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Measuring Chronic Hunger from Diet Snapshots: Why 'Bottom up' Survey Counts and 'Top down' FAO Estimates Will Never Meet

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Working Paper in Economics 7/16

August 2016

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Abstract

Widely used global hunger estimates from the FAO are 'top down' in that they combine data on each country's total food balance with variance estimates from household surveys. Food balance sheets are only annual so the FAO just estimate the prevalence of chronic hunger. These estimates are criticized and recent research advocates 'bottom up' counts of hunger directly from household consumption surveys. These surveys give a snapshot of living standards, for the week, fortnight or month reference period, so only noisy measures of annual dietary energy can be derived from them. This overstated between-households variance raises the share of the population who appear below nutritional standards, for any standard set below the median, and so overstates chronic hunger. In this paper, a new method of deriving chronic hunger estimates from snapshot surveys is proposed, which also lets the transient component of hunger be identified. This method is demonstrated using a household survey from Myanmar that has repeated observations on households during the year. The transient component of hunger is almost one-half of the total and uncorrected snapshot surveys would overstate the chronic hunger rate by almost 90 percent.

Keywords

chronic hunger
survey design
transient hunger
undernourishment

JEL Codes

C81, O15, Q18

Acknowledgements

I am grateful to Trinh Le for assistance with the data and to Valerie Kozel and audiences at Otago and Reading universities and to the AJARE pre-conference workshop for helpful discussions.
All remaining errors are those of the author.

I. Introduction

This paper considers a largely ignored issue that is at the intersection of three distinct literatures: how to measure hunger, how to deploy survey resources, and how to split observed shortfalls in living standards into chronic and transient parts. In the first literature there is debate about Food and Agriculture Organization (FAO) global hunger estimates, which combine food balance sheets for each country with variance estimates from household surveys, so as to calculate the likely percentage of people suffering from chronic hunger. Criticisms of the FAO method are made by Smith (1998), Svedberg (1999, 2002), and de Haen *et al*. (2011), with rebuttals by the FAO in Naiken (2007, 2014) and Cafiero (2014). In part due to this debate, a recent set of studies argue for directly measuring hunger from household consumption expenditure surveys [HCES] (Fiedler *et al*. 2012, Carletto *et al*. 2012 and Fiedler 2013). The rising number of HCES and their use for global poverty counts may fuel some of this demand for a 'bottom up' approach to measuring hunger, which contrasts with what is here called the 'top down' FAO approach.

The fragility in these approaches is highlighted by the disparate hunger counts in a recent survey experiment (De Weerdt *et al*. 2016). But even if surveys were harmonized and sources of non-comparability were ironed out, there still is a problem in matching top-down and bottom-up estimates. The FAO food balance sheets are annual, so the hunger measures also are annual, and refer to a typical level of daily energy consumption during a year (FAO 2013, p.46). This approach to measuring chronic hunger is based on mean consumption over time being less than requirements although even within FAO it is sometimes confused with a spells approach, as seen in the following remarks from Assistant Director-General Jomo Kwame Sundaram:

> '…at least 842 million people in the world are believed to be still suffering from chronic hunger. This is a very, very strict definition…they have been hungry for at least a year. So if they have not been hungry for a month in that year, or for a season in that year, they no longer qualify as being hungry…' (Sundaram 2013).

In fact, contrary to the quotation, the FAO approach cannot distinguish between large and small intra-year fluctuations around the energy requirement that result in the same mean annual energy consumption. Indeed, there is an explicit neglect of short-term fluctuations according to Cafiero (2014, p.4) who notes '[t]hough temporary food shortages may be stressing, the FAO definition of the indicator is based on a year.'

In contrast to the annual focus of FAO estimates, advocates of direct hunger counts from HCES are relying on data that only give a snapshot of living standards for the week, fortnight or month survey reference period.[1] Absent perfect smoothing, these HCES snapshots are a noisy basis for extrapolating to annual estimates of habitual dietary energy consumption, which the FAO approach relies upon. The overstated between-households variance will raise the share of the population who appear below calorie requirements, for any cut-off set below the median, and so bottom-up direct hunger counts will exceed the top-down FAO estimates, even if for the same population and even if HCES and food balance sheets cover the same food consumption.[2] The HCES variances also pose a problem for FAO estimates, which ideally would use estimates of annual variances when distributing annual average dietary energy over the population. Since HCES do not observe households for a full year, the FAO have to use short-term estimates that have excess variability, which they dampen by indirectly relating them to income.[3]

In this paper, I propose a new method of deriving chronic hunger estimates from HCES data that accounts for excess variability from just observing a snapshot of living standards. This method is potentially useful for both the bottom-up approach of measuring hunger directly from HCES and for the top-down FAO approach. In addition, this method can identify the transient component of hunger, which is a type of welfare fluctuation neglected in the literature compared to the emphasis placed on transient poverty. The proposed method can aid the bottom-up approach since it is a viable way to estimate chronic hunger on the same annual basis as used by the FAO, without requiring year-long observations on households. Since the method provides a variance estimate for annual dietary energy it also should be a better starting point for the FAO when they derive the distribution parameter (the Coefficient of Variation) needed to spread annual energy consumption over the population, compared to the starting point provided by typical HCES data that have what Cafiero (2014) considers to be excess variability.

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[1] Some HCES try to measure long-run living standards from a single interview, by asking about months that items are consumed, and usual consumption in those months. These questions are cognitively burdensome and introduce education-related inequality into responses, and are generally unsuccessful (Beegle *et al*. 2012).

[2] The example by de Haen *et al.* (2011) of inconsistency between the two approaches – that HCES show 59% of the population in 12 African countries are undernourished while the FAO estimate was just 39% - is thus unsurprising. That this difference is an *inherent* feature of the two methods does not seem to be remarked upon in the literature.

[3] Specifically, the variance in calories between income classes is combined with a component due to other factors (Cafiero 2014). The FAO previously calculated the coefficient of variation (CoV) (needed to distribute the average across the population under assumed log-normality) from medians of ten income or calorie decile groups, with 0.05 subtracted to account for excess variability. The FAO forced the resulting CoV to lie between 0.20 and 0.35.

The proposed method needs survey resources to be deployed so that the same households are observed in at least two, non-adjacent, periods in the year. This survey design is rare. Dupriez *et al.* (2014) poll statistics offices in 100 low- and middle-income countries to obtain metadata on HCES design related to food consumption; of 95 surveys in their sample fielded since 1999, just two use this type of intra-survey panel where the same households are observed in multiple rounds of the survey.[4] The rarity of such surveys is unlikely to be due to lack of resources since many surveys budget for revisiting households to check on their diary-keeping. For example, among surveys using the diary method (40 percent of the Dupriez *et al.* 2014 sample) the median number of interviewer visits made to the household was five, and some surveys revisited households seven or more times in quick succession. Little new information relevant for food policy is obtained after the second or third visit, when these visits are for short, adjacent, periods, such as every second day (Engel-Stone *et al.* 2016). Thus a potentially better use of survey resources would be to revisit the households after some months have elapsed. Such revisits are feasible in surveys that have interview teams work continuously over a year in each region, with the teams often doubling back to revisit the same areas in each season even if traditional survey design has them avoiding households whom they had seen earlier in the year.[5]

The literature on the optimal deployment of survey resources is limited. In part this may reflect a focus by statistics offices on surveys that should yield accurate measures of means and totals for the population, even if data are atypical of long-run living standards for individual households. Gibson *et al*. (2003) is an exception, for the important case of urban China where households were meant to keep an expenditure diary for a full year and where the statistics office faced growing non-compliance and unrepresentative samples due to this heavy reporting burden. The month-by-month expenditure records for each household were used to see how switching to a monthly reference period, with households revisited once, twice, or five times per year, would affect estimates of mean annual expenditures, poverty, and inequality. A monthly reference period and surveying 1/12th of the sample every month gave the same mean annual expenditures as the year-long diary but the standard deviation was twice as high and the poverty head count and Gini inequality measures inflated by 50-65 percent. This just reflects the fact that snapshots

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4 These two examples are from Belarus and Georgia where households are observed each quarter. A more common design is panels across multiple surveys, of which there were nine. Typically these cross-survey panels are linking across surveys that may be separated by some years, whereas the intra-survey panel can be conveniently thought of as an intra-year panel even if fieldwork for a particular survey spills into two calendar years.

5 The surveys that concentrate their fieldwork in a single period of the year are less adaptable to the proposed design. Just under one-half of the Dupriez *et al*. (2014) sample use this temporally-concentrated design; arguably these surveys could especially benefit from observing living standards in different times of the year.

from HCES may adequately measure annual means but not annual variances, and supports the FAO concerns about excess variability in HCES-based measures, noting that these were urban dwellers so it was not seasonality on the production side causing this variability.

The China results improve greatly for designs with households observed for two months, six months apart. First, with naïve extrapolation, where data from both month-long observations were multiplied by six and combined to give an annual total, the overstatement in variance-based measures was less; by about 50 percent for the standard deviation and 32-36 percent for poverty and inequality. But the real benefit from revisiting households six months after the initial visit came from using a corrected extrapolation method due to Scott (1992); with this method the head count poverty rate equaled that estimated from year-long diaries, and the standard deviation and Gini index were within six percent of the annual estimates (Gibson *et al*. 2003). This corrected extrapolation relies on empirically estimated correlations between values of a living standards indicator in two separate periods for the same households; correlations that are implicitly assumed to be 1.0 when data from snapshot surveys are naïvely extrapolated to annual totals.

A similar corrected extrapolation is proposed here as a way to derive chronic hunger estimates from snapshot surveys, so the good performance in China against the benchmark of a year-long diary should be borne in mind. Intuitively, the method uncovers more about the typical living standards of a household because seeing the same household some months later reveals new information about it, compared with seeing it just once or else seeing it repeatedly in a short, concentrated period like a fortnight (as in surveys with diary-checking visits). A related study, in the context of randomized experiments in economics, notes that a common design uses a baseline survey and single follow-up (McKenzie 2012). This suits highly auto-correlated and relatively precisely measured educational outcomes but is not optimal for less auto-correlated outcomes such as incomes, expenditures, and microenterprise profits. For such outcomes, taking multiple measurements at relatively short intervals (as happens with an intra-year panel), lets one average out the noise and increase statistical power.

Whether the fluctuations that create this ‘noise’ are of interest or are just a nuisance depends on whether they are viewed as measurement error rather than as genuine volatility. At least in the literature on poverty, the fluctuations over time that are revealed by panel surveys are considered worthy of study in their own right. While across-survey panels are mostly used, there are some intra-year panels providing evidence on transient poverty (Gibson 2001). Yet studies that split observed shortfalls in living standards into chronic and transient components pay little attention to hunger. For example, a search of journal article titles and abstracts in *EconLit* (on November 7, 2015) found 38 journal articles on transient poverty but none on transient hunger.

This lack of attention to transient hunger is despite the fact that it can, potentially, be measured from the same HCES surveys that are considered trustworthy enough to study transient poverty. One reason for the sparse literature on transient hunger may be the emphasis by FAO on annual estimates, with an explicit neglect of short-term fluctuations.

The method proposed here can reveal transient hunger under the 'components approach' to welfare fluctuations proposed by Jalan and Ravallion (1998). Under this approach, chronic poverty is the part of total poverty due to permanent consumption being below the poverty line, while transient poverty is the remainder. Thus, a chronically poor household may have transient welfare fluctuations, with consequent welfare costs (or conversely, unmet demand for smoothing mechanisms), and these fluctuations are counted by this method. Another decomposition is the spells approach, where 'the chronic poor are identified based on the number or length of spells of poverty they experience – so that all poor households are classified as either chronic or transient poor' (McKay and Lawson 2003, p. 427). The spells approach is a discrete method that does not consider information about transient welfare for chronically poor households.[6]

The remainder of the paper is structured as follows: Section 2 describes the method of deriving chronic hunger estimates from the snapshots of living standards provided by HCES. Section 3 discusses the data from Myanmar used to illustrate the method, while Section 4 has the results and Section 5 concludes.

2. A Method of Deriving Chronic Hunger Estimates from HCES

In order to provide a bottom-up measure of hunger that should match FAO estimates, the ideal (but impractical) survey would observe the same households over the course of a year. Such a design would likely show that many short term shocks tend to cancel out over a year. In contrast, a snapshot from a short reference period survey, even if it is for a month, will have a higher variance because some of the shocks, but not their reversal, occur in that period. This higher variance, compared with the variance of annual estimates, will inflate the share of the population who are seemingly below the energy requirement, for any threshold that is below the median, and thus will overstate the prevalence of chronic hunger (Figure 1).

[6] An example of the spells approach (albeit using indicators of food insecurity rather than food intakes) is Maxwell *et al*. (2014) who show movements into and out of food insecurity using a four wave panel survey over two years.

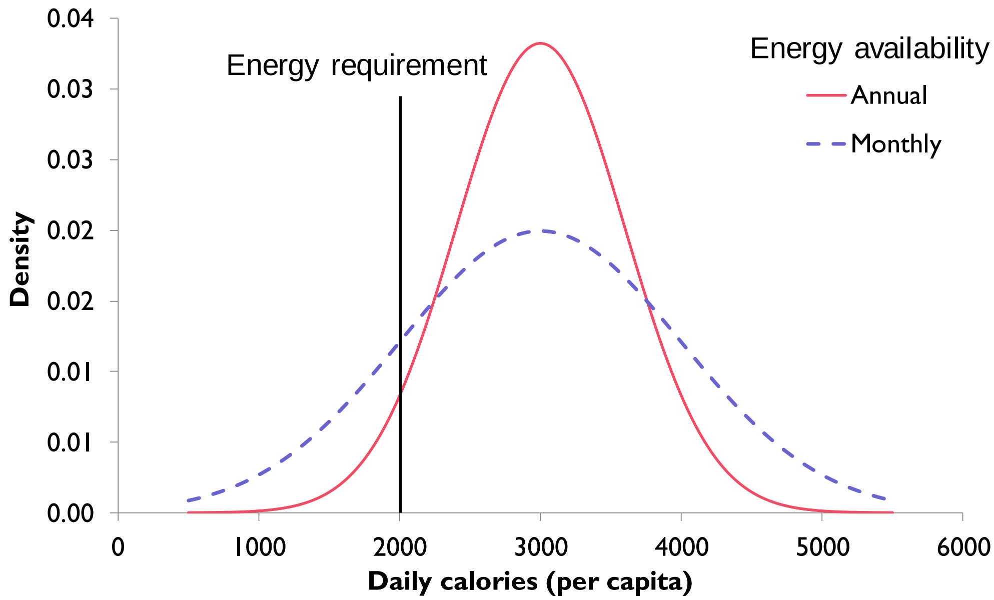

Figure 1: Over-stated Chronic Hunger from a Snapshot Survey

It should be obvious that simply taking survey data that are based on a monthly reference period and multiplying them by 12 so as to annualize them does not alter the problem shown in Figure 1. Let $\bar{x}_m$ refer to the average, and $V(x_m)$ the variance, of monthly calories across all i households and t months in the year. Extrapolating to annual calorie totals by multiplying monthly calories by 12 gives an estimated variance of annual calories of $144 \cdot V(x_m)$. This will overstate the true variance in annual calories, which is defined as:

(1) $$V(x_a) = \frac{1}{N}\sum_{i=1}^{N}\left(x_{i,a} - \bar{x}_a\right)^2$$

where $x_{i,a}$ is annual calories for the ith household and $\bar{x}_a$ is the average annual calories across a sample of size N (so for a nationally representative sample this should be a counterpart to what FAO hope to estimate from Food Balance Sheets). Equation (1) can be expressed as:

(2) $$V(x_a) = \sum_{t,t'=1}^{12} r_{t,t'}\sigma_t\sigma_{t'}$$

where $r_{t,t'}$ is the correlation between calories in month t and month t' and σ_t is the standard deviation across households in month t. This follows because $x_{i,a} - \bar{x}_a$ in equation (1) can be expressed as the sum of the deviations of each household's monthly calories from the mean for that month, $d_{it} = x_{it} - \bar{x}_t$ and the d_{it} terms are components of the correlation coefficient:

(3) $$r_{t,t'} = \frac{1}{N}\sum_{i=1}^{N} d_{it}\, d_{it'} \Big/ \sigma_t \sigma_{t'} .$$

By assuming that the dispersion across households does not vary from month to month, i.e., $\sigma_t = \sigma_{t'}$, Scott (1992) shows that equation (2) can be expressed in terms of the average of all of the inter-month correlations, $\bar{r}$:

(4) $$V(x_a) = [12 + 132 \cdot \bar{r}]V(x_m) .$$

Hence the variance from simple extrapolation to annual totals, $144 \cdot V(x_m)$, equals $V(x_a)$ only in the special case of $\bar{r} = 1$. The assumption to get to equation (4), that dispersion across households does not vary from month to month, is unlikely to be exactly true. For example, heterogeneity in diet preferences has scope to have more impact in seasons of plenty (for example, post-harvest) than in lean seasons when reduced access to food (or higher prices) makes diets more alike (Behrman *et al*. 1997).[7] The results below include a simulation to explore the sensitivity to this assumption and it seems to not have much bearing, at least for the empirically observed degree of seasonal variation in the inter-household variance of calories in Myanmar. Indeed, in the results from urban China (Gibson *et al*. 2003), where the corrected extrapolation method worked very well at recreating annual variance-based measures, the degree of dispersion across households did vary month by month but evidently this did not hamper the performance of the method.

The corrected extrapolation method proposed by Scott (1992) uses estimates of $\bar{r}$ to scale the *i*th household's deviation from the overall monthly average, $(x_{it} - \bar{x}_m)$ up to an annual value, and adds this to the annual average across all households, $\bar{x}_a = 12 \cdot \bar{x}_m$:

(5) $$x_{i,A} = (x_{it} - \bar{x}_m)\sqrt{12 + 132 \cdot \bar{r}} + 12 \cdot \bar{x}_m .$$

For example, if the average correlation between the same household's calories in all pairs of months in the year is 0.5, the scaling factor is only 8.8 (=√78), rather than 12 that is implied by simple extrapolation. In other words, deviations of a household's one-month calories from $\bar{x}_m$ have a smaller effect on the annual variance than under simple extrapolation, leading to a less

[7] Support for this claim comes from Maxwell (1996) who shows that various coping strategies like skipping meals and eating less preferred foods are practiced more in the rainy (lean) season in Kampala than in the dry season when food is more abundant.

dispersed distribution of annual calories than what is implied by a snapshot survey (since simple extrapolation just mirrors the variance in the snapshot survey). The intuition is that some of the factors that caused the household to deviate from the sample average for the month might subsequently be reversed in the rest of the year but applying a scaling factor of 12 to these deviations locks them in, as if they occur in every month of the year.

The most reliable estimate of $\bar{r}$ would use the 66 correlation coefficients, $r_{t,t'}$ between all $i \neq j$ pairs of months (noting that $r_{t,t'} = r_{t',t}$ to get to the 132 used in equation (5)) but this requires observations on each household's calories in every month in the year. Such a survey design is impractical because of the burden on respondents and in fact was the design in China where problems of rising non-compliance lead to a study of potential reform by Gibson *et al*. (2003). Instead of needing 66 values of $r_{t,t'}$ to form the average, a sampling approach can be used, estimating $\bar{r}$ from only a few of the possible inter-month correlation coefficients for calories in the various $i \neq j$ pairs of months. This sampling approach reduces the cost of fieldwork and the burden on respondents but relies on the $r_{t,t'}$ having roughly the same value and varying little as the gap between t and t' increases. Existing evidence (which is for consumption expenditures rather than for calories) supports this assumption. For example, a survey from Zambia found that $r_{t,t'}$ fell by just 0.0078 for each month that the gap between t and t' increased (CSO 1995). In the data from urban China used by Gibson *et al*. (2003) the correlations between observations on the same household showed no statistically significant (t=0.2) time trend as the months between the two observations increased and each additional month that separated the two observations changed the correlation coefficient by just 0.004.

The bold line in Figure 2 shows by how much the variance of an extrapolation from a monthly reference period, $144 \cdot V(x_m)$, will overstate the true variance in annual calories, $V(x_a)$, as the average correlation between months varies. For example, if $\bar{r} = 0.9$ there is just a small overstatement of about ten percent, but at $\bar{r} = 0.7$ the overstatement factor is 1.4, and it grows to 1.8 (2.8) if the average correlation between the same household's calories is as low as 0.5 (0.3). The dashed line in Figure 2 adjusts for possible seasonal differences in the degree of dispersion across households, letting the inter-household variance of monthly calories be 25 percent less in a hungry season (that is assumed to be for three months) when diets are constrained.[8] The correction factor in equation (5) to apply to the variance of an extrapolation from a one-month reference period, so that it matches the variance if households were observed for a year, would need to be adjusted by six percent to account for this unequal dispersion of calories across

[8] This difference between the two seasons is based on what is observed in the data for Myanmar described below.

months. This is only a small adjustment compared to the size of the overall correction factor and so this complication, due to $\sigma_t \neq \sigma_{t'}$, is not considered further.

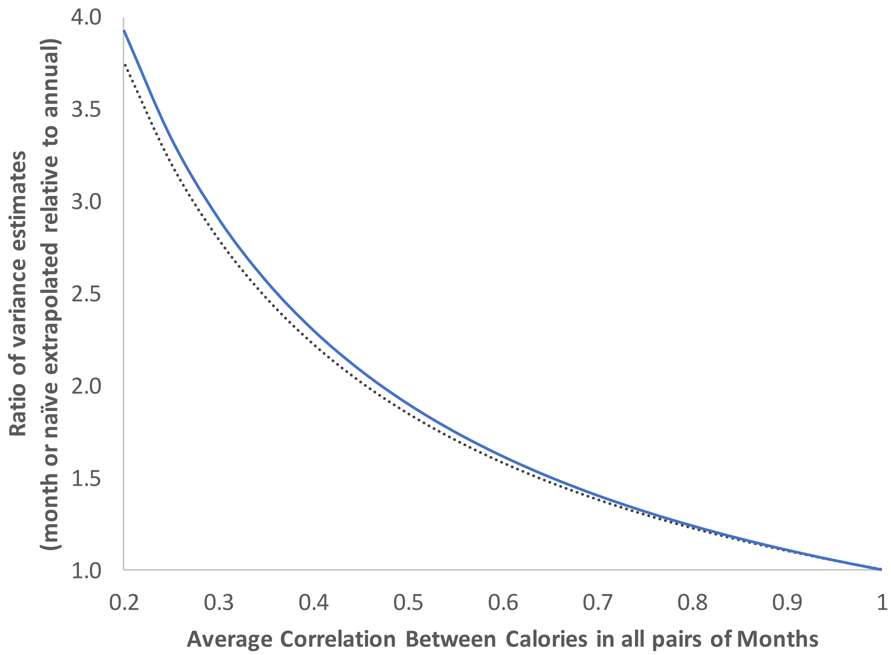

Figure 2: Variance of Monthly Calories (or naïvely annualized) Relative to Annual Variance

The fact that the adjustment in Figure 2 for possible seasonal differences in the degree of dispersion across households has such a small effect, compared with the effect of incomplete smoothing $(\bar{r} < 1)$, suggests that there may be a misunderstanding in the literature. Other studies have noted that surveys with short observation periods have higher variance, but they attribute this to seasonality, as seen in this quotation from FAO researchers (emphasis added):

> 'Usually, the observation is limited to the amount of food consumed over a very short period of time (a day or a week) in order to reduce problems associated with recollection. While evenly spreading the various survey rounds over the year can help improving the estimate of the mean consumption, it induces additional variance in the data to the extent that food consumption <u>varies systematically across seasons</u>' (Cafiero and Gennari 2011, p.9).

In fact, systematic variation across seasons is just one, quite possibly minor, reason why the variance from surveys with short reference periods will overstate the annual variance. The fluctuations in calories for the same household over time may be due to many reasons, including demographic shocks from changes in household composition, income shocks, price shocks, and changes in food availability, whether seasonally or with other periodicity.

Consistent with this claim of a misunderstanding in the literature, in the results reported below, the intra-year correlation in per capita calories is lower for urban households than it is for rural households; yet country folk are more at the mercy of seasonal fluctuations. Urban dwellers have greater instability in their household size, most probably from hosting rural relatives for extended visits, yet this type of fluctuation and its effect on diets is rarely considered in the literature.[9] The emphasis on seasonality directs attention towards survey designs where fieldwork is staggered over 12 months of the year but with each household observed in just one time of the year; such designs cannot deal with other sources of fluctuations, compared with what is possible with relatively ignored designs such as intra-year panels.

3. Data and Descriptive Statistics

In order to illustrate the method of deriving chronic hunger estimates from diet snapshots I use the Integrated Household Living Conditions Assessment (IHLCA) survey from Myanmar. This nationwide survey of over 18,000 households collected the first round of data in December 2009 and January 2010, and the second round in May 2010 when all households were revisited.[10] It is a multi-topic survey with a consumption expenditure module that uses the recall method. In comparison to many multi-topic surveys, such as the Living Standards Surveys of the World Bank, the IHLCA consumption recall has a much longer list of food groups. Specifically, 228 foods were covered, divided into two groups; for 46 major staples and calorie-sources a one month recall was used, while for a further 182 foods a one-week recall was used (Table 1). These other foods are less important calorie sources (contributing just 26 percent of the total) so the one-week recall data are extrapolated to one month totals; this also was the approach used in the official report on the IHLCA data, with the constructed monthly data for each of the two rounds of the survey then multiplied by six and added together to give (naïve) annual estimates.[11]

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[9] Halliday (2010) is an exception in noting that household size may be quite fluid, with evidence presented on the high proportion of households that experience fluctuations in household size over the short to medium term.

[10] Some border states are not fully covered because they were inaccessible in 2004/05, when the first IHLCA survey was carried out. The 2009/10 survey was based on the same communities.

[11] It may seem inconsistent to use naïve extrapolation for these 182 foods while criticising use of naïve extrapolation from a one month reference period to annual totals (or equivalently, criticising the treatment of one-month data as equivalent to annual data). Three factors that favour this choice are that (i) these foods are less important sources of calories, (ii) there are no revisit weeks within the month to calculate the required correlation for making a corrected extrapolation from one week to one month, and, (iii) there is less overstatement of the variance in going from one week to one month compared with going from one month to one year.

Table 1: Structure of the IHLCA Food Quantity Recall Questionnaire

Quantity Consumed in Last 30 Days		Quantity Consumed in Last 7 Days	
Number of items	Broad food category	Number of items	Broad food category
6	Rice	16	Pulses, nuts and seeds
7	Other cereals	11	Meat and dairy
10	Oils and fats	37	Fish and seafood
5	Dairy	11	Roots and tubers
18	Other foods	31	Vegetables
		21	Fruits
		14	Spices and condiments
		12	Other foods
		5	Alcohol
		24	Meals outside the home

In addition to the number of food groups, three other features make the IHLCA survey a useful HCES for measuring hunger. The survey questions focus on consumption rather than on acquisitions (which do not equal consumption because of storage and other leakages). Food and beverages consumed outside of the home are covered in great detail, with 24 categories just for these. Finally, for all food categories, including those consumed out of the home, the survey asks about quantities consumed, and uses the most natural local units for each type of food (with metric-equivalent conversion done post-interview at the survey processing stage). Consequently there should be more confidence in the food quantity and calorie data from the IHLCA survey than from many other HCES that are less suited to nutritional analyses.

Descriptive statistics from the two rounds of the survey, and the correlations between data in each round for the same household, are reported in Table 2. The survey shows dietary energy averages 2600 calories per person per day. This average is about two percent higher in round 1 (December/January), during the main paddy harvest. The variance across households in the May survey round is three-quarters that of the main harvest period, which is consistent with the argument made by Behrman *et al.* (1997) that heterogeneous dietary preferences are more easily expressed in times of plenty than in lean seasons.

Table 2: Descriptive Statistics and Intra-Year Correlations in IHLCA Survey
Myanmar 2009-2010

	Round 1 (Dec-Jan)		Round 2 (May)		Correlation
Variable	Mean	Std Dev	Mean	Std Dev	between rounds
	------------------------National level (n=18274)------------------------				
Monthly calories per household (thousands)	378.18	184.24	371.93	168.99	0.68
Household size	5.01	2.18	4.99	2.16	0.92
Calories per person per day	2616	949	2576	821	0.45
Age of household head	53.21	13.80	53.40	13.72	0.95
Household head is female (=1, else 0)	0.20	0.40	0.21	0.41	0.90
Household head is married (=1, else 0)	0.74	0.44	0.73	0.45	0.88
Years of education of household head	6.27	3.98	6.27	3.98	0.86
H'hold head mother tongue is Myanmar (=1, else 0)	0.78	0.41	0.78	0.41	0.91
Household head is Buddhist (=1, else 0)	0.90	0.31	0.90	0.31	0.96
Household head is a farmer (=1, else 0)	0.42	0.49	0.40	0.49	0.73
	------------------------Rural areas only (n=12863)----------------------				
Monthly calories per household (thousands)	400.62	190.37	393.29	172.22	0.68
Household size	5.05	2.15	5.03	2.14	0.93
Calories per person per day	2741	950	2696	813	0.44
Age of household head	52.14	13.76	52.33	13.68	0.95
Household head is female (=1, else 0)	0.18	0.39	0.19	0.39	0.91
Household head is married (=1, else 0)	0.77	0.42	0.75	0.43	0.87
Years of education of household head	5.39	3.45	5.39	3.45	0.82
H'hold head mother tongue is Myanmar (=1, else 0)	0.76	0.42	0.77	0.42	0.93
Household head is Buddhist (=1, else 0)	0.91	0.29	0.91	0.29	0.97
Household head is a farmer (=1, else 0)	0.54	0.50	0.52	0.50	0.67
	------------------------Urban areas only (n=5411)----------------------				
Monthly calories per household (thousands)	316.18	149.59	312.91	144.11	0.61
Household size	4.90	2.26	4.87	2.22	0.89
Calories per person per day	2270	857	2245	752	0.38
Age of household head	56.17	13.48	56.33	13.39	0.94
Household head is female (=1, else 0)	0.26	0.44	0.27	0.44	0.89
Household head is married (=1, else 0)	0.67	0.47	0.66	0.47	0.88
Years of education of household head	8.67	4.35	8.67	4.35	0.87
H'hold head mother tongue is Myanmar (=1, else 0)	0.84	0.37	0.84	0.37	0.82
Household head is Buddhist (=1, else 0)	0.87	0.34	0.87	0.34	0.96
Household head is a farmer (=1, else 0)	0.06	0.24	0.06	0.23	0.70

While the sample average dietary energy is similar between the two rounds, at the level of each household there is a lot of intra-year volatility. The correlation in total monthly calories per household is only 0.68, and in per capita daily calories is only 0.45. These correlations are well below the value of 1.0 that is assumed if data from snapshot surveys are extrapolated to annual totals. These low correlations also suggest that the basis of the FAO approach, of trying to measure habitual dietary energy consumption, may be misplaced; outcomes with autocorrelation of just 0.45 can hardly be considered habitual. Two other features of these calorie correlations are notable; they are lower in per capita terms than in household total terms because household size also fluctuates over time, and they are lower in urban areas than in rural areas. Both of these features suggest that intra-year volatility of calories is not just due to seasonality, which would be expected to more affect rural households and more affect total calories. Instead, demographic shocks and other sources of volatility may be more important than seasonality.

The correlations for the other household characteristics described in Table 2 fit well with what is known about the reliability of survey data. For example, Fuller (1987) reports reliability ratios (the share of measured variation due to variation in the true but unobserved data) based on repeated interview studies for age and gender that are greater than 0.95, for education it is 0.88, and for unemployment it is 0.77. Gibson and Kim (2013) estimate reliability ratios for household expenditures of 0.51 and for adult school years of 0.86. In the IHLCA data, the age, gender, religion and mother tongue of the household head have correlations across repeat visits ranging from 0.90 to 0.96 (mean of 0.93), the years of schooling of the household head has a correlation of 0.86, and occupational data (whether the household head worked as a farmer in the last seven days) has a correlation of 0.73. In contrast to the stability of demographic characteristics, calories exhibit much greater intra-year volatility. This supports the finding of McKenzie (2012) that many economic outcomes, such as incomes and expenditures, have low autocorrelations. For such outcomes, it is useful to take multiple measurements over time, since snapshots will be a misleading picture of the usual position of the household.

4. Results

The estimates of per person daily calories from each survey round are annualized in two ways: a naïve extrapolation that multiplies estimates from each round by six and then adds them, and a corrected extrapolation using equation (5), which is based on the intra-year correlation of 0.45. A single daily energy requirement of 2000 calories is used because tailoring requirements to the demographic composition of each household would give no further insights about intra-year volatility. With the naïve extrapolation, 22 percent of households, containing 26 percent of the population, have dietary energy below the 2000 calorie threshold. If the analysis is recast in terms of monthly reference periods the estimated prevalence of hunger would be the same, since annualizing data by naïve extrapolation reproduces the volatility in short reference period data.

In contrast, the corrected extrapolation, which uses the empirically estimated $\bar{r} = 0.45$, shows that just 14 percent of the population is below the 2000 calories threshold. This reduction in the estimated prevalence of chronic hunger is because the corrected extrapolation has lower variance, with a CoV of 0.20 compared to 0.28 under naïve extrapolation. The distribution of the naïve and corrected extrapolation estimates are presented in Figure 3.

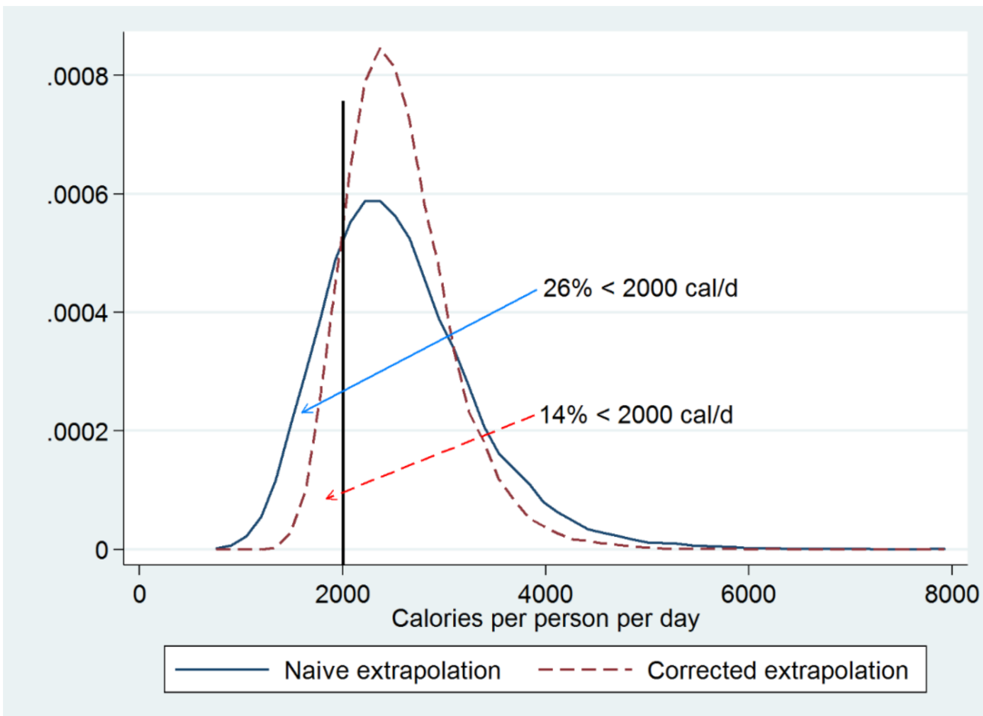

Figure 3: Chronic Hunger Overstated by Naïve Extrapolation from Monthly Calories to Annual

Since naïve extrapolation is equivalent to using the monthly data, Figure 3 also shows why bottom-up survey-based approaches to measuring hunger from diet snapshots will not meet top-down FAO estimates based on distributing annual average dietary energy from food balance sheets across the population. The bottom-up survey approach would estimate that 26 percent of people in Myanmar are hungry while the FAO approach would estimate that just 14 percent are hungry, as long as the steps that the FAO use to dampen the excess variability in short reference period survey data give the same CoV as what the corrected extrapolation used here gives. In other words, with a snapshot survey one would be likely to overstate the chronic hunger rate by almost 90 percent in this setting (irrespective of whether one uses the original monthly data or naïvely extrapolates to annual calorie totals).

The 12 percentage point gap between these two estimates of the hunger rate represents the transitory component of hunger. Thus, when hunger in Myanmar is observed with a survey using a monthly reference period, almost half of that measured hunger would be transitory. With such a large transitory component, it seems wrong of the FAO approach to ignore this important component of suffering by focusing so much attention on a definition of hunger based on a year. Moreover, this transitory hunger may be geographically widespread, since both urban and rural households seem to have large intra-year fluctuations in calories.

4.1 Sub-national Analyses

The literature on splitting observed shortfalls in living standards into chronic and transient parts ignores hunger. The loss of understanding due to this lacuna is worsened by the hunger literature attributing intra-year fluctuations to seasonality rather than to other constraints on the ability of consumers to smooth calories over time. In order to demonstrate how misplaced the attention to seasonality has been, two sub-national analyses are conducted here. The first looks at intra-year correlations in calories across four different agro-ecological zones of Myanmar: Delta, Coastal, Dry Zone, and Hills. Figure 4 maps the States and Divisions of Myanmar into these zones. The focus is especially on the Dry Zone; a large low-lying region with a semi-arid climate, situated between two higher regions, the Shan plateau to the East and the Chin Hills and Rakhine Yoma to the west. The elevated western area acts as a barrier between central Myanmar and the Indian subcontinent, causing a rain shadow effect on the Dry Zone. Thus, the Dry Zone is the sort of area where seasonality may be quite pronounced, especially compared to the Delta zone which has the biggest irrigated area in Myanmar.

Yet despite the greater seasonality in the Dry Zone, the intra-year volatility in calories in that zone is no higher than it is in the largely irrigated Delta zone. Table 3 reports two sets of correlations between per capita daily calories in round 1 and round 2. The first are Pearson correlation coefficients, which the corrected extrapolation method in equation (5) relies upon. For the rural and urban areas of the Dry Zone these are 0.42 and 0.39, compared to 0.41 and 0.35 for the Delta. The second set of values are Spearman correlation coefficients for the rank of each household in the per capita calorie distribution in each round of the IHLCA survey; since these rank correlations are much smaller than one they show the reshuffling that happens between rounds. The values are, again, almost the same, at 0.43 in the Delta and 0.42 and 0.44 in rural and urban areas of the Dry Zone. Since the Delta and the Dry Zone should differ in their degree of seasonality, the similar intra-year instability in calories suggests that it is other factors that limit the ability of households to smooth dietary energy over the months of the year.

The second sub-national analysis considers characteristics of households that may be associated with intra-year instability of dietary energy. The results of two regressions are shown in Table 4, where the dependent variable is the absolute change in per capita calories between each survey round, relative to the combined-rounds mean for each household. This measure of intra-year variability at the household level is unrelated to whether the household head was a farmer (the explanatory characteristics are averaged across rounds) yet farm households would be expected to be more at risk of seasonality than others. Instead, households whose head is more educated have statistically significantly higher fluctuations in calories over time (although this effect becomes insignificant once agro-ecological zones and urbanity are controlled for). It is also apparent that minority households, in terms of either not having Myanmar as a mother tongue or not being Buddhist, experience significantly greater intra-year variability in calories than do other households.

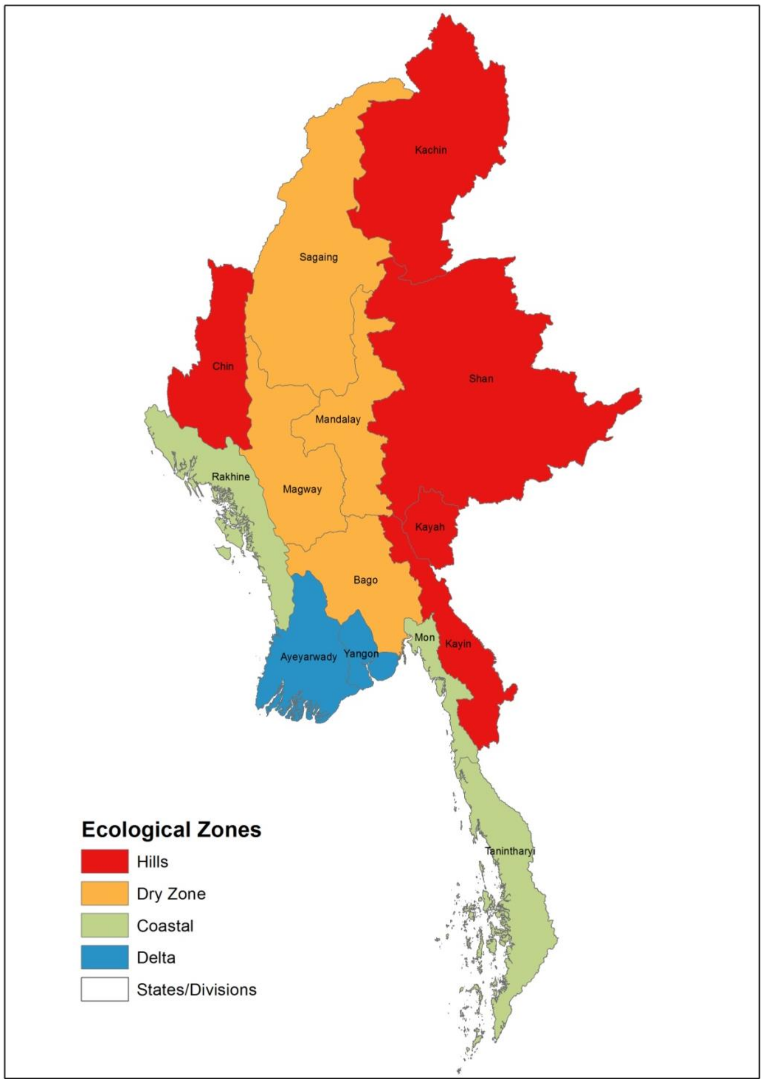

Figure 4: The Agro-Ecological Zones of Myanmar

Table 3: Correlations Between Survey Rounds for Per Person Daily Calories

	Pearson (product-moment)		Spearman (rank)	
	Rural	Urban	Rural	Urban
Hills	0.44	0.48	0.46	0.44
Dry Zone	0.42	0.39	0.42	0.44
Coastal	0.35	0.18	0.39	0.37
Delta	0.41	0.35	0.43	0.43

Table 4: Associations Between Household Characteristics and Intra-Year Calorie Variability

Explanatory Variables	No Area Fixed Effects		With Area Fixed Effects	
	Coefficient	t-statistic	Coefficient	t-statistic
Age of household head	0.03	1.67	0.02	1.36
Household head is female (=1, else 0)	-0.67	-0.75	-0.71	0.79
Household head is married (=1, else 0)	-1.91	-2.28	-2.03	2.44
Years of education of household head	0.16	2.53	0.09	1.46
H'hold head mother tongue is Myanmar (=1, else 0)	-2.52	-4.09	-1.87	2.33
Household head is Buddhist (=1, else 0)	-2.57	-2.82	-2.70	2.99
Household head is a farmer (=1, else 0)	-0.54	-1.07	0.26	0.50
Agro-ecological zones:				
Urban Hills			-5.75	2.61
Rural Hills			-9.20	4.47
Urban Dry Zone			-6.99	3.60
Rural Dry Zone			-7.64	4.06
Rural Coastal			-6.16	3.04
Urban Delta			-6.93	3.41
Rural Delta			-8.80	4.56
Constant	0.29	17.59	0.31	16.05
F-test (slopes=0)	8.80		6.57	

Notes:

Coefficients show the percentage point change in calorie variability (the absolute difference between per capita calories in each round relative to the inter-round average) for a unit change in the explanatory variable.

The omitted agro-ecological zone is Urban Coastal.

The *t*-statistics are from robust standard errors and the *F*-statistics are significant at $p<0.01$.

The final feature of Table 4 is that even after controlling for household characteristics, the rural Dry Zone – which is expected to be a seasonal environment – has less intra-year variation in calories than seen in most other regions. Specifically, only the rural Hills and rural Delta have less intra-year variability, while all urban zones and the rural Coast have greater intra-year variability than occurs in the rural Dry Zone. Evidently, factors other than just seasonality contribute to intra-year fluctuations in dietary energy at the household level.

5. Conclusions

This study has proposed a new method of deriving chronic hunger estimates from household consumption expenditure surveys, which is demonstrated using data from Myanmar. The study adds to three distinct literatures: how to measure hunger, how to deploy survey resources, and how to split shortfalls in welfare into chronic and transient parts. In terms of measuring hunger, both sides of the debate over FAO global hunger estimates seem to be at fault; the advocates of bottom-up survey approaches do not emphasize that snapshot surveys overstate variance and will exaggerate the rate of living beneath nutritional cut-offs, while FAO advocates focus on habitual dietary energy but there is little habitual in an indicator whose auto-correlation is just 0.45. Also, proponents of both approaches use a narrative of seasonality when discussing intra-year dietary variation, ignoring other constraints on consumers smoothing calories over time. The result that urban people in Myanmar have less smoothed calories, and that the Dry Zone and farmers are no more prone to intra-year calorie fluctuations than are others, is a counter-example to the (over-) emphasis on seasonality in the hunger measurement literature, and corroborates findings of low auto-correlations in other living standards indicators for urban households elsewhere (Gibson *et al*. 2003).

Yet little is known about intra-year volatility of living standards because of the wasteful way household survey resources are deployed. Instead of designs that reveal if short-term shocks during a snapshot survey are eventually reversed, most designs have a single, short, observation window, where measured outcomes will be some unknown mix of chronic and transient welfare components. A misplaced focus on seasonality may lead to designs that stagger the sample over the year, so that the mean for a synthetic household can be interpreted in annual terms, but this design locks in shocks from the snapshot period as if they occur in each period of the year. For example, annualizing a one-month reference period by multiplying by 12 treats a shock in that month as if it occurs in every month. Yet with an auto-correlation of just 0.45 for per capita daily calories the extrapolation factor to scale dietary shocks up from the reference month to a year should be just 8.4 since part of the shock is subsequently reversed.

It would be helpful if survey evidence enabled these intra-year correlations to be more widely estimated, and to test if they decay as time elapses. We rarely get this chance because intra-year panel surveys are so rare. While many surveys revisit households, they are structured in a way that reveals little about deviations of current living standards from their long-run level. For example, the typical diary-keeping survey in the Dupriez *et al*. (2014) sample has the interviewers make five visits to each household but these are for short, adjacent periods (such as every second day) so hardly anything new is revealed about the household after about the third visit, compared to what may be learned if those revisits had been several months later. These designs ignore the point made by McKenzie (2012) that for outcomes with low autocorrelations there is value in multiple measurements over time since snapshots will be a misleading picture of the usual position. Moreover, surveys with revisits need not be too costly if the main aim of the revisit is to estimate $\bar{r}$ for doing a corrected extrapolation (and splitting observed welfare into chronic and transient parts); a randomly selected subset of survey areas may suffice, with Gibson (2001) using a one-in-seven revisit sample that added about 10 percent to the fieldwork cost for a standard cross-sectional survey design with fieldwork staggered over the year.

In addition to implications for hunger measurement and survey design, the results here suggest that the literature on splitting observed shortfalls in living standards into chronic and transient parts is deficient in ignoring transitory hunger. In Myanmar it appears that about half of the observed hunger is transitory, and if the same holds in other low-income settings this makes transitory hunger too large of a problem to ignore. The appropriate policies for dealing with transitory hunger may be quite different to those for reducing chronic hunger, since they will involve smoothing mechanisms such as insurance and social protection. Moreover the agencies who can best intervene to deal with transitory hunger may not be typical participants in food policy debates, so broadening the focus away from the current emphasis on chronic hunger may require creative approaches to mitigate this avoidable source of misery.

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