sala-i-Martin, 2014a) trust the surveys to measure inequality even when they do not trust them to measure mean levels of consumption or the

growth in living standards over time. Rather than being trustworthy, it is likely that there is a time-varying error in survey measures of inequality since housing has a rising share of consumption over time.

### **Life Satisfaction and Happiness Data Don't Help**

The proposal by Jeffrey Sachs that every country will collect data on reported life satisfaction is part of a broad thrust for governments to adopt policies that aim to promote increases in surveyed measures of wellbeing (either happiness or life satisfaction). These survey measures are available for a growing number of countries, but popularity does not mean they are a correct basis for policy. Welfare economics does not justify maximising either happiness or life satisfaction, since neither correspond to utility. A useful example of this point is given by Glaeser, Gottlieb and Ziv (2014) who consider internal migration in the United States; on average, movements of people are towards cities that score higher on survey measures of life satisfaction, but many movers go to less happy cities and experience the lower life satisfaction of the area they have chosen. Since no one is forcing these people to move, their revealed preference is that the move makes them better off. How can this be? The answer is that unhappy cities tend to be declining cities (the 'rust belt' especially) and these areas offer higher real incomes because of the low cost of housing. These higher real incomes partly compensate for lower reported life satisfaction, which is a trade-off that people are willing to make because they are maximising utility – of which life satisfaction may be one argument but that does not make it the same as utility.

At a more conceptual level, McCloskey (2012) provides an entertaining discussion of the problems with research by economists and psychologists that measures happiness as self-reported declarations of one's level of happiness, with the self-reported numbers assigned to this level then added up and averaged across people and eras. These efforts entail asking survey respondents where they fall on a three-point scale, 1-2-3: "not too happy," "pretty happy," "very happy" which is an accurate description of the General Social Survey, although some other surveys such as the World Values Survey or the Mental Health Inventory may use 4- or 5-point scales. The problem with this effort is that when these numbers are treated as data that are to be used in the same way that incomes, expenditures and other quantitative measures are used, the researchers are guilty of mixing up a "non-interval scale" with an interval scale. For example, the gap between, say, a "1" and a "2" is not the same across different people, and may not be the same in different times for the same people. As McCloskey notes (2012, p.4):

If a man tormented by starvation and civil war in South Sudan declares that he is "happy, no, very happy, a regular three, mind you," we have learned something about the human spirit and its sometimes stirring, sometimes discouraging, oddity. But we inch toward madness if we go beyond people's lips and claim to read objectively, or subjectively, their hearts in a 1-2-3 way that is comparable with their neighbors or comparable with the very same South Sudanese man when he wins an immigration lottery and gets to Albany.

The temptation to interpret these happiness and subjective wellbeing values as cardinal measures is strong but it is likely to lead to misleading conclusions and bad policies.

Bond and Lang (2014) expand upon this theme by showing that standard happiness measures cannot rank the average happiness of two groups (or the same group in two time periods), unless researchers are willing to make strong assumptions about the underlying (and unknown) distribution of happiness. The problem occurs because a continuous variable (life satisfaction or happiness) is being placed into discrete categories. It is possible to reverse the average happiness ranking between two groups (or countries or time periods) by using different monotonic transformations. In a time series example, Bond and Lang (2014) relate mean self-reported happiness in the United States to real per capita GDP over 1972-2006, with a negative (but statistically insignificant) time trend found. However if a monotonic transformation of the distribution of happiness is sufficiently left-skewed, under what they call the ‘Tolstoy Assumption’ (*All happy families are alike but each unhappy family is unhappy in its own way*) then a positive and statistically significant relationship between GDP and happiness is found. Similarly, cross-country rankings from the World Values Survey are sensitive to the use of left-skew or right-skew transformations. A left-skew suggests that the highest average happiness levels are in small, rich OECD countries like New Zealand, Sweden, Canada and Norway, while the least happy countries are in Africa (Ghana, Zambia and Ethiopia). If happiness is assumed to be normally distributed, Mexico is the happiest country and the top countries under the left-skew ranking fall to 10<sup>th</sup> place on average. If happiness is assumed to be right-skewed, Ghana becomes the happiest country and the small OECD countries that were at the top under the assumption of a left-skew distribution drop to 22<sup>nd</sup> place on average.

Of course researchers might assert that happiness is distributed in a particular way; for example, they could claim that it is normally distributed. Bond and Lang (2014) note that making such an assertion amounts to assuming the conclusion that is reached. The distribution of income, and even more so of wealth, is skewed to the right (that is, the mean is well above the median). Thus, if happiness is assumed to be normally distributed then, by construction, the marginal effect of income or wealth on happiness will tend to be strongly decreasing (given the right skew in income and wealth). Thus, the large literature that suggests diminishing returns to income in terms of happiness rests on shaky foundations, since it depends on an assumption about the unknown functional form of the distribution of happiness. This literature is often used to support many non-monetary rankings like the Human Development Index that put declining marginal weight on income, so policy makers who want to use such rankings should be aware of these shaky foundations. Thus, notwithstanding the problems with survey reports of monetary measures, such as consumption, pointed out in the earlier sections of this report there are even greater problems with happiness and life satisfaction data.

The final concern is that these indicators could be used to support policies that perversely reduce welfare and increase poverty, such as restrictions on human mobility. According to the sociologist David Bartram (2010, p.340) “some findings of happiness research can be used to derive the implication that migration might make some immigrants less happy than if they had stayed put.” The reason is that even though migration will likely cause immigrants’ absolute incomes to rise, their relative income position will fall as their reference group changes to include those in the destination country, and lower relative income leads to unhappiness (Bartram, 2011). Even some economists make such claims.

For example, Knight and Gunatilaka (2012) suggest rural-to-urban migration in China “may well have had the unexpected consequences of reducing subjective well-being” (p.108) because migrants’ aspirations rose faster than incomes, leading to frustration and unhappiness.

This is dangerous ground to tread, since for some countries, and some regions, migration is easily the most effective development policy available (Clemens, 2010). As an example of the efficacy of migration, four of every five Haitians who have escaped sufficiently far from extreme poverty to live above \$10 per day (in PPP terms) do so by living in the United States, while for the Indian-born population the comparable figure is 27 percent (Clemens and Pritchett, 2008). These policy conclusions about migration reducing happiness and subjective well-being rest on unreliable evidence, because survey questions on happiness and life satisfaction are treated as interval measures of cardinal data that can be compared over time and space.

In contrast to that assumed cardinality, these measures are not consistent when frames of reference change, as occurs with migration. Evidence for this point comes from Stillman et al (2015), who compare Tongan immigrants in New Zealand who are selected by random lottery with unsuccessful applicants into the same lottery who stay in Tonga. The immigrants moved to a country where average incomes are six times higher, and experienced at least three-fold increases in their own personal and household incomes, but their self-rated subjective wellbeing showed no change compared with the control group in Tonga. But when asked to compare their subjective wellbeing in New Zealand to their pre-migration wellbeing in Tonga immigrants report significant improvements; this corroborates the revealed preference of their move (and the lack of return migration to Tonga). Why are the self-reported changes and the cross-sectional comparisons so different? Because from the point of view of living in New Zealand, the immigrants retrospectively judge their former standard of living in Tonga lower than how it is judged by the control group in Tonga, whose frame of reference has not changed. These subjective measures from surveys of happiness and wellbeing are not cardinal data that allow consistent comparisons over time and space because there is no consistent measuring rod for respondents who answer such questions. Therefore, proposals for every country to adopt monitoring of such survey measures will not lead to any improvement in our ability to monitor progress in reducing extreme poverty. Despite the large and growing literature that uses such survey responses as data, these measures are a mirage and proposed post-2015 targets based on such measures should not be supported.

### **Limited Understanding of Complex Responses to Transfers**

The possible benefits, in terms of poverty reduction, from increasing social protection transfers are set out by proponents such as Clunies-Ross and Huq (2014). But unintended consequences of expansion mean that in some cases the ultimate beneficiaries are people who are not poor. In other cases, an expansion of social protection programs may do more harm than good in the aggregate because of a reduction in labor market activity. Overall, we know much less about the responses to transfer payments than we should, if expanded social protection is to be a target for post-2015 poverty goals. In this section, three types of unintended consequences are discussed: distortion of employment choices between informal and formal sectors; damage done to non-recipient households because of general

equilibrium effects; and, damage done to informal social safety nets so that over time the expanded social protection programs may ultimately benefit richer participants in these informal networks rather than the poorer participants who were the intended beneficiaries.

When some funding for social protection programs comes from payroll taxes or other contributions tied to workplaces, and where people outside of formal work or in low-paid work can get subsidized alternatives to the protection that employees self-contribute to, it can create costly distortions. An example comes from Colombia's public health insurance scheme; Camacho et al (2013) identify causal effects of the expansion of this social protection program from variation across municipalities in when each one began to conduct interviews to screen for eligibility. The results suggest that subsidized health insurance can be a substantial disincentive to formal employment, with an increase in informal employment of approximately four percentage points from expansion of the scheme.

Distortions to work choices are likely to be a typical, and costly, byproduct of expanded social protection systems and are not restricted to poor countries. Mulligan (2013) calculates implicit marginal tax rates of five percentage points (of employee compensation) due to subsidy provisions in the Affordable Care Act (ObamaCare) in the United States. This social protection intervention subsidizes health insurance premiums of low-income workers. The subsidies phase out as incomes rise and thus act as a tax on working more. The impact on the labor market of these higher marginal tax rates is substantial; the Congressional Budget Office predict that there will be a fall in the total hours worked in the United States of up to two percent, during the period from 2017 to 2024, due mostly to these effects of social protection subsidies acting to tax work more heavily (CBO, 2014). There also may be distortions to labor-leisure choices because an increase in transfers received raises reservation wages (due to higher non-labor income) and thereby leads to lower labor force participation. For example, Abel (2014) studies South Africa's Old Age Pension and finds that having pension recipients in the household adversely affects employment outcomes of prime-aged adults; gaining a pensioner more than doubles the probability that employed adults lose their job and almost halves the chance that unemployed adults find employment.

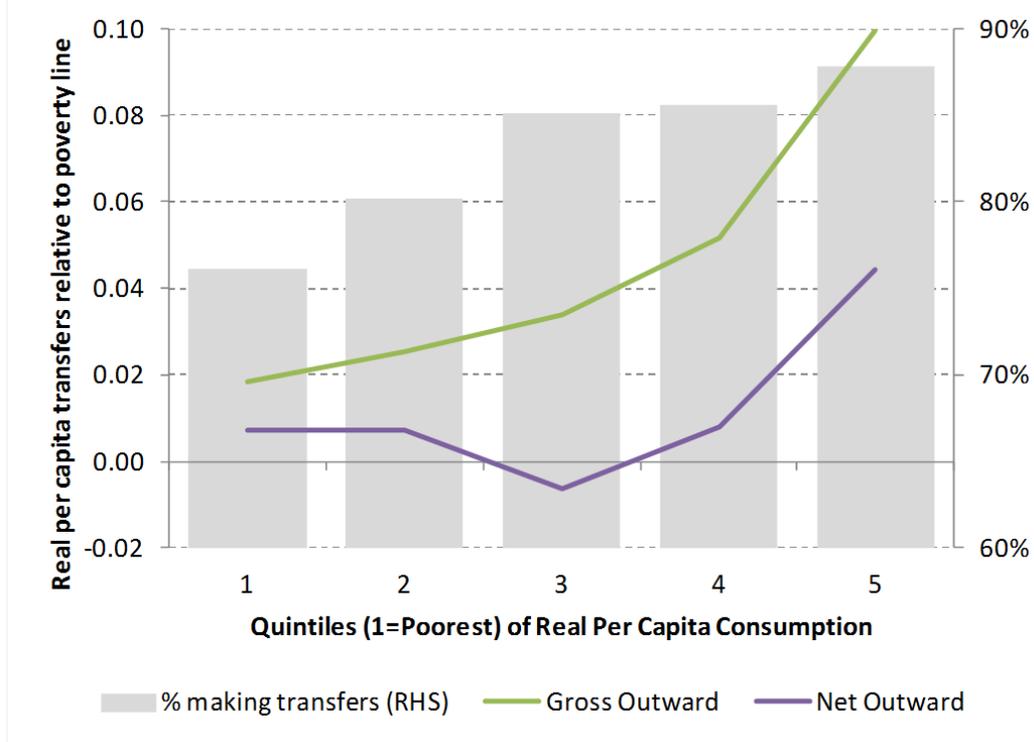
In addition to potential for social protection transfers to distort work choices, they also may impact other markets with unintended negative effects. One effect that may be particularly important in more isolated rural areas is on wage rates and the prices of goods imported into the area from the outside world. Lehmann (2013) uses a general equilibrium model to look at the impact of cash transfers made by the *Oportunidades* program in Mexico, and finds that these transfers generate local inflation which erodes the real incomes of non-targeted households. The mechanism for this negative effect is that *Oportunidades*-induced higher wages in a village will raise the price of imported items (since these goods need local labor to make them available from the outside world) and will lower the net returns to items exported from the village to the rest of the world. These effects are familiar to anyone who works on small, remittance-dependent economies, such as the Pacific Islands, where it has been argued that the inflow of cash remittances from abroad has the same stifling effects on returns to traditional activities as do Dutch Disease effects from a

booming non-traditional export sector in national economies (Faeamani, 1995). In isolated village economies, an influx of cash transfers with no improvement in conditions on the supply side of the local economy could create village-level Dutch Disease effects, to the detriment of many.

The final unintended consequence of expanded social protection transfers considered here is the impact on the existing informal safety net. There is ambiguous evidence on whether expanded formal transfers weaken existing informal safety nets. In Ghana the introduction of a formal health insurance scheme appears to have crowded out informal transfers, due to a reduced willingness to contribute to transfer networks (Klohn and Strupat, 2013). Conversely, in urban Papua New Guinea workers living in traditional villages in urban areas (and so they stay embedded in the social relationships that sustain informal solidarity networks) do not opt out even when they have access to formal sources of insurance and other types of social protection (Boe-Gibson et al, 1998). This rate of substitution between formal and informal systems is a parameter that proponents of expanded social protection transfers should highlight as a key unknown for assessing the welfare impacts of their proposals – if this substitution is high, creating a wider safety net through expanded formal social protection programs may simply crowd out existing informal transfers, meaning that the ultimate beneficiary is not the targeted recipient living in poverty but instead the people who previously transferred money, food and other resources to that recipient. Since the net donors to these informal safety nets tend to be better off individuals (and may live overseas in the case of remittance-dependent countries) an increase in formal social protection programs may not be pro-poor.

Evidence on the patterns of participation in informal transfer networks is provided in Figure 9, using data from the 2009/10 Household Income and Expenditure Survey for Papua New Guinea. The bars show the participation rate, which is the percentage of households in each quintile of real per capita consumption where a householder made transfers to people outside their household. The cost of basic needs poverty rate calculated from these data is almost exactly 40 percent, so the first two quintiles are the 'poor' and the upper three the non-poor. Even amongst the poor, more than three-quarters of households have members who were involved in making outwards transfers to others sometime in the previous 12 months. This pattern follows from the fact that these informal transfers are not based on notions of charity but instead are based on complex and reciprocal social obligations. This reciprocity means poorer people may be obligated to give a transfer to a richer person (e.g. because of past support from the richer person for brideprice payments). A popular hypothesis in the literature on voluntary transfers is that their motivation depends on where in the income distribution the recipient is located, with altruistic motivations dominating for transfers to poorer recipients and 'exchange' motivations more important for transfers to richer recipients (Cox et al, 2004). However the empirical evidence from four countries in Asia and the Pacific is not in favor of this hypothesis (Gibson et al, 2011). Thus it is probably unhelpful to think of the transfer activity summarized in Figure 9 as altruistically motivated and voluntary, and instead it may be better to view it as the outcome of much more complex social relationships, which may respond in unpredictable ways to new sources of cash in the form of transfers from social protection programs.

Figure 9: Participation in Informal Transfer Networks, Papua New Guinea 2009-10



The relative sizes of gross and net outward transfers are shown by the lines in Figure 9, where the unconditional average is calculated for each quintile. The monetary values include in-kind transfers, and are put into real per capita terms by dividing by the poverty line for each region and by the size of the household where the person making the transfer lives. Thus, the scale of gross outward transfers by each person in the richest quintile is equivalent to 10 percent of the value of the poverty line while those transfers made by the poorest quintile are equivalent to two percent of the poverty line. The net value of outward transfers is calculated by subtracting the value of inward transfers from the gross value of outward transfers.<sup>27</sup> The key feature to observe from Figure 9 is that the net value of transfers is roughly zero for all but the richest quintile. So while there is a lot of participation in these informal networks, there also is a lot of churning where people are making informal transfers amongst each other and the only group that, on balance, makes net donations into the system is the richest quintile. It is the response of this richest group to an expansion of transfers made by social protection programs that is crucial; if these people realize that they are paying taxes to fund social protection programs and also are paying informal ‘taxes’ in the form of net outward transfers to their friends and relatives, they may decide to opt out of the existing informal system and thus there could be no overall improvement in the position of poor people.

The final aspect of unintended consequences could be viewed more as a complication for researchers, but it points to the difficulties of targeting, which policy makers should bear in mind. Most analysis of the impacts of cash transfers from social protection programs

<sup>27</sup> The average net values are positive most likely due to the questionnaire asking about inward transfers from a single report per household whereas outward transfers were asked of each adult and so likely had better recall.

assumes that household composition remains fixed in the face of new inflows of cash. This is doubtful, and adherence to this assumption potentially undermines the identification of the ultimate beneficiaries and the calculation of by how much they benefit. For example, developing countries in the Pacific Islands have very fluid household structures, with a “floating” population of unattached people who may go and reside with *wantoks* or *aiga* for several months at a time (and cultural obligations prevent hosts from turning them away).<sup>28</sup> Since the extended family networks may spread over several locations, especially once spousal lineage and polygamy are accounted for, there is a lot of scope for the floating population to move, which makes spatial targeting difficult. For example, Gibson and McKenzie (2011) show how extended family structures and flexible land use rights allowed some people to resettle in response to spatially targeted development interventions in Tonga. But even for finely grained targeting, at the level of households or individuals, the receipt of cash transfers likely would cause a number of “guests” to arrive, to help consume the extra resources. This makes it hard to target, and also makes it hard to model impacts, since there is no guarantee that the stipulated recipient of the transfer is the ultimate beneficiary.

This issue of endogenous household size is investigated by Hamoudi and Thomas (2014) using a relatively generous cash transfer program, the South African Old Age Pension (OAP). The OAP lets researchers identify causal effects from exogenous increases in transfers to blacks when racial gaps in pension payments were removed in the early 1990s. The OAP provided each older black man (> 65 years) and woman (> 60 years) a guaranteed income of about \$120 per month at the time of the 2000 Household Income and Expenditure Survey, placing the recipient pensioners around the black median income. Prime age adults co-residing with the pensioners are negatively selected in terms of human capital characteristics, and are presumed to be people who have a comparative advantage in providing domestic labor, of which the newly richer pensioner can demand more. To provide this domestic labour these low human capital adults co-reside with the pensioner, and this endogeneity of household size complicates the analysis of exactly who are the recipients over which impacts should be measured.

## **METHODS OF VALUING BENEFITS OF REDUCED POVERTY**

A review of literature shows that the question: “how much does poverty cost?” is not one that is commonly asked, or answered, by economists. Consequently, existing benefit-cost ratios are not easily obtained from the literature, for any of the poverty, social protection, disaster resilience, economic rights, and full employment targets that are to be evaluated. Amongst these five types of targets, priority is given here to the poverty target. Therefore, in this section two potential methods of valuing the benefits of reduced poverty are considered. There are likely to be other methods that could be used so the treatment here is by no means exhaustive.

### ***Consumption Insurance Effects***

A target of eliminating extreme poverty can be thought of as providing a floor for

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<sup>28</sup> For example, the correlation between two reports on the size of the “same” household approximately six months apart was just 0.65 for urban Papua New Guinea (Gibson, 2001) because of short-term movements into and out of households by the floating population.

consumption. Such a floor has both a level effect and a variance effect. For anyone whose mean consumption is below the extreme poverty line, lifting their mean to the poverty line is valuable to them (and perhaps has wider benefits, as considered below). The value to place on this level benefit could be calculated as the discounted present value of all of the poverty gaps. However, the existence of a floor to consumption levels that is set at the level of the extreme poverty line (or set anywhere that it provides a floor) also provides value to the non-poor, in the form of insurance benefits. These benefits would need to be incorporated into any calculations of the value of eliminating poverty.

An example of the how to calculate the value of certainty due to insurance comes from Morduch (1995) who uses longitudinal survey data from households in India (the ICRISAT Village Level Studies that were based on two villages from Andhra Pradesh and four villages from Maharashtra). In order to calculate the share of income that low-income households might be willing to give up to insure against fluctuating welfare levels Morduch needed evidence on the volatility of household income, from the ICRISAT panel, and also needed to make an assumption about the degree of risk aversion (specifically, that the coefficient of relative risk aversion is two). The basis of this calculation is that uncertainty reduces the utility of anyone who is risk averse, and so they would be willing to give up some fraction of their actual, fluctuating income, to have a lower mean income with certainty. The mathematical basis of the calculation is that for any concave utility function (the assumed coefficient of risk aversion is related to the degree of concavity), the expected utility from uncertain income is equal to the utility from a certainty income, where that certainty income is less than the mean of the fluctuating income. The calculation by Morduch suggested that these Indian households would be willing to give up 16 percent of their total income in order to have a certainty income. On the basis of such calculations applied more widely, one could put a value on the floor that would be effectively provided by eliminating extreme poverty (or alternatively, having universal social protection with a floor set at the extreme poverty line). However, the calculated benefits would rely heavily on the assumed degree of risk aversion and also require longitudinal data so that the coefficient of variation can be calculated for a much wider range of settings than in Morduch's example.

### ***Mortality and Human Capital Effects***

Poor people live shorter, less economically productive, lives. Thus, one way to calculate the cost of poverty (or equivalently, the value of poverty reduction) is from the change in the value of a country's human capital stock. If there is less poverty then fewer potential workers either die early or have less productive work lives. The advantage of this method is that it can rely on an existing lifetime labor income approach of Jorgenson and Fraumeni (1989, 1992a, 1992b) that measures the value of the aggregate stock of human capital that is embodied in individuals. The data requirements for this method are not excessive. It is based on the discounted present value of expected future labor incomes generated over the working life of the people currently living. Thus, a time series of cross-sectional labor force surveys (or censuses), life tables to measure survival probabilities, and assumptions about future discount rates and growth rates in income are all that is required by the existing Jorgenson-Fraumeni approach.

This approach has been used to value human capital in several developed countries, including Australia (Wei, 2004), Canada (Gu and Wong, 2010), New Zealand (Le, Gibson and Oxley, 2006) and the United States (Christian, 2010). A cross-country comparison of human capital for 16 OECD countries valued according to this approach is provided by Liu (2013), who also discusses other methods of measuring human capital, such as cost-based measures. This OECD study shows that the stock of human capital is worth about five times the value of produced capital, and from eight to 16 times the value of GDP. The lifetime income approach has also been applied to the very long run history of Britain, producing human capital estimates from 1760 to 2009 (Kunnas *et al*, 2013). The conditions in Britain in the 18<sup>th</sup> century were considerably worse than in today's developing countries, with average life expectancy below 40 years (by way of comparison, for a developing country like Vietnam, life expectancy was already over 70 years at baseline for the MDGs in 1990). Moreover, the economic structure of 18<sup>th</sup> Century Britain was not unlike today's poor countries, with more than 70 percent of the population located in the countryside in 1800 and about three-quarters of the rural population involved in agriculture (Allen, 2000).

Therefore, despite various data issues that are discussed below, there is no inherent reason why the lifetime labor income approach to measuring human capital cannot be applied to the developing countries of today. Note however that there are no existing estimates in the literature that apply this method to developing countries. Thus, it is necessary to go beyond the terms of reference of the Copenhagen Consensus, where reports are simply meant to review existing estimates in the literature in order to form benefit-cost ratios. Since there is only limited time to undertake new research, this lifetime labor income approach will be applied to just a single developing country (Vietnam), which is chosen because there are credible estimates of the mortality due to poverty available for this country. This country also has high quality and high frequency labor market survey data. Hence, the estimated benefits from these calculations will be more in the form of an exemplar of what could be done elsewhere rather than the sort of compilation of worldwide evidence that might have been expected.

It is straightforward under the Jorgenson-Fraumeni approach to estimate the present value of lifetime labor income from a cross-section. For a person aged  $a$  years with a certain level of education, their expected earnings  $n$  years in the future are assumed to be the current earnings of people of the same education and gender who are  $a + n$  years old, adjusted for an expected rate of growth in real incomes. The calculations are made easier by the fact that the present value of lifetime labor income for an individual is just their current annual labor income plus the present value of their expected lifetime income in the next period (where this expectation also takes account of survival probabilities). The expected lifetime income term also incorporates the risk of being unemployed at each age. Thus, by backwards recursion from an assumed retirement age of 65 it is possible to calculate the present value of lifetime income at each age, and to aggregate across cohorts so as to produce a national total value of human capital at a given point in time. The required formula is:

$$H_a^t = W_a^t Y_a^t + S_{a,a+1}^t H_{a+1}^t (1 + g)/(1 + i)$$

- $H_a^t$  = per capita human capital; the present value of life-time labor income, of individuals aged  $a$  at time  $t$ ;
- $W$  = employment rate;
- $Y$  = per capita current annual labor income of employed individuals;
- $S_{a,a+1}$  = probability of surviving one more year from age  $a$ ;
- $g$  = annual growth rate in real income;
- $i$  = discount rate.

In order to adapt this method to measuring human capital effects of poverty reduction, it is necessary to add an additional stratifying variable for defining cohorts – poverty status. Thus, separately for poor and non-poor, males and females, with different levels of educational qualifications, age-earnings profiles are estimated from the Vietnam Household Living Standards Survey (VHLSS) for every two years from 2002-2012. The effects of reduced poverty can be incorporated into this human capital calculation from an increase in survival probabilities as people escape poverty, which is considered here to be a causal effect (see below), and also from a switch in the age-earnings profiles as people graduate out of poverty. Obviously, the future shape of the labor income profile also depends on the assumed rate of income growth, and most likely it is income growth that triggers much of the poverty reduction. So whether it is growth rather than poverty reduction that should be considered as the main driver of the rising value of human capital is an open question that is not dwelt on here. Notwithstanding that interpretation issue, in order to implement this method, one requires credible estimates of the effects of poverty on mortality, and these are discussed below.

### **Effects of Poverty on Mortality**

A large literature examines links between mortality and socio-economic status, with evidence at both the country level (Preston, 1975) and within countries (Rodgers, 1979). Mostly, this literature considers the effect of income inequality on population health (Wilkinson, 1992) although the relationships are unlikely to be causal and instead reflect confounding effects of education and race (Deaton, 2002a). A smaller literature examines effects of poverty on mortality, with own-poverty and area-level poverty both being potential risk factors for individuals. It is likely that poverty kills many more people than inequality. For example, Galea *et al.* (2011) attribute 172,000 deaths in the United States in 2000 to poverty (39,000 from area-level poverty and 133,000 from individual-level poverty) and just 119,000 deaths to inequality.

The smaller literature on poverty-mortality links may reflect stricter data requirements since one needs more than just area-level data, which is sufficient for estimating links between inequality and mortality. At the very least, accurate measures of individual socio-economic status are needed, and such details are rarely recorded on death certificates. Ideally, longitudinal data are needed to help rule out reverse causation effects where it is ill-health that pushes people into poverty and early death, rather than poverty causing early death. The longitudinal evidence from developed countries is that the higher death rate of the poor is not an artifact of ill health causing people to drift down the social ladder (Fox, Goldblatt and Jones, 1985). The effects of poverty on mortality appear to be large. For

example, a longitudinal study in the United States (Hahn *et al.*, 1995) found that for males aged 25-44 [45-64] the relative hazard of death for the poor was 3.0 [2.2] times that of the non-poor. Adjusting for a rich set of potential confounders slightly lowered the relative hazard, to 2.9 [1.6]. Females had a smaller poverty penalty, which was not age-dependent, with an unadjusted relative hazard rate due to poverty of 2.0 and a confounder-adjusted hazard rate of 1.2.

Less is known about effects of poverty on mortality in developing countries, where there are fewer long-term longitudinal studies. The main results are from Banerjee and Duflo (2010) who use nationally representative longitudinal data for Vietnam and data with almost national coverage for Indonesia. Mortality rates of the non-poor are compared with those for individuals in extreme poverty (below \$1 a day in PPP terms) at baseline. In rural Vietnam, 15% of the extremely poor who were above age 50 at baseline (in 1992/93) had died five years later but only 5% of those in the same age range who had been living on \$6-\$10 per day at baseline had died. For prime-age adults (aged 15-45), the mortality rate of the extremely poor and the very poor (below PPP\$2/day at baseline) was 1%, for those on \$2-\$4 per day it was just 0.6% and none of the \$6-\$10 per day group had died. In rural Indonesia the poverty-mortality gradient was even steeper amongst prime-age adults; 5.6% of the extremely poor and 5.3% of the \$1-\$2 per day group at baseline (in 1993) were dead by the time of the follow-up survey in 2000, but the death rates were just one percent for those living above \$2 per day at baseline.

The advantage for the current analysis of using the mortality estimates from Vietnam is that the VHLSS data were readily to hand, and a considerable amount is already known about the trend poverty decline in that country (Figure 5). Obviously Indonesia would be a good candidate country for the method to be applied to next. Since both of these countries are relatively data-rich by the standards of developing countries it is an open question as to how easily this method might be applied elsewhere, and especially to countries in Africa where the survey evidence base is much weaker and where causal estimates of the effects of poverty on mortality may not be available.

Since the Banerjee and Duflo (2010) mortality estimates for Vietnam are for a five year period, they have to be transformed to calculate mortality rates per year. Also, two non-poor groups are defined in the original study; \$2-\$4 per day and \$6-\$10 per day, and to err on the side of caution it is the \$2-\$4 per day group whose mortality rate is used in the calculations as the estimate of the mortality risk for the non-poor. In other words, what will be modeled is escaping extreme poverty but not the potentially unrealistic case of leaping from below \$1 a day all the way into the \$6-\$10 per day income group. Also, the mortality rates are not disaggregated by gender, which would be a further refinement that future studies could make. The mortality rates and survival rates used are in Table 1.

*Table 1: Mortality Rates and Survival Rates Used in Calculating the Monetary Benefits of Poverty Reduction in Vietnam in Terms of Human Capital*

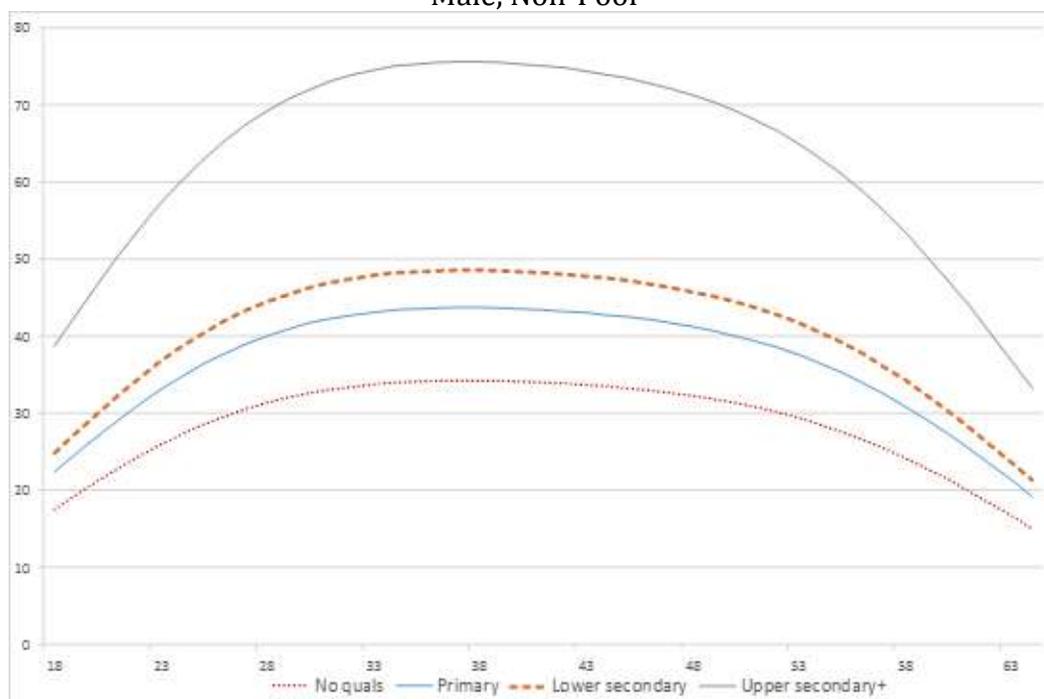
Age Group	Five-year Mortality Rate (%)		Annual Survival Rates (%)	
	Poor	Non-Poor	Poor	Non-Poor
15 – 45	1.0	0.8	99.799	99.840
45 and up	12.0	9.6	97.476	98.002

Source: Derived from Banerjee and Duflo (2010) Table 7.7

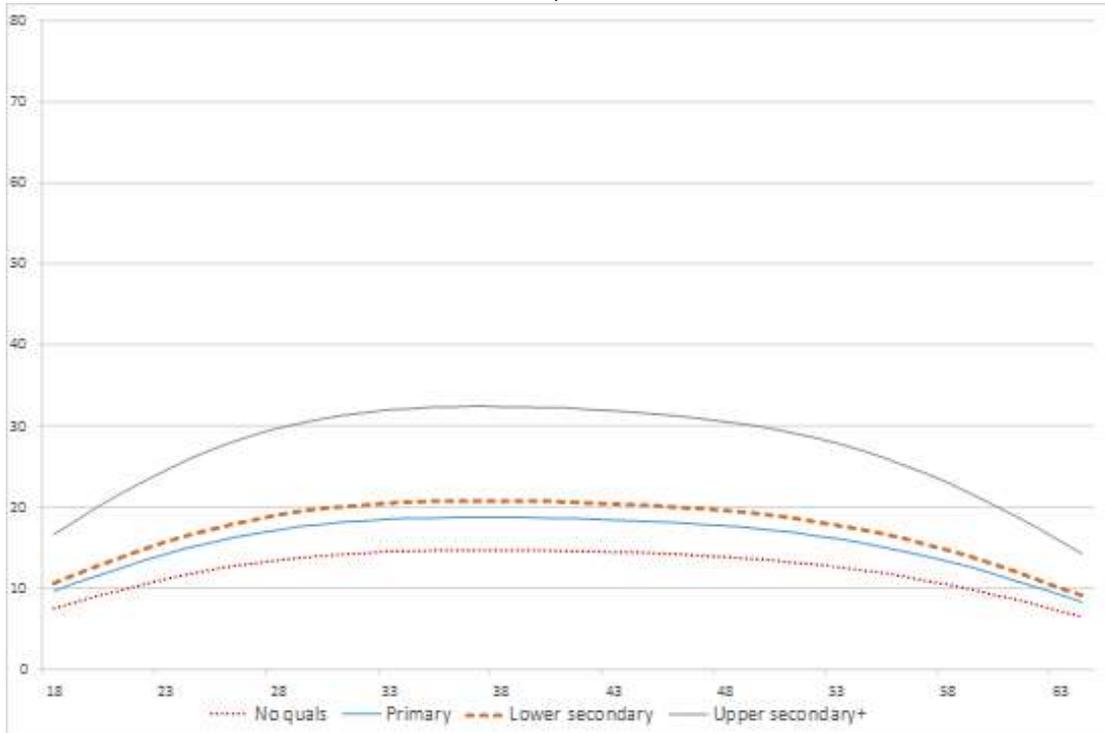
### Other Modeling Assumptions

The other assumptions made are that the annual (real) discount rate is 0.03 and the real income growth rate is 0.05. The estimated age-earnings profiles for the gender and poverty groups are shown in Figure 10, where for each group there are profiles for cohorts defined by four levels of completed education: no qualifications, primary, junior secondary, and senior secondary and above. These profiles are estimated for paid employees, and are assumed to hold across the relevant working age population of each cohort, in the same manner that Mincerian returns to schooling are estimated for those in employment but used to evaluate the returns to schooling investments for the entire cohort who were schooled, rather than just those who were schooled and who are employed. The results are all reported in terms of December 2012 Dong, based on using the monthly CPI for Vietnam to put the VHLSS data from previous years into real terms (and to account for inflation within each year that the VHLSS was fielded).

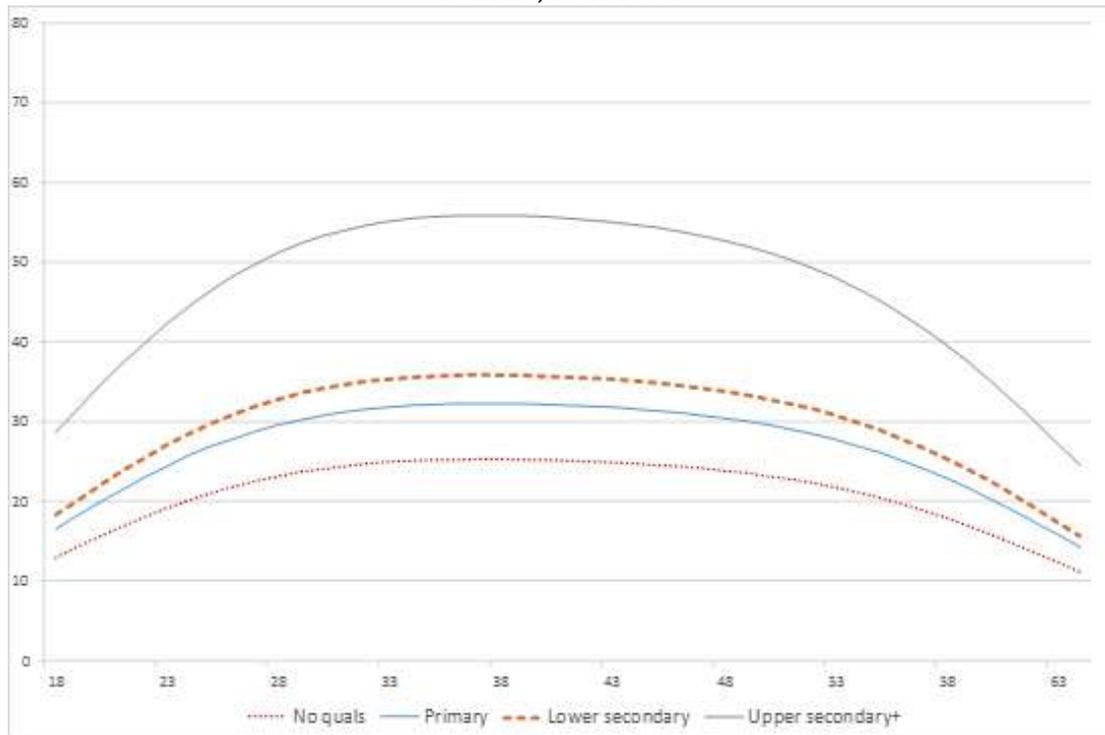
*Figure 10: Age-Earnings Profiles (million VN dong, in December 2012 prices)  
Male, Non-Poor*



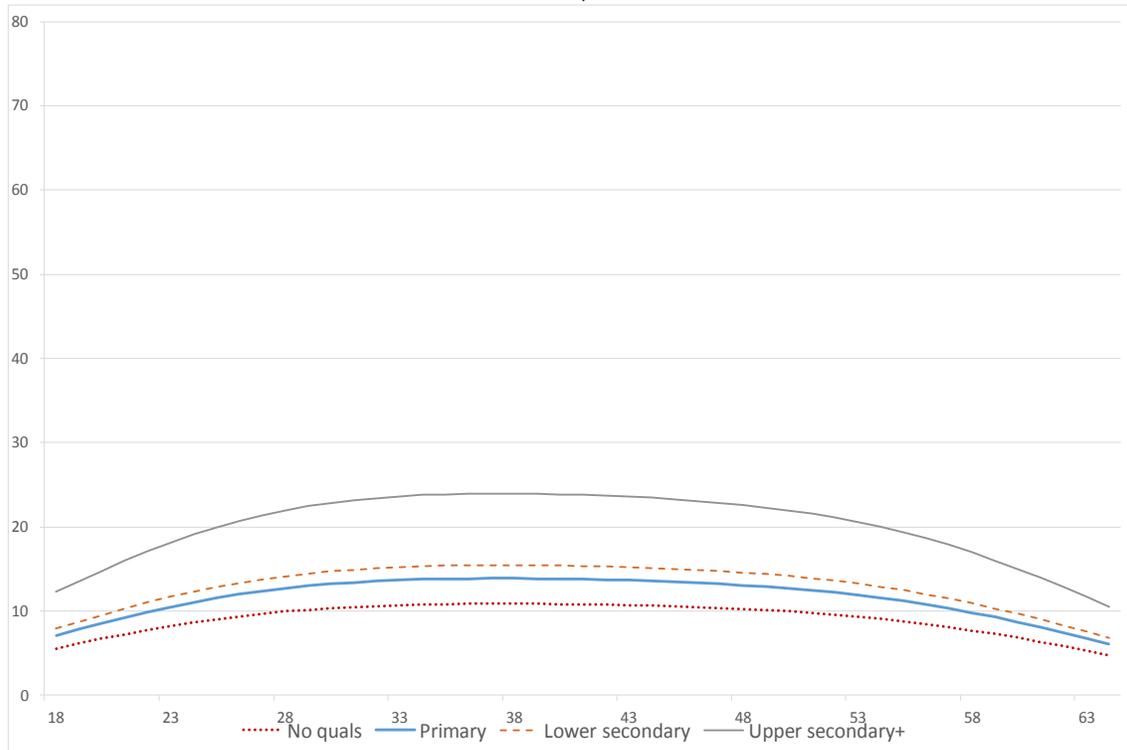
### Male, Poor



### Female, Non-Poor



Female, Poor



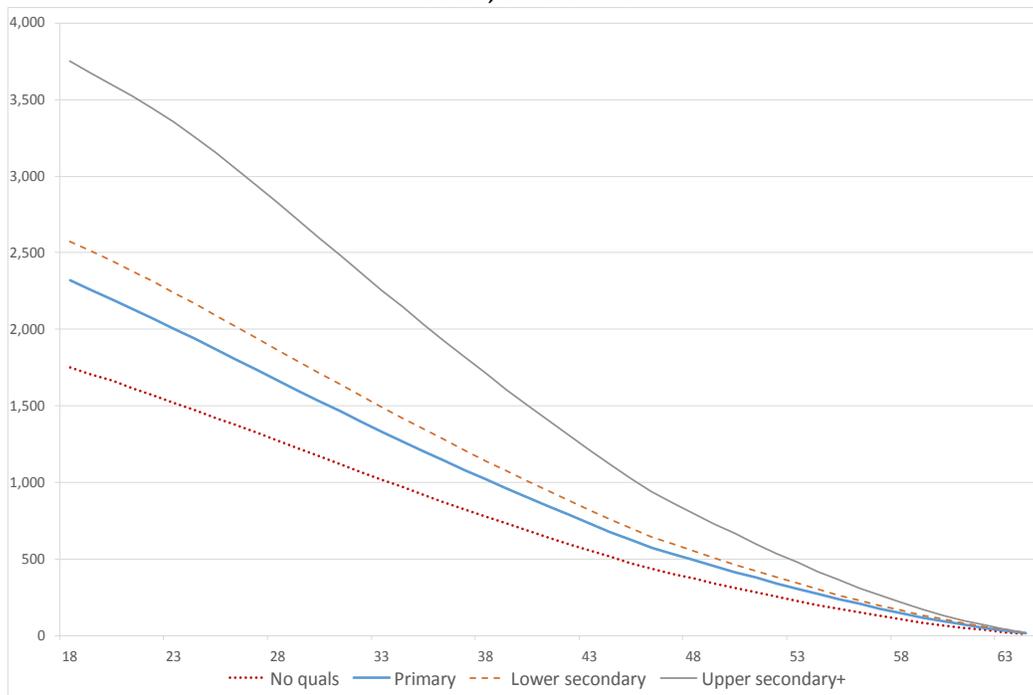
## COST-BENEFIT EVALUATION OF PROPOSED TARGETS

There are five types of targets to be considered, based on the discussion in Section II: “poverty”; “social protection”; “disaster resilience”; “economic rights”; and, “full employment”. Of these, greatest attention is paid to the poverty target. A revealed preference argument suggests that if these other targets were related to activities that had higher benefit-cost ratios than the benefit-cost ratios from achieving the poverty target, than these other targets would already have been included in the MDGs. Since only the poverty target was included in the MDGs, with the other targets not making the cut, the logical conclusion is that activities related to these other targets must offer lower benefits relative to costs. In fact, this logical approach to assessing targets raises a broader question that can be directed at all of the proposals made under the guise of the post-2015 development agenda; if the benefit-cost ratios truly are as high as might be suggested in some quarters (for example, the Copenhagen Consensus has an evaluation category for benefits that are claimed to be more than 15 times higher than the cost), why aren’t people already taking actions to capture these phenomenal benefits. Absent credible evidence of market failure and collective action problems, the presumption should be that the optimal set of interventions are already being made.

## Poverty<sup>29</sup>

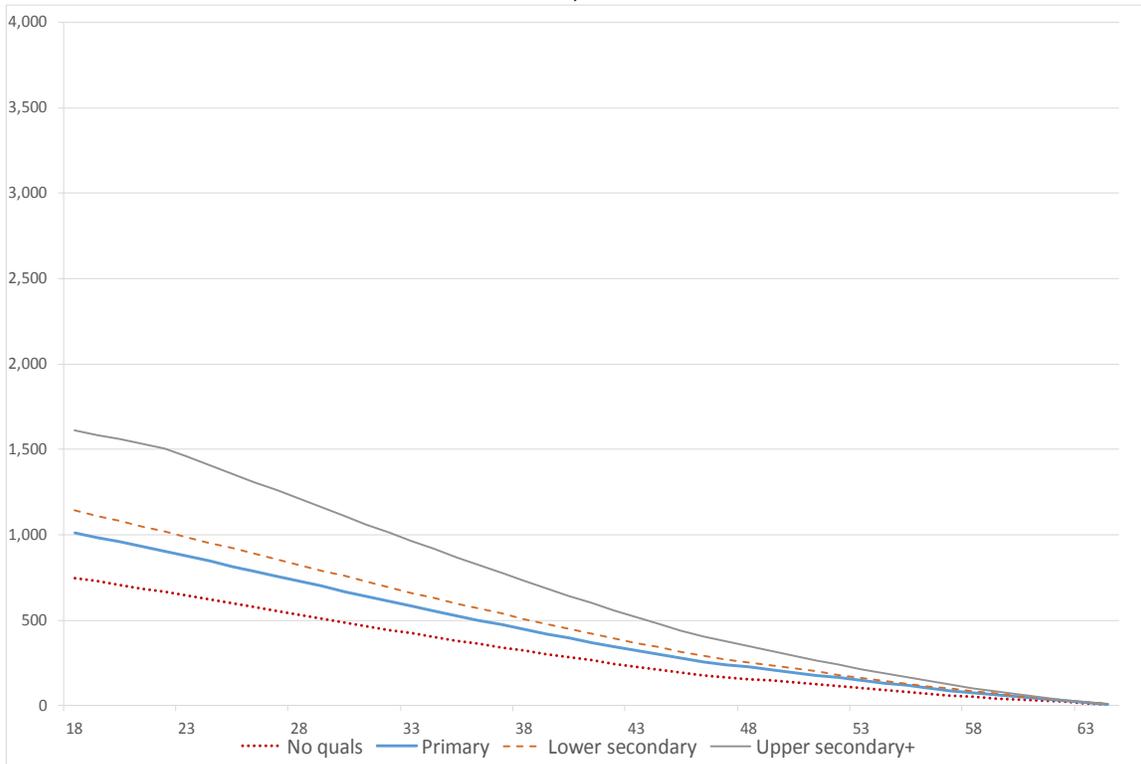
The estimated benefits of poverty reduction are based on calculated aggregate human capital for Vietnam over 2002-2012. As noted above, this is just an exemplar of what might be calculated in other countries, so it involves a great leap of faith to extrapolate from this one case to elsewhere. Figure 11 shows the estimates of lifetime labor income for survey respondents aged 18-65, for four educational qualification groups, within cohorts defined by gender, and poverty status. These figures tell us the economic value of a man or a woman of a particular age and a particular type – in other words, you are the capitalized value of what you are expected to earn between now and retirement, after allowing for risks of unemployment and death. Thus, young, well-educated, non-poor, males are worth over 3.5 billion dong (US\$170,000) in the labor market while young, poor, unqualified, males are worth less than 1 billion dong. Old people are always worth less since they have fewer years remaining of expected earnings to be capitalized. Adding up these estimates across all ages and cohorts, and weighting by the (changing) size of each group in Vietnam, gives the aggregate value of human capital in each year.

*Figure 11: Lifetime Labor Income By Gender and Poverty Status, 2012 (million VN dong, in December 2012 prices)*  
Male, Non-Poor

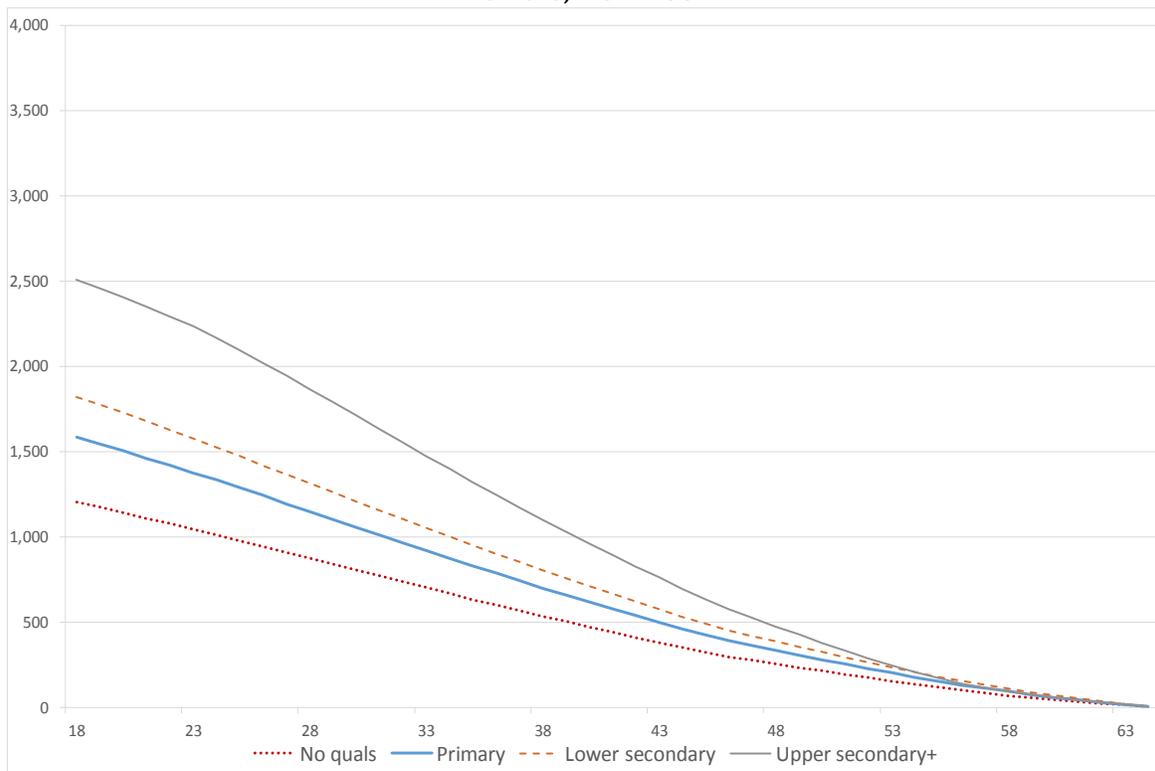


<sup>29</sup> The human capital estimates in this section for Vietnam (including the simulated effects of eradicating poverty, and of faster growth) are from joint work with Dr Trinh Le. She is not responsible for any interpretations placed upon those results here. The comparisons of monetary benefits and monetary costs of eliminating poverty are not the work of Dr Le and she should not be deemed responsible in any way for those.

### Male, Poor



### Female, Non-Poor



## Female, Poor

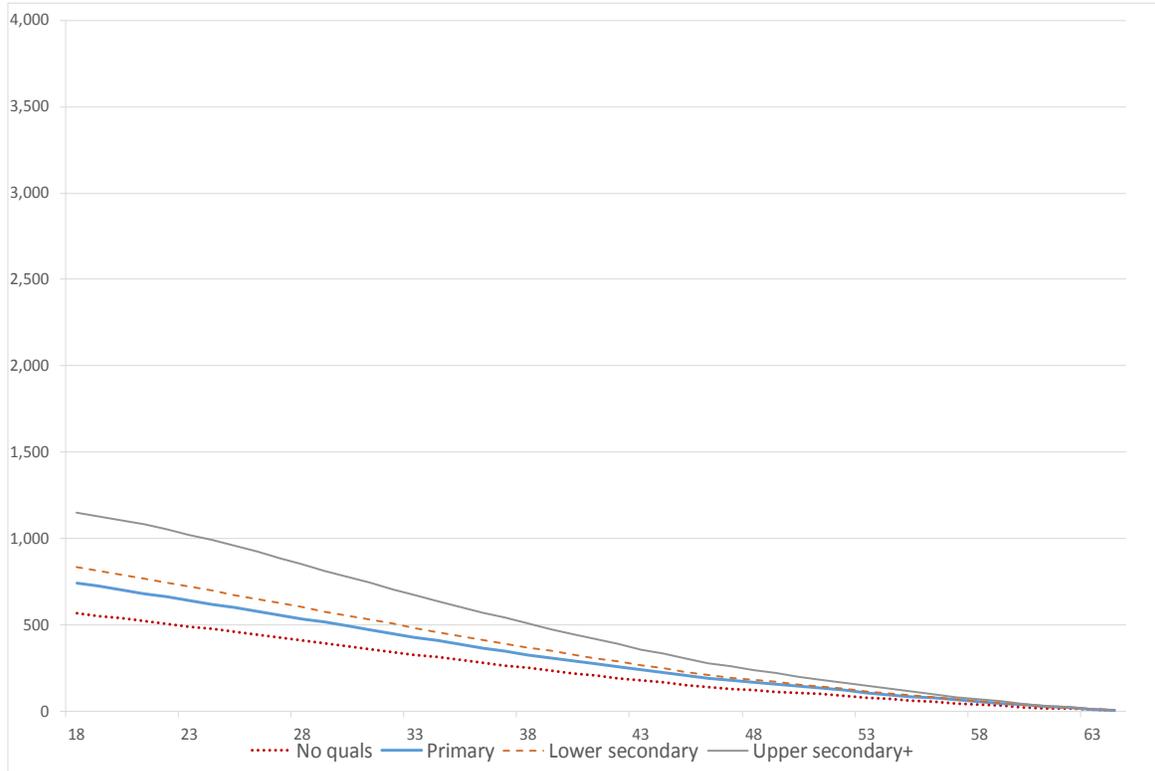
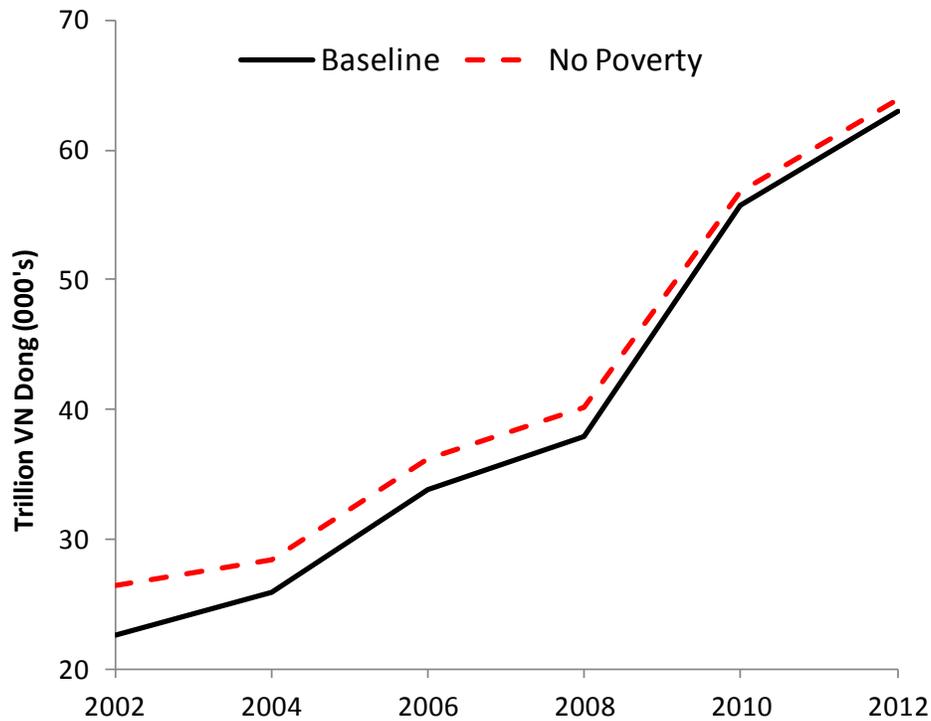


Figure 12 shows what happened to the real value of Vietnam's human capital, as measured by this approach, over the decade from 2002. At baseline, human capital was worth 22,667 trillion dong and by 2012 this had increased to 63,049 trillion (all in December 2012 prices). This increase of 40,382 trillion dong was equivalent to approximately two trillion US dollars at market exchange rates. A decomposition of this decadal increase in human capital indicates that just less than one-half of it was due to labor market improvements, in terms of higher real wages and employment rates. These labor market improvements can broadly be considered a direct result of the economic growth and structural change that Vietnam benefited from during this period. The rise in the working age population accounted for about 18 percent of the human capital change, poverty reduction accounted for 8.1 percent, and educational improvements (in terms of qualifications – the returns to school quality may be different) accounted for a very minor share (1%) of the change. The decomposition also indicates that about one-quarter of the total increase in the value of human capital is due to interactions between these various factors.

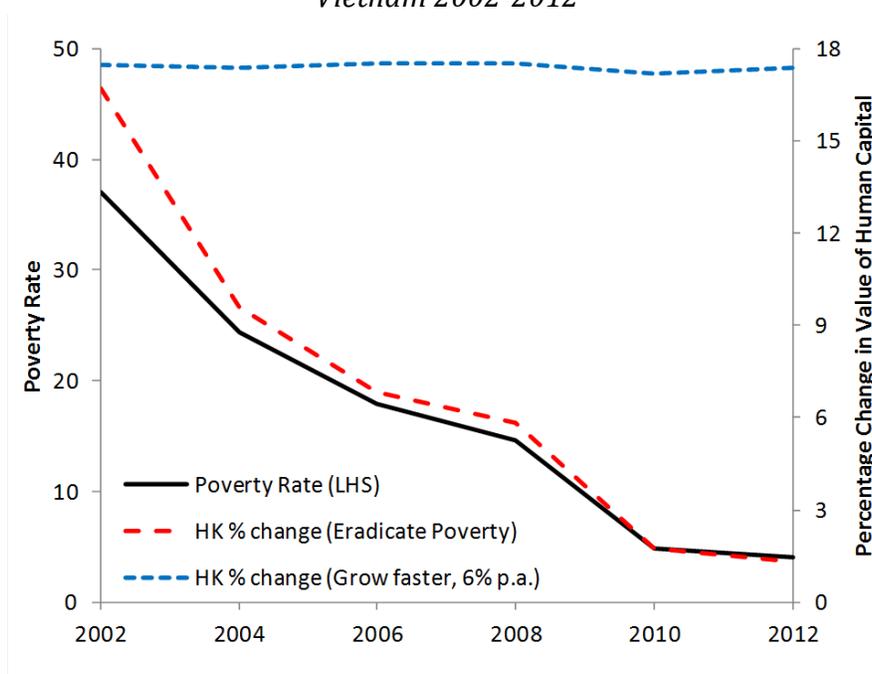
Figure 12: The Growth in Aggregate Human Capital, Vietnam 2002-2012



In each of the years with VHLSS data available, the estimate of the baseline value of human capital is compared with a simulated value, where the simulation is based on the assumption that extreme poverty has been eradicated. The time series for this hypothetical situation is also shown in Figure 12, using a dashed line. The 'no poverty' value of aggregate human capital rises from 26,450 trillion dong in 2002 to 63,883 trillion dong in 2012. It is apparent that the gap between the baseline value of human capital and the value obtained from the no-poverty simulation gets proportionately smaller over time.

It should be no surprise that the marginal value of eliminating poverty falls over time, since this coincides with a falling rate of extreme poverty in Vietnam. The percentage change in the value of human capital relative to the baseline estimate in each year is shown in Figure 13, using the red dashed line. In 2002, eradicating extreme poverty would have raised the aggregate value of human capital in Vietnam by 16.7 percent. In contrast, by 2012 the elimination of extreme poverty would raise the value of human capital by just 1.3 percent. This diminishing effect closely follows the declining percentage of the population who are in extreme poverty (based on a standard of \$1 a day consumption in PPP terms, with the time series for the head count poverty rate shown by the solid black line in the figure, using the scale on the left hand axis).

Figure 13: Effects on Value of Human Capital of Poverty Eradication and Faster Growth, Vietnam 2002-2012



Another way of considering these diminishing returns to eliminating extreme poverty is by comparing with the percentage change in human capital that would result from faster growth. Therefore, the other simulation that is shown in Figure 13 is for the annual growth rate of income to be six percent rather than the five percent rate used in the baseline estimates. This faster rate of income growth would raise the value of human capital by about 18 percent, and there is no apparent decline over time in this effect. In contrast to this constancy of the proportionate human capital payoff to faster income growth, the evidence is that poverty becomes less sensitive to economic growth over time, as the poverty rate falls (Figure 5). Thus, based on the pattern in Figure 5, policy makers might be increasingly tempted to abandon growth as a lever for reducing poverty since the relative effect of growth is falling at the same time that the inequality-sensitivity of poverty is rising. Yet the results in Figure 13 suggest that turning away from growth (perhaps by emphasizing redistribution) would do considerable damage to the value of human capital. While causing this damage by opting for slower growth, it would be providing little in the way of aggregate human capital benefits (but may be providing distributional benefits) because the extreme poverty rate is already so low that there is only a small loss of human capital due to poverty.

How do these human capital benefits of poverty reduction compare with the costs of eradicating extreme poverty? This question is hard to answer because estimates of how much it would cost to eradicate extreme poverty are generally not published. It is true that there are estimates of total budgetary outlays for particular anti-poverty projects (e.g., see Chen, Mu and Ravallion, 2008) but these fall well short of what is needed. First, the projects that have been evaluated never achieve 100 percent elimination of poverty, so one would need to extrapolate from the project budget and the achieved amount of poverty reduction to calculate what might be the cost of totally eliminating poverty. Moreover, it is likely that

the historic average cost from the existing project is not a good guide to the prospective marginal cost. Second, budgeted funds are fungible and there are also likely to be other interventions in place that affect the poverty status of a studied population, so with this scope for substitution it is difficult to draw a direct link between a certain level of spending and a certain amount of poverty reduction.

In light of these constraints on what is available from the literature, some simple, back-of-envelope, calculations are made about possible costs of eradicating poverty. As noted above, in the discussion of the calculations made by Clunies-Ross and Huq (2014), the sum of poverty gaps is not a very plausible estimate of the benefit of poverty reduction because it is tautological. But the sum of poverty gaps could be considered as a lower bound to the cost of implementing one way to eliminate extreme poverty, by means of perfectly targeted transfers. These poverty gaps are measured from the VHLSS data, using a poverty line of \$1 a day consumption, in PPP terms. The present value of perpetual transfers at the level needed to close the poverty gaps are calculated, using a discount rate of 0.03 to match what is used in the human capital calculations. The calculated costs are presented in the first two columns of Table 2.

*Table 2: Comparison of Monetary Costs and Benefits of Eradicating Extreme Poverty in Vietnam (With Benefits Measured in Terms of Human Capital)*

Year	Hypothetical Costs of Eliminating Extreme Poverty With Transfers		Benefits of Eliminating Extreme Poverty and Benefit-Cost Ratio			
	Sum of the poverty gaps (trillion dong)	Present value of perpetual transfers (trillion dong)	Human Capital (trillion dong)			Benefit to Cost Ratio*
			No	Baseline	Poverty	
2002	18.2	606.4	22667	26450	3783	6.2
2004	13.6	452.5	25953	28444	2491	5.5
2006	10.9	364.8	33857	36168	2311	6.3
2008	11.5	383.2	37975	40180	2205	5.8
2010	3.4	112.6	55772	56748	976	8.7

*Source:* Author's calculations derived from joint unpublished research with Dr Trinh Le.

\* This Benefit-to-cost ratio assumes that transfers are perfectly targeted, have no administrative, informational and incentive cost, and also that the marginal cost of public funds needed for making the transfers is zero. These are clearly unrealistic assumptions.

The estimates of the benefits of eradicating extreme poverty are based on the gap between the two time series in Figure 12. Since these estimates are for capitalized values that have already taken account of future income streams, no further discounting is needed. Therefore a comparison of the change in human capital – the marginal benefit of eradicating extreme poverty – and the cost of perpetual transfers should yield a valid benefit to cost ratio. The figures in the last column of Table 2 suggest that this ratio ranges from 6 to 9, although further considerations need to be taken into account before drawing inferences from this.<sup>30</sup>

<sup>30</sup> The Copenhagen Consensus Center requires that analyses are conducted using discount rates of both 3% and 5% to test the sensitivity of results to the choice of discount rate. If 5% were used, the simulated increase in the value of aggregate human capital due to eradicating extreme poverty would be less, but so too would be the cost of perpetual transfers to

The first consideration is that transfers are not costless.<sup>31</sup> A large literature estimates what is known as the “marginal cost of funds” – the fact that to raise a dollar of tax revenue (in order to make an anti-poverty transfer or to fund any public spending) it costs the economy more than a dollar because of the deadweight losses caused by the tax system (Allgood and Snow, 1998). These costs will tend to vary with the tax mix, since some forms of taxation are less efficient than others (Beaud, 2011). The costs will also vary with the importance of the informal sector (Warlters and Auriol, 2005) because if firms and households can more easily avoid the tax by moving into the informal sector then the deadweight costs on the remaining taxed agents tend to be higher. In a simulation analysis for 38 African countries, Warlters and Auriol (2005) suggest that the marginal cost of public funds ranges from 1.1 to 1.3.

In addition to the marginal cost of funds, one should allow for the administrative costs of making transfers. There is also the likelihood of targeting errors, and any informational cost of targeting. Putting these all together, at a minimum the estimates of the present value of perpetual transfers in Table 2 should be raised by at least 50 percent. After making this adjustment, the benefit to cost ratios would be in the range of 4 to 6 rather than the 6 to 9 range as shown in the table. Moreover, Vietnam has one of the better statistical systems amongst developing countries, and has had one of the most successful records of economic growth and poverty reduction in the last two decades. Therefore, any extrapolating from these estimates to other developing countries should add some discount for the fact that most of those other countries are unlikely to be as successful as Vietnam and so the benefit to cost ratio elsewhere may be even lower.

In terms of the assessment categories used by the Copenhagen Consensus, after a possible discount due to the aforementioned factors, benefit to cost ratios that were in the range of 4 to 6 would, once discounted, be placed squarely in the “Fair” category (benefits from 1 to 5 times higher than costs). It can thus be said that the target of eliminating extreme poverty does not have “Phenomenal” or “Good” benefits relative to costs, but nor are the benefits necessarily less than the costs (the so-called “Poor” category). It should also be noted that, despite resorting to new empirical evidence here, since the literature does not have what is required, these estimates of the benefits and costs remain fundamentally uncertain.

A final comment related to poverty targets concerns the Open Working Group’s second proposal about poverty: “reduce the proportion of people living below national poverty lines by 2030”. In order for a target about nationally defined poverty to make sense, as something that is enshrined in an international agreement, there must be greater effort

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enable this poverty eradication. Overall, the benefit to cost ratios would be higher than using a 3% discount rate, by 13% on average, with the biggest difference in 2002 when poverty rates were highest (a benefit-cost ratio of 7.3 rather than 6.2 as shown in Table 2). Full details of the calculated costs and benefits using a 5% discount rate are reported in Appendix Table 1.

<sup>31</sup> Often in welfare economics, transfers are not counted as costs (although rent-seeking arguments suggest that they should be, as first noted by Tullock (1967). Specific evidence of corruption, which is a type of rent-seeking loss, in a developing country redistribution program is provided by Olken (2008)). But in this case it is not just the deadweight loss of the transfer that has to be counted as a cost, so too is the transfer itself part of the costs. The reason is that the transfer lifts someone out of poverty, and they are being attributed a higher value of human capital from being out of poverty, so if the transfer was not counted as a cost then there would be benefit that had occurred seemingly without cost, which would bias the calculations.

made across countries and over time to harmonize methods of measuring living standards and defining poverty. There will inevitably be a temptation to compare performance in reducing nationally defined poverty, and possibly even to add up national poverty counts to form global totals (Klasen, 2013). Such an exercise will be adding apples and oranges unless more efforts at harmonization of poverty statistics are made. This is an area where the United Nations could have made a contribution but failed (see Section III).

### ***Social Protection***

The High Level Panel suggested a target of “cover x% of people who are poor and vulnerable with social protection systems” and the Open Working Group suggests that the social protection should include floors. The discussion above has already covered the uncertainty about who are the ultimate beneficiaries of expanded social protection (crowding out and endogenous household composition effects), the unintended consequences for employment choice, general equilibrium effects, and the interactions with the informal safety net. Despite ignorance of these issues, there is enthusiasm for expanded social protection programs, spurred in part by the careful randomized evaluations of conditional cash transfer (CCT) programs and unconditional variants (e.g. Baird et al, 2011; Robertson et al, 2013). However, policy has often preceded evidence; Cecchini (2014) describes the expansion of CCTs in Latin America, which have gone from covering six percent of the population in 2000 to 21 percent by 2012. Many of the published evaluations have come during this expansion rather than before it.

Technological change also kindles enthusiasm for expanded social protection. What was previously infeasible in low-income settings, to make cash transfers to masses of people who have only limited connections to the formal economy, is now possible with mobile wallets. The launch of national biometric identity schemes, such as *Aadhaar* in India, also helps, since cash is more likely to reach the intended recipient. As then Prime Minister Manmohan Singh announced at the launch of *Aadhaar*, its purpose is to give the poor an identity (Daugman, 2014). Of course, having the technological capability to make cash transfers to targeted individuals is a separate question from whether an expansion of such transfers is economically desirable.

An important aspect of the current enthusiasm for social protection programs is the hope that they can help to promote people out of poverty (Hashemi and Umaira, 2011). But there are grounds for doubting some of the mechanisms behind the promoting-out-of-poverty idea. Kraay and McKenzie (2014) show that evidence for poverty traps (the idea that poverty begets poverty), is quite weak. Long periods of stagnant incomes are rare, both for countries and for households, so being ‘stuck’ in a low-level equilibrium is unlikely. Various claimed causal mechanisms for poverty traps also lack credible evidence. One reputed basis for poverty traps is nutritional efficiency wages – the poor are too hungry to be able to work themselves out of poverty – but Subramanian and Deaton (1996) show that calories are too cheap for their lack to trap someone in poverty. Another claim is occupational poverty traps – the poor choose occupations and businesses with low entry costs (Dercon and Krishnan, 1996) but borrowing constraints and lumpy production technology keep them trapped earning at subsistence levels. But the evidence from studies of microenterprises in Mexico (McKenzie and Woodruff, 2006) and Sri Lanka (de Mel, McKenzie and Woodruff, 2009) is that these enterprises can begin with a range of different

starting capital levels, and those starting with a low level of capital are not necessarily trapped using a particular production technology that limits their scope for growth. In other words, stories about non-convexities that lead to poverty traps are just that – stories.

Notwithstanding the lack of credible evidence for some of the background mechanisms, many CCTs aim to break inter-generational links to poor human capital for children. What is not given enough consideration, however, is that there may be various unobservable factors that limit the productivity of the parents, and thus limit the real income available for spending on children. These unobservable factors also may directly limit the development of the children, rather than the limitations coming just through lack of income. It is impossible for observational studies to show causal effects of household resource limitations on children's human capital, no matter how strong may be the empirical correlation between income and child outcomes. But analysis of a randomized program that gave large subsidies (equivalent to two-thirds of average income) gives grounds for caution; Jacob et al (2014) find no effects of these transfers on children's schooling outcomes, or their subsequent criminal activity or health. This lack of effect from transfers contrasts with the results from observational studies in the same setting where cash transfers are associated with large changes in children's outcomes.

There are also grounds for concern about conditionality. When social protection benefits are targeted to groups that are perceived to be socially needy, individual members of those groups may have an incentive to continue the behavior that causes them to be viewed as needy. For example, in rich countries where welfare benefits to single mothers expire once their children reach a certain age there is an incentive for these mothers to have more children so as to prolong their access to these benefits (McDonald and Spindler, 1988). The same effects may occur with CCTs; for example, in Brazil's *Bolsa Alimentacao* an evaluation showed that children exposed to the program gained significantly less weight per month than similar children (Morris et al, 2004). The authors of the evaluation conclude that the failure of one of the target outcomes (children's weight) to respond positively to the program may have been due to a perception that benefits would be discontinued if the child started to grow well. That is, conditionality seemed to perpetuate the circumstances that the transfers were trying to overcome.

It is also the case that social protection programs create long-term fiscal obligations and the burden of these may harm future economic growth. It is typical to see the fiscal costs of transfer programs reported as flow measures; for example, the International Labour Office suggest that the costs of a complete social protection package in low- and middle-income countries ranges from 4-11 percent of GDP per annum (Hagemejer, 2009). Some of the available estimates also make very optimistic assumptions; for example, Asher and Bali (2014) consider a hypothetical social pension for Southeast Asia with an 11 percent growth rate in nominal incomes, no administrative costs, no disincentive effects, and no general equilibrium effects on the rest of the economy. But more sophisticated analyses are starting to forecast costs using lifetime simulations, based on methods somewhat akin to what lies behind Figure 11. For example, in New Zealand the lifetime cost of future fiscal liabilities for the cohort of current welfare beneficiaries was estimated by actuaries as equivalent to 59 percent of GDP (Bennett, 2012). This actuarial analysis also showed a large gap between the programs with the most policy attention and the ones that were the big drivers of

future costs (McBride and Greenfield, 2013). In general, the costly programs were the ones that expanded in a passive, demand-driven, way so that large future fiscal liabilities were created with not much attention paid to them.

For the countries that have not yet created these fiscal burdens they should carefully consider the incidence of the taxes needed to fund such transfer programs. In a world with increasing competition for skilled workers (Kapur and McHale, 2005) relying on highly progressive taxes to fund social transfers may be unwise. The developing countries with such taxes already suffer heavy fiscal losses from the emigration of highly skilled workers (Gibson and McKenzie, 2012) and it is likely that inter-country competition for such workers will intensify in future given uneven population aging trends. The funding of expanded social protection programs may therefore by quite vulnerable and international targets that encourage such expansion may be unhelpful.

Taking all of these considerations into account, the best that can be said about the social protection target is that the ratio of benefits to costs remains uncertain. If this target had to be consigned to a particular evaluation category, it would most likely be “Poor” given the many unintended consequences that may result.

### ***Disaster Resilience***

The target proposed by the Open Working Group is to “build resilience of the poor and reduce by x% deaths and economic losses related to disasters”. Since deaths and losses due to disasters are bad things, the goal of reducing these losses seems like a good thing. But like all good things, it is best in moderation. In fact, economics has a standard for exactly how much moderation is needed: the socially efficient standard is to reduce the risk from disasters only to the point where the marginal cost per life saved is the same as for other risk reducing activities (Viscusi, 2000). The target as stated has nothing about an efficiency standard, in terms of the level of death and loss reduction expected per dollar invested into disaster resilience, so it should be considered as poorly specified. If this target was adopted, it would likely lead to over investment in disaster mitigation activities, at the cost of under investment in other risk reducing activities.

Disasters are costly, and all else the same, we would like that they did less damage and took fewer lives. But just how costly they are is not very clear, since the estimates available typically use different methods, with quite big discrepancies between them. Some variation in estimates is also due to the multiple dimensions of impacts from natural disasters and the large redistributive effects; these make it hard to determine whether the various published cost estimates correspond to net welfare impacts (Hallegatte and Przulski, 2010). Notwithstanding these comparability issues, when a development target specifies reducing losses from a particular type of risk, and does not put it in a framework of efficient overall risk reduction, there is a high likelihood that costly distortions will result.

One example will suffice to make this point about inefficient risk reduction. In 2002, I was carrying out fieldwork on the Thai-Cambodia border, where the Thai army was clearing an area suspected of having land mines. This area had also been the site of a refugee camp, and had a high rate of metal contamination from canned food rations. Therefore the army used mechanical clearance methods because labor costs were too high for manual demining,

given the large number of false alarms from metal fragments. In order to ensure that they got every last land mine, the army subjected each square meter of ground to 16 different passes of various machine operations (e.g., rotating metal flails, heavy rolling, rolling plus magnets, and so forth). This was not very efficient, with marginal benefits falling very quickly after the first couple of passes by the machines (Marsh, Boe-Gibson, and Gibson, 2002). On the road outside the field being cleared, whole families sat precariously riding motorbikes, typically transporting live animals or other cargo in their arms, and with neither riders nor passengers having helmets or other safety devices. Clearly the level of background risk in this setting was quite high. Therefore it was socially inefficient to go to great length to reduce one type of risk to zero, as the Thai army were doing, while other, much higher, risks were not being mitigated. For example, money spent for the 15<sup>th</sup> and 16<sup>th</sup> passes of the demining machines over the same bit of ground might have better been spent buying crash helmets for the villagers.

Returning to the post-2015 targets, by enshrining a target of reducing deaths related to a particular type of risk – disasters in this case – while not dealing with other risks, it will almost surely guarantee that countries will misallocate their risk mitigation and risk bearing efforts. Society often spends too much to reduce some risks (and not enough to reduce others) because of political influences and errors in judgement about risk (Viscusi and Hamilton, 1999). These inefficiencies cost lives, which is ironic since proponents of the disasters target are probably motivated by a desire to save lives. Stricter disaster mitigation standards may have the perverse effect of creating such large social losses that they cause an *increase* in expected mortality. The reason for this paradox is that excessive disaster risk standards will reduce incomes, because ‘too many’ resources are spent on trying to prevent losses from disasters. This wasteful over-investment in reducing loss from a particular risk provides a lower return than if the money was spent in other ways, and so lowers incomes. At lower incomes, people engage in more risky activities (Lutter, Morrall and Viscusi, 1999). Therefore, in general, undertaking an inefficiently excessive risk reduction effort may, paradoxically, increase the expected number of deaths through this income pathway (Gerdtham, 2002).

Apart from making this logical point, about a poorly specified development target, no evaluation is attempted here. Fundamentally the costs and benefits of this target are unknowable because an evaluation of disaster resilience has to use endogenous damage functions and there is no clear basis on which these can be identified. These damage functions would apply if, say, attempts at protection against disasters by forming seawalls for defence against tsunamis or stop banks for flood protection, lead to increased land use in the at-risk zone, and that greater land use then causes losses to increase. For example, the economic literature on road safety regulations considers this complication (Peltzman, 1975; Traynor, 1993). In conclusion, this post-2015 target should be considered as “Poor” because it is poorly specified and if followed closely it would likely result in inefficient patterns of risk reduction.

### ***Economic Rights***

The target proposed by the Open Working Group is to “ensure equality of economic opportunity for all women and men, including secure rights to own land, property and other productive assets and access to financial services for all women and men” while the

High Level Panel suggest a target of increasing the proportion of the population with secure rights to land, property, and other assets. These can broadly be considered as targeting “economic rights” with a special focus on equalizing rights for men and women. While there is nothing egregious about this target, the motivation for the gender-focus may reflect an outdated reading of the evidence.

The evidence from agriculture is that there seems to be a gender bias in access to productive assets and inputs, and/or in the returns to entrepreneurial and labor effort. For example, a longitudinal study of coffee producers in Papua New Guinea found returns to men’s own-account coffee production up to twice the returns to women’s work on coffee (Overfield, 1998). Women reacted to this by allocating more time to the cash-earning activities that they had more control over, especially vegetable production and marketing, even though net returns to the household were lower from vegetables than they were from coffee. Female labor is the binding constraint in this environment (also seen from marriage market institutions of bride price rather than dowry) and unequal access to inputs that skews female labor allocation harmed overall household production efficiency (Overfield and Fleming, 2001). Similarly, in Burkina Faso, household output could potentially have been 10-20 percent higher if inputs were reallocated across the plots controlled by men and women (Udry et al, 1995). These authors also note how policy interventions aiming to reduce this apparent inefficiency might be thwarted by changes in male labor supply and changes in bargaining in the marriage market. Another result comes from Benin; female rice farmers are discriminated against with regard to irrigation scheme membership and access to land and equipment, resulting in significant negative impacts on their productivity and income, despite women appearing to be equally as technically efficient as men (Kinkinginhoun-Médagbé et al, 2010).<sup>32</sup>

Yet it is unclear if gender inequalities in access to productive inputs (including finance) cause the same inefficiencies in other sectors of the economy. Since agriculture declines with development while these other sectors become more important, targets for the post-2015 period should probably pay less attention to the evidence from agriculture and more attention to evidence from other parts of the economy. This non-agricultural evidence is much more recent and so may not have influenced the perceptions of participants in the debates about the post-2015 agenda. The other feature of this new evidence is that it tends to be more rigorously identified than the earlier evidence from agriculture, which was exclusively based on observational studies. Some of the most credible evidence comes from the randomized allocation of grants to microenterprise owners, which overcomes some of the problems of previous studies caused by endogeneity of input choices. In Sri Lanka, these grants resulted in large, sustained, increases in income for male owners but no increase in income for female owners (De Mel, McKenzie and Woodruff, 2009). In urban Ghana, where cash and in-kind grants were randomly given to male- and female-owned microenterprises (Fafchamps et al, 2014), this additional capital caused no gain in profits for female-owned subsistence enterprises (although there were some effects for the larger enterprises run by women). These results suggest that capital alone may not be enough to grow subsistence enterprises owned by women, and that even with equal access to inputs

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<sup>32</sup> An earlier review found that it seems to be a general feature of farming in low-income countries that males and females are equally as technically efficient (Quisumbing, 1996).

for male and female entrepreneurs there may not be the changes in outputs or profits that might be expected, based on the prior results in the agricultural sector.

It is also unclear how countries would monitor the percentage of their population with secure rights to land, property, other assets, and financial services. Most property rights are not binary variables but instead vary in strength. That subtlety would be hard to capture in an easily comparable indicator over time and space. While there is a large literature on access to finance, and one could use surveys to monitor whether men or women have higher rates of bank account ownership, technological change is quickly undermining such indicators. In many developing countries, all that will be needed to access financial services in the future is a mobile phone. Given these difficulties, one could imagine that monitoring of this proposed post-2015 target might be based on an indicator that measured formalization of informal businesses and employment, since promoting formalization is also a goal that has emerged from other parts of the post-2015 dialogue. Yet the evidence is that most informal firms appear not to benefit from formalizing and that it is unclear whether there is a public rationale for trying to formalize subsistence enterprises (Bruhm and McKenzie, 2014). Thus, if some variant of this “economic rights” target is adopted, it quite likely will be used to support a formalization drive that is probably unnecessary. Perhaps the best categorization of this target is “Uncertain” since the evidence does not show consistent patterns, and also because this rating may give policy makers grounds for pause before they embark on misguided efforts to promote formalization when the evidence to support such policies is weak at best.

### ***Full Employment***

The target proposed by the Open Working Group is to “achieve full and productive employment for all, including women and young people.” There is no similar target proposed by the High Level Panel. Yet without any further consideration of what full employment means, this is an empty target since there is no clear basis for what is to be monitored.

The typical notion used by economists for “full employment” is the non-accelerating inflation rate of unemployment (NAIRU). The NAIRU corresponds to a level of unemployment that, if policy makers attempted to lower it further (e.g. by stimulatory monetary policy), the rate of inflation would rise. It is difficult to see how a post-2015 target could accommodate this complexity, since the NAIRU is well above zero. Moreover, there are further complications. First, the NAIRU varies over time; for example, estimates for the United States suggest that the NAIRU has varied between 4.8 percent and 6.5 percent over the last couple of decades.<sup>33</sup> Second, these variations in the NAIRU may have systematic causes, such as productivity shocks, demographic change, trade policy, and changes in the efficiency of job matching (Ball and Mankiw, 2002). None of these factors would be expected to stay constant during a 15-year period that corresponds to the length of time that the post-2015 target might be on the books. Therefore, adherence to this target

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<sup>33</sup> The Philadelphia Federal Research Bank provides a spreadsheet with ‘real time’ estimates of the NAIRU calculated by their economists, available here: <http://www.philadelphiafed.org/research-and-data/real-time-center/greenbook-data/nairu-data-set.cfm> These estimates vary quarter by quarter, but also by forecast vintage; there may be different estimates of the NAIRU for the same quarter, as understanding about the state of the business cycle and the available productive capacity of the economy varies over time.

would potentially distort the flexibility of macroeconomic policy around the world for a long time.

It is also likely that this full employment target would be used by some policy makers and interest groups to argue for greater employment protection provisions. Yet the economic evidence is that employment protection regulations are costly for the overall economy, and often end up hurting the very workers they are designed to protect. In terms of economy-wide effects, Besley and Burgess (2004) study variation in pro-worker employment regulation in India, and find that states with such a bias had increased urban poverty, and lowered output, employment, investment, and productivity in formal manufacturing. These regulations also increased the size of the informal sector (hence, potentially working against the aims of the formalization target discussed under the “economic rights” section above).

There is also evidence that these type of employment protection regulations tend to hurt vulnerable workers. Leonardi and Pica (2013) exploit a natural experiment from a reform in Italy that raised the cost for employers of dismissing workers from small firms. Theory predicts that offered wages will be lower if employers demand compensation for higher expected future firing costs (Lazear, 1990), and the empirical results support this; roughly two-thirds of the expected increase in firing costs is translated onto lower wages. The effects are concentrated on the entry wage of newly hired workers, who lose up to six percent of their wage upon hire by a small firm after the reform, with the negative effects especially marked for young, blue-collar, workers with low bargaining power.

The evidence suggests that a “full employment” target would be difficult to monitor, since there is a lot of complexity as to what full employment means. Such a target also would likely cause a number of perverse, costly, and unintended consequences in the labor market and more broadly. As such, this proposed target falls into the “Poor” category.

## **CONCLUSIONS**

The targets related to poverty that have been proposed as part of the post-2015 agenda generally have poor or uncertain benefits relative to costs. The only target that is likely to have a better ratio of benefits to costs is reducing extreme poverty. The existing literature on poverty forecasts, and the discussion about special features of the poverty reduction from 1990, both point to the same conclusion, that getting to zero extreme poverty by 2030 is very unlikely. Whilst an aspiration of zero extreme poverty by 2030 could be defensible, it would be much better to improve the monitoring of whatever poverty reduction is truly occurring than to put extreme efforts into reaching an unrealistic target.

Many of the proposals made in the context of the post-2015 agenda appear to be unhinged from the reality of statistical measurement in poor countries. The proposals also appear to come from a position of cheerful ignorance about how little is known about many aspects of poverty and about the diverse ways that people may respond to interventions that are motivated by some of these proposed goals and targets. If the current report has helped to highlight some of these areas of ignorance, particularly related to the weakness of the statistical evidence on poverty, then it may have achieved some small good.

## APPENDIX

*Appendix Table 1: Using a 5% Discount Rate When Comparing Monetary Costs and Benefits of Eradicating Extreme Poverty in Vietnam (With Benefits Measured in Terms of Human Capital)*

Year	Hypothetical Costs of Eliminating Extreme Poverty With Transfers		Benefits of Eliminating Extreme Poverty and Benefit-Cost Ratio			
	Sum of the poverty gaps (trillion dong)	Present value of perpetual transfers (trillion dong)	Human Capital (trillion dong)			Benefit to Cost Ratio*
			No	Baseline	Poverty	
2002	18.2	363.9	16906	19570	2664	7.3
2004	13.6	271.5	19418	21127	1709	6.3
2006	10.9	218.9	25268	26862	1594	7.3
2008	11.5	229.9	28346	29816	1470	6.4
2010	3.4	67.6	41724	42357	633	9.4

*Source:* Author's calculations derived from joint unpublished research with Dr Trinh Le.

\* These benefit-to-cost ratios can be compared with those in Table 2 that uses a 3% discount rate. All other assumptions are unchanged from those used in Table 2.

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This paper was written by John Gibson, Professor, Department of Economics at University of Waikato and Senior Research Associate at Motu Economic and Public Policy Research. The project brings together 62 teams of economists with NGOs, international agencies and businesses to identify the targets with the greatest benefit-to-cost ratio for the UN's post-2015 development goals.

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