Poverty Measuresurement and Dynamics[1]

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Poverty Measurement and Dynamics

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ABSTRACT

The importance of measurement error for the measured versus true dynamics of indices of poverty, income and unemployment has long been recognized in projects using data from developed countries. Often such studies have found the measurement error process to be less persistent than the underlying true process, making the *measured* poverty or unemployment dynamics (for example), appear less persistent than they truly are. Owing to the lack of appropriate data for developing countries, however, these methodologies have not been widely imported into studies on poverty and income dynamics in developing countries. This is unfortunate as the conclusions drawn from developed countries are likely not generalizable to developing economies where income processes are generally less persistent owing to their dependence on weather shocks, illness, and other 'high frequency' shocks. In addition, the *statistical* methodologies which are commonly employed for data on households in developed economies may also not be appropriate for the developing country context. Households in developing economies are often rather diverse enterprises in which household members are likely to be comparatively less informed of the overall household activities as compared to the smaller, 'nuclear' household in the developed country context. As such, the use of survey responses by multiple household members on a given household activity are unlikely to differ by only a pure classical 'measurement' or response error. We instead construct a statistical framework that allows for a behavioral component of mis-reporting that allows the multiple reports to differ by more than just a classical measurement error term, but also due to asymmetric information regarding household activities, for example.

JEL Classification:

Keywords: Poverty Measurement; Vulnerability; Measurement Error Models; Survey Design

1 Introduction

The measurement of poverty and inequality indices has generated a large conceptual and empirical literature in economics. Recent surveys can be found in Atkinson and Bourguignon (1999) and Fields (2001). Since the summary of an entire distribution of consumption, income, or wealth into a scalar measure necessarily involves some explicit or implicit value-weights, the theoretical literature has focused on the axioms needed to rationalize any particular poverty or inequality index measure. Estimating these point-in-time or *stock* measures of poverty is then generally rather straightforward, and beyond interest in such indices in their own right, researchers have related the change in such indices over time to economic growth and similar factors. A related branch of this literature has looked at the decomposition of the inequality of income by its components and tracked the sources of changes in inequality via that method. However, even studies that track the changes in inequality or poverty indices over time for a given country generally only require repeated cross-sections of similar individuals. In this sense they do not rely on characterizing the underlying income or consumption process for the individual household across time, and thus are not *dynamic* in this microeconomic sense.

However, a more recent branch of the poverty and inequality literature *has* begun to utilize the many emerging sources of panel data on households for developing countries. An important concept related to a point-in-time poverty measure is how exposed or 'vulnerable' a household is to poverty tomorrow even if it is not currently in poverty today (see Morduch (1994) and Pritchett, Suryahadi, and Sumarto (2000) for definitions and related discussions). The *persistence* of poverty - or the lack thereof - has important implications for how policies should be structured to reduce the negative consequences of poverty. However, once the focus shifts away from stock measures of poverty to flow measures such as vulnerability, empirical issues such as measurement error shift from second order complications to first order importance. Furthermore, data from developing countries on income and consumption are notoriously noisy indicators of their true values, which is all the more reason to carefully consider their influence on the inferences regarding poverty dynamics from a given dataset. This means that it is crucial to distinguish between observed or measured persistence versus the persistence in the true process, and in tracking the individual household movements over time, the measurement error can have a dramatic effect.

The problem is that while panel data are becoming increasingly available for developing countries, surveys which contain redundant measures so that the character and degree of measurement error remain uncommon. While classical measurement error that is *by assumption* independent across time makes any true underlying process appear to be less persistent than it truly is, the challenge is the empirical distinction between measurement errors, income and consumption shocks, and the relative variance that is due to the various components in the observed processes. In this paper we make use of the cross reporting scheme often used in household surveys to calculate the 'reliability' of various survey responses. We show that due to the decentralized nature of households in developing countries, the cross reports may diverge for reasons other than pure classical measurement error. We consider a simple modification of the classical model to allow for 'hidden' consumption, which has a zero reliability from the cross reports due to the asymmetric information among household residents on these components of overall consumption. The resulting model, which allows for private and 'public' or household-level consumption, appears to fit the framework of the classical measurement error as compared to the case where all aspects of individual consumptions are pooled. The model then allows us to estimate the reliabilities on the public household consumption choices, and thus infer the true dynamics of household poverty from the measured dynamics. Using our results and our econometric structure, we then return to the issue of household survey design. We discuss briefly how future fieldwork can design the survey instrument, thus allowing for flexibility in the specification of the measurement error model used to explain the information contained in cross-reporting, while accounting for special circumstances imposed by the cultural aspects of household structure.

In this paper we examine these issues using survey data on consumption from a household-level survey from Ghana described in earlier work by Goldstein and Udry (1999). The survey is novel in that for three of the rounds of the survey husbands and wives were asked not only of their own consumption, but also on the consumptions of their spouses. This survey design allows us to correct for the presence of measurement error under various assumptions about the reporting errors. The panel aspect of the survey allows us to describe both point-in-time measures of poverty as well as poverty dynamics while correcting for the confounding effects of measurement error. Furthermore, the richness of the data allows us to test the adequacy of the classical measurement error model to capture the discrepancies in the cross reports. Due to the decentralized nature of the southern Ghanaian household structure (see e.g. Oppong (1974)), we find that the pure classical measurement error model is rejected in favor of a model that allows some components of consumption to be 'private' in nature, and thus essentially unobserved in the cross reports. This modification to the standard measurement error model shows that the data are not as error-ridden as the initial analysis would suggest, and that under assumptions of homogeneity in the response error processes, we can control for both components of individual consumptions that are 'private' as well as for classical response error in the public (i.e. observable to the household) consumption component. These corrections have a large impact on the degree of poverty transitions, and thus on the implied degree of vulnerability.

In the next section of the paper we review some of the earlier literature on the impact of measurement error on transitions in discrete and continuous outcomes. We then review the literature dealing with earlier evidence on the structure of Ghanaian households, from which we then describe our specific household survey. Section three starts with the standard classical measurement error model to describe the discrepancies in cross reports on some outcome. We review the testable implications of that model, and then use this preliminary discussion to construct a richer framework that allows the cross reports to differ in an asymmetric fashion, due to components of consumption that are potentially private to the individual. We show how this richer model can be identified and tested. Section four discusses the impact of the estimates of the structural parameters of the observed consumption processes for measured transitions in and out of poverty. Section five concludes, and includes a brief discussion of how our work can aid in the design of future surveys where measurement error and/or private information is a concern in the context of cross-reporting strategies.

2 Summary of the Earlier Literatures and Data Description

In this section we review the prior work on measuring and controlling for the influence of measurement error on the observed dynamics of employment and income processes. We then turn to the prior literature on household structure from Ghana as a context that leads us to consider richer response error frameworks to account for the discrepancies in household consumption for Ghanaian husbands and wives. We conclude this section by discussing some of the relevant details from the household panel survey we use from Ghana in the 1990's.

2.1 The Influence of Measurement Error on Labor Market Dynamics: The U.S. Literature

While the measurement of income, consumption, and poverty dynamics in developing countries is becoming increasingly common with the availability of more and more panel data sets from such countries, the development literature has yet to confront the issues considered by labor economists in the 1970's and 1980's. In the United States, the availability of 'validation surveys' and related survey redundancies allowed for researchers to empirically quantify the importance of measurement error on observed, as opposed to true, state transitions. The intuition that a serially uncorrelated measurement error added to a rather persistent process such as income or consumption would make the observed series appear much more volatile was understood well in advance of the availability of the data needed to estimate the actual effects, and when researchers did so, they often found dramatic impacts on the implied volatility of unemployment or lifetime income. We review two of these literatures here.

Early work in the U.S. by Clark and Summers (1979) on labor market transitions challenged the view of the unemployment in the U.S. as being due short-run churning as workers sought out other jobs. They conjectured the empirical work that gave rise to this view in the early 1970's was due to measurement error in the "gross flows data" which gave the appearance of a large volume of short run spells of unemployment, as opposed to a small fraction of spells characterized by long unemployment durations. Subsequent work by Poterba and Summers (1986) and Abowd and Zellner (1985) provided empirical content for this view by using re-interview (or validation) samples from the CPS to correct for spurious transitions in the flows data. Poterba and Summers (1986) in particular, found that the measurement error corrected unemployment transition matrices implied that the uncorrected unemployment durations were understated by as much as *eighty percent*.

Another example of the empirical importance of measurement error on the observed volatility of the U.S. economy comes from the study of intergenerational income mobility. Specifically, researchers were interested in correlating the permanent income of fathers and sons, which was measured with error. Initial work by Sewell and Hauser (1975) and Behrman and Taubman (1985) on the inter-generational correlation in incomes of fathers and sons found it to be quite low - on the order of 0.2. However, a measurement error component in *observed* earnings that is less persistent than the *true* earnings process will tend to produce an attenuated estimate of the true correlation. Indeed, Goldberger (1989) cautioned that measurement error in earnings measures would bias this measure downward. Consistent with Goldberger's warning, Solon (1989) found that the measurement error corrected estimate of this correlation rose to 0.4, implying far less intergenerational mobility than was previously believed. Zimmerman (1992) replicated these results, and performed a number of robustness checks, adding weight to the conclusion of limited intergenerational mobility in modern U.S. data. The literature now implies significantly less intergenerational mobility than the early raw tabulations suggested.

In contrast to the U.S. literature, however, the development literature has given little attention to the role of measurement error in poverty dynamics.[4] Whether the *conclusions* of the U.S. literatures are generalizable to the developing country setting is an empirical question (and likely doubtful): it depends on the relative persistence in the measurement error process to the persistence in the true underlying process. But the *methods* and empirical approaches to control for the effects of measurement error in these contexts are certainly generalizable. A remaining issue, however, is whether the assumption of classical measurement error, which was widely used in the above literatures to character-

[4] Glewwe (2002) is a recent exception we found while writing this paper. But his approach relies only on conventional instrumental variables methods together with the assumptions of classical measurement error, as opposed to validation surveys or similar survey redundancies, to estimate the importance of measurement error. Fields' (2001) recent book surveying poverty measurement and dynamics makes no mention of the role of measurement error in his text.

ize the discrepancies in duals reports on a single variable (see Ashenfelter and Krueger (1994) for a recent example), is amenable to describe the dual reports on household consumption in our sample of Ghanaian households. We review the earlier literature on this topic next.

2.2 Anthropological Evidence on the Structure of Households in Southern Ghana

The potential pitfall with a pure classical measurement error framework to describe the dual reports on household consumption from husbands and wives in our data is that the framework assumes that both parties are equally well informed on household activities. That this assumption may well be falsified in cultures where the household is more decentralized than the typical 'nuclear' household in the U.S. is a topic to which we turn now. The data for this paper come from a household survey conducted over the two year period from November 1996 to October 1998 in the Akwapim South District of the Eastern Region of Ghana. Given the distinct differences across regions and cultures in Ghana, by no means do we wish to generalize our findings to an area beyond the sampling frame for the survey. In particular, the area we are studying here is composed primarily of Akwapim Akan, a distinct ethnolinguistic group. One aspect of the division of household activities in Ghana that has been previously noted in the anthropology literature is the separate 'spheres' or economies kept by husbands and wives in areas of Ghana. For example, Vercruijsse, et. al. (1974) noted in their their study of the coastal Fante communities in Ghana that "...women are economically active in their own right as much as the men are and that this is not affected by being married and having children. Accordingly their income does not have the character of a supplement and cannot even be conceived as being part of the 'family income' " (p. 36).

This same pattern for the division of household activities for the region covered by our data appears to also apply. Oppong (1974), for example, observed for the broader Akan norm that, "according to custom the Akan husband and wife do not own, manage, or inherit together any exclusive or substantial property of their own" (p. 328). She finds (in her sample of civil servant couples) that "more than twice as many husbands own property together with their kin, as with their wives, and fewer than one in ten couples have joint accounts...The new urban norm thus follows the traditional pattern to some extent in that responsibility for day to day maintenance of the family seems to be shared by most husbands and wives, while the majority maintain separate financial arrangements for spending, owning and saving" (p. 329-30). Thus, even in urban areas, we might expect husbands and wives to have incomplete information about each other's income and expenditure. Indeed, in some earlier work with the data used in this paper, economists have noted the distinct and separate networks used by husbands and wives in coping with household risk (Goldstein (1999); Goldstein, de Janvry and Sadoulet (2001)).

Because of these prior pieces of evidence, as well as the rejection of the pure classical measurement error model to describe our data, we need to consider a modified response error model that allows for some components of consumption to be 'private' for individual household members. In the case of such 'hidden' components of consumption cross-reporting will provide no information on the true value of such components, by definition. Furthermore, conventional estimates of the reliability of overall household consumption will be too small the larger is the relative share of private consumption in overall household consumption. This insight implies it is crucial to ask the key members of the household on their own consumption so as to capture the consumption that is private to these individuals. In contrast to classical measurement error, this source of response error will tend to *understate* the true economic status of the household in level terms, although its influence on poverty dynamics remains unclear *a priori*. We now provide a brief introduction to the survey data we use.

2.3 Description of the Household Data From Ghana

In the late 1990's, one of us was involved in fieldwork that collected data from four village clusters from the Akwapim district of (southern) Ghana. These villages were selected due to their varying degree of market integration and diverse cropping patterns. The primary income earning activity of the residents of these villages is agriculture, both in food crops (mainly maize and cassava) and export crops (pineapple). However, given the proximity of two of these villages to larger towns, as well as intra-village commerce, a significant number of the respondents in the survey also engage in non-farm income earning activities. Information on the data and questionnaires (as well as links to papers which have previously used these data) are available at *http://www.econ.yale.edu// udry/ghanadata.html*.

Within each village cluster, 60 married couples or triples were selected at random for the survey.[5] Men and women were interviewed separately, by an enumerator of the same gender. The survey was conducted in 15 rounds, each round being about 4 to 6 weeks apart. A core set of agricultural questionnaires were asked each round, complemented by a rotating set of modules. In this paper, we our focus is on the two expenditure questionnaires that were administered three times during the course of the two years. One questionnaire asked about food consumed from own production, while the second asked about purchased food.[6] Two of these questionnaires (round 4 and round 12) were

[5]We exclude the polygamous households (about 5 to 10 percent of the sample) for data quality reasons, as well as the observation that such households are likely to be secularly different in their household organization.

[6]While a third questionnaire asked about non-food expenditure, we do not use that data

administered at the same point in the year during 1997 and 1998 respectively, with the third (round 8) fell in the middle of these two. For the purpose of this paper, we use only respondents where both the man and the woman responded to the expenditure questionnaire, thus providing a matched pair.

The unique feature of the survey for the purposes of this paper, in addition to its panel structure, is that it asked each respondent to report on their own expenditure, the expenditure of their spouse, and the expenditure of any other person in the household that was used for household consumption.[7] We will focus on what we call the 'own' and spouse (or 'cross') reports, as the expenditure by other household members was limited and it also showed a significantly larger divergence as reported by the spouses. Thus, this array of own and cross reports provide us with three potential measures of total household expenditure:

1. Women's reports (WR). These consist of the female report of her own expenditure plus her report of the expenditure by her husband. We have this data for rounds 4, 8 and 12.

2. Men's reports (MR). These consist of the male report of his own expenditure plus his report of the expenditure of his wife. Due to the fact that the men were initially reluctant to even guess at their spouse's expenditure, we asked this question only for round 12.

3. Own reports (OR). These are comprised of the woman's report of her own expenditure plus the man's report of his own expenditure. We have these reports for rounds 4, 8 and 12.

Table 1 shows the total household food expenditure per month (purchased food plus food from own farm), by village for each round. On the whole this is a poor area, average household expenditure on food is roughly 200,000 to 300,000 cedis.[8] As part of our work on these data, we examined each village and disaggregated by purchased food and food consumed from own farms. When we disaggregate these by purchased and own farm food, the reports in village 4 seem to be driven by abnormally high women's reports of own farm food in round 12 and by generally low male reports in village 1 for round 4. These deviations are probably due to enumerator effects - in village 4 the round 12 female enumerator was new to the job and in village 1 the male enumerator initially did not do a through job of collecting expenditure data. Thus, in work we do below, particular the Round 12 data, we disaggregate by village and sometimes drop village 4 from the analysis. We have also excluded a small number of households from various rounds because of extreme values, which

here. The reason is that the non-food expenditure is less comparable across households, as well as being much sparser.

[7]The survey conducted and used by Ashenfelter and Krueger (1994) in their work on education and earnings in twins also used exactly this survey structure, and is what motivated this component of the survey. In their context, the 'cross' and 'own' reports obtained on each twin's level of education were treated as two reports on one variable (the education level of that twin) that were hypothesized to differ by two independent and classical measurement errors. We elaborate on this model in the next section.

[8]The average value of the Ghanaian Cedi during this time was around 2000 to the dollar.

were compared to the written questionnaire.

The total food reports broken out by own farm and purchased food are shown in Table 2 for Round 12. In purchased food, men and women's reports for rounds 12 and 8 differ from the own reports by about 25 percent. Food consumed from own farm shows a marked difference by gender. In round 12, men's reports are about 15 to 20 percent different from the own report but women's reports are 90 percent smaller than the own report, with a similar gap in round 4 (not shown). However, the round 8 gap (also not shown) between the women's and own reports is much smaller - the woman's report is about 43 percent lower. The difference in own and women's reports may be due to the fact that rounds 4 and 12 are much closer to harvest times and since men have a larger area under cultivation than women, they may be reporting more accurately the harvest collected by them from their own farms.

3 A Conceptual Framework for the Response Errors in the Dual Reports on Household Expenditure

The primary goal of this paper is simply to measure the 'reliability' in the household consumption data using our survey data from Ghana. At the simplest level, this would involve simply correlating the male and female reports on total household consumption under the assumption of independent reporting errors. But as we show in this section, the cross-reported data allow us to *test* the collection of the "classical measurement error" assumptions when we have available the cross-reported data as described in the previous section. We find that in fact these assumptions are overly strong in trying to fit them to our data, and the overidentifying restrictions of the classical framework are rejected. However, in investigating the reasons for the rejection, we find that a simple modification of the pure classical measurement error framework describes the data rather well. In particular, we find that allowing for 'public' and 'private' food consumption goods in the household yields the flexibility to capture the poor performance of the cross-reports on some goods. In particular, we posit that the cross-reports on 'private' goods contain essentially no information, as these are goods which are consumed outside of the household. However, we show that the cross-reporting survey design is still fruitful to eliminate measurement error in the public and private goods via a method of moments scheme. Thus a reliability measure can be obtained for both components of food consumption, as well as for household consumption as a whole.

3.1 Some Preliminaries: Using The Classical Measurement Error Model to Explain the Dual Household Reports

The purpose of asking more than one sample respondent to report on the same quantity - years of education or individual consumption, for example - is that when the response errors are assumed to be independent across individuals, the simple covariance of the dual responses will recover the variance in the *true* quantity that is being reported on. Thus if the response errors and process generating the true data are homogeneous across people, the parameters generating both the observed and the unobserved true processes can be estimated. An example of 'cross-reporting', and how it can be used in a regression context, can be found in the earnings and education study by Ashenfelter and Krueger (1994). They used the differences in education and earnings between twins to purge education choices of common family and genetic components to provide additional evidence of the causal effect of education on earnings in the U.S. However, differencing can exaggerate the attenuating effects of measurement error, and thus they included in their survey of twins questions that elicited the *cross reporting* of one twin on the other twin's schooling, in addition to the usual own report of schooling. The assumption that the response errors are mean zero and *iid* across twins in this setting - as education is a stock as opposed to a flow variable and tends to be fairly 'visible' as it is measured in years of time spent acquiring it - seems to be reasonable, and so we begin our discussion with this framework as a benchmark.

To establish notation and concepts, it is useful to begin with a discussion of the classical measurement error (CME) model applied to the setting where dual reports on an outcome y are available. Let the *observed* reports on the outcome y for person i given by individual j be generated by:

$$y_i^j = y_i^* + v_i^j \tag{1}$$

where y_i^* is individual i's *true* outcome, and v_i^j is the reporting error made by respondent j on i's outcome. We assume we also have available individual i's own-report on his own outcome, and this is denoted as y_i^i. In the general case, individual i reports his own outcome with error as well, so that both y_i^j and y_i^i represent error-ridden measures of the true outcome y_i^*. Under the classical measurement error assumptions the response errors are assumed to be uncorrelated with the true values and with each other. The virtue of these assumptions is that the covariance of the observed measures across respondent pairs (i, j) yields the variance in the latent y_i^*:

$$Cov(y_i^j, y_i^i) = Var(y_i^*) \tag{2}$$

A related quantity that measures the fraction of the variance in the observed own report that is due to variation in the true outcome is termed the reliability ratio, often designated by λ:

$$\lambda = \frac{Cov(y_i^j, y_i^i)}{Var(y_i^i)} = \frac{Var(y_i^*)}{Var(y_i^*) + Var(v_i^i)} \tag{3}$$

The reliability ratio is bounded between 0 and 1, and attains the upper bound of 1 only when no response error in the own report is present. It attains the lower bound of zero only when there is no variation in the true outcome and/or the variance in the response error goes to infinity.

The components of measurement error models, as discussion in the previous paragraph indicates, are most readily identified via the variance-covariance matrix of the relevant variables. Looking at the identification problem in this way also makes clear what assumptions of the model are potentially over-identified, and thus which could be tested, and therefore weakened if such tests reject. For example, consider the classical measurement error model from above, but with the added assumption that the response errors in the own and cross reports have the same variance. Under this assumption the variance-covariance matrix of the responses (y_i^j, y_i^i) would then be:

$$\begin{bmatrix} \sigma_{y^*}^2 + \sigma_{v_{ij}}^2 & \sigma_{y^*}^2 \\ \sigma_{y^*}^2 & \sigma_{y^*}^2 + \sigma_{v_{ii}}^2 \end{bmatrix}$$

if the own and cross-reports had the same response error variance, then we could further impose the restriction that $\sigma_{v_{ij}}^2 = \sigma_{v_{ii}}^2$ although it is perhaps more reasonable to allow that the cross report is noiser than the own report, and thus that $\sigma_{v_{ij}}^2 > \sigma_{v_{ii}}^2$. In this latter case, we have 3 structural parameters $\sigma_{y^*}^2$, $\sigma_{v_{ij}}^2$, and $\sigma_{v_{ii}}^2$, and the empirical version of this 2×2 variance covariance matrix will have 3 unique elements, and so the parameters are exactly identified. This implies the resitriction that $\sigma_{v_{ij}}^2 = \sigma_{v_{ii}}^2$ is thus testable, as there is 1 extra degree of freedom in this case.

There are additional resictions implied by the classical measurement error model on empirical quantities for the means of the variables as well. In particular,

$$E[y_i^j - y_i^i] = 0 \tag{4}$$

and under the independence assumption of the response errors

$$Var[y_i^j - y_i^i] = \sigma_{v_{ij}}^2 + \sigma_{v_{ii}}^2 \tag{5}$$

The implication being that a plot of the difference of the cross report and the own report should be symmetrically distributed around 0 (under the assumption that the difference in the response errors is symmetrically distributed) with a variance that is the sum of the variances in the two response errors. Another way to summarize the same argument is to note that the plot of the data of y_i^j versus y_i^i is that it should be symmetrically distributed around the 45° line, but with possibly a larger variance on the axis relating to y_i^j due to the potentially greater variance in the response error of the cross report as compared to the own report. This plot allows for a convenient visual check on some of the assumptions of the classical measurement error model when cross reports are available.

3.2 Testing the Classical Measurement Error Model Using the Ghanaian Survey Data

The object of interest for the measurement of poverty in the Ghanaian data is household-level consumption. As we indicated in the initial discussion of the data in Section 2, we have a number of ways to construct this household-level measure, given the various cross reports in the data. Here we establish the notation to tie the specifics of our data to the general discussion of the classical measurement error (CME) model discussed above. Consider first the expression for total household food consumption, as reported by the Female:

$$y_{HH}^F \equiv y_F^F + y_M^F \tag{6}$$

Given the survey scheme on the cross-reporting, this measure on household food consumption is available in all three of the rounds 4, 8, and 12. In Round 12 we can also construct the Male report on household food consumption via:

$$y_{HH}^M \equiv y_F^M + y_M^M \tag{7}$$

Finally, we can also have available in all three rounds the "Own" report on household food consumption, where the Female and Male reports on their own contributions to household food consumption are summed, and no cross-reports are used:

$$y_{HH}^O \equiv y_F^F + y_M^M \tag{8}$$

Under the CME assumptions, we can use the correlation between the Male and Female reports of total household food consumption to estimate the reliability ratio in Round 12 of the data. Using the plot of y_{HH}^M versus y_{HH}^F we can thus test the CME model based on the discussion of the previous subsection. Since the discussion of the previous sub-section pertained directly to the case of a single outcome and cross report, we briefly relate that discussion to the precise case of our data. Note that for spouse j, their report on *household* consumption is an error-ridden version of true household consumption.

$$y_{HH}^j = y_{HH}^* + v_i^j + v_j^j \tag{9}$$

for $j = M, F$ and $i = F, M$. Thus even in the case where the cross-reports exhibit greater variance than the own-reports (i.e. they are less 'reliable'), as long as the distributions from which they are drawn are homogeneous across

Male and Female, the CME framework implies that the reports should be symmetrically distributed around the true household food consumptions y^*_{HH}:

$$E[y^j_{HH}] = y^*_{HH} + E[v^j_i] + E[v^j_j] = y^*_{HH} + 0 + 0 \tag{10}$$

(where we take expectations with respect to the probability distributions of the reporting errors, as our focus for the moment is on a *given* household, and the thought experiment is of repeated draws on the response errors for that household). Similarly, the implications for the variance of the distribution in the reports is:

$$Var[y^j_{HH}] = y^*_{HH} + Var[v^j_i] + Var[v^j_j] \tag{11}$$

where we have invoked the simplifying assumption of independence in own and cross-reported measurement errors even though they are both given by person j. This just assumes the response errors are generated by independent draws from a given distribution, and so rules out, for example, a person-specific effect in the response error mechanism. We revisit this assumption later.

As long as $Var[v^F_M] = Var[v^M_F]$ (i.e. the dispersions in the cross reports are the same for both Males and Females), and the response errors are additive and not proportional (an issue we come to), then as discussed in the previous subsection, the CME model implies the plot of y^M_{HH} versus y^F_{HH} should be symmetrically distributed around the 45^o line. If there were no response errors, then the data points would line up exactly on the 45^o line. Figure 1 shows this plot using our Round 12 data. this figure appears to show that the CME assumptions are roughly met. However, when we break this plot out by village (to account for the possibility of the Village 4 being corrupt for reasons discussed above), it is visually apparent that both the common mean and the common variance assumption implied by the CME assumptions are violated in villages 1 to 3. Figure 3 shows this more starkly by pooling across the three villages. In particular, the Female reports appear to have a much lower mean as compared to the mean of the Male reports, and the Female reports also exhibit far less dispersion than the Male reports. The implication of this plot is that the measured incidence of poverty in these data will depend greatly on which person's reports we opt to use. Furthermore, from a methodological point of view, the failure of the CME assumptions implies that the estimated reliability ratio based on the correlation between these two reports will be a misguided estimate of the true reliability in the data. We need to examine the underlying components of household consumption to understand the source of the failure in the CME assumptions, and to construct an alternative statistical and behavioral framework to explain the discrepancies in the two reports. Using the prior anthropological evidence of Oppong (1974) and others, we turn next to an extended CME framework that allows for some components of consumption to be 'private' and thus unobserved by the cross-responding spouse.

3.3 Private Information and An Alternative to The Classical Measurement Error Framework

The larger variance in the men's reports of household food consumption versus the women's reports, along with the higher mean in the men's reports, suggests a number of alternative hypotheses that involves some form of behavioral interaction in the surveying process. While this finding may appear in other datasets and in our country contexts, we do not want to create the impression that the precise reasons we uncover here for the Ghanaian data will generalize to these other contexts. But the overall logic of breaking down the components of household food consumption *will* likely be fruitful in establishing alternative frameworks to the CME framework for particular country contexts to explain the discrepancies in the dual reports.

As the earlier rounds of the Goldstein and Udry (1999) fieldwork suggested that Men were unwilling to even guess as to the Female's contributions to household food consumption, we used this as a basis to formulate our first alternative hypothesis to the CME framework. In this model, we thought of Men's cross-reports on Female consumption as a 'noised-up' version of her own consumption. This would explain the greater dispersion in the Men's household level reports, and the higher mean could be explained by the Male respondents wishing to enhance their social standing to the interviewer by inflating their own (admittedly poor) guesses on the cross reports of her food consumption. However, while this hypothesis with the household level data, it has direct implications for the Female's own and cross reports in particular, y_F^F and y_F^M. If this hypothesis were true, the right skewing in the Male reports versus the Female reports should manifest themselves in these components of household food consumption. The scatter plots analogous to those presented above, here broken out by village, are presented in Figure (missing from this draft). If anything, these results appear to the eye to adhere much better to the CME framework than the household level plot. Thus, there appears to be little support for this 'Males are less informed as to the food consumption sphere' hypothesis, at least when confronted with this more direct test.

However, this test led us to consider the dual reports on the Male consumption across households, y_M^M and y_M^F. The results of this plot are shown in Figure 4 (which drops Village 4 for the reasons discussed above). The similarity in the skew in the men's own report y_M^M to the skew in the men's household level report y_{HH}^M are striking. This is clearly the source of the right skew in y_{HH}^M versus y_{HH}^F. While this could easily be explained by departing from the assumed homogeneity in the parameters governing the response error distributions by gender, we sought to first disaggregate *these* components by food item to find a hypothesis that was not purely tautological. This proved to be highly useful, as we found that the cross-reports on certain food items contained essentially no information. Indeed, in many cases, the cross-reports were simply 0, indicating no information.[9] These overall patterns, both pooled and disaggregated by village, are shown in Figures 5, 6, 7, and 8 for the earlier Rounds 8 and 4.

This phenomenon was especially pervasive in the consumption of off-farm 'prepared' foods (i.e. alcoholic beverages and prepared foods), to which men devoted a larger share of their household food contributions than did women. As these food items are very easy to conceal, or simply be difficult to observe for the spouse, we posited the existence of 'private' goods in the household for which the cross-reports alone provide no information, but for which the own reports still contain a (classical) measurement error. For notation, let yr denote consumption of 'private' goods, and so we posit that the own and cross reports adhere to:

$$yr_i^i = yr_i^* + v_i^i \tag{12}$$

and

$$yr_i^j = 0 \tag{13}$$

where the '0' in the cross-report is meant to denote the absence of information. Equivalently we could write that this response is simply a free noise term, although nothing of interest is changed - basically it should not co-vary with anything of interest. We then return to the CME framework to posit that it captures the cross reports in the remaining 'public' household consumption goods, denoted as yb, as follows:

$$yb_i^i = yb_i^* + v_i^i \tag{14}$$

and

$$yb_i^j = yb_i^* + v_i^j \tag{15}$$

We have retained one potentially strong assumption in writing down this framework that will prove important in our identification strategy below, and that is that the own-report response errors, while not identical realizations, *are* assumed to be drawn from the same distribution, and are thus assumed to have common variance for a particular gender group.

Thus, in a sense we have 'resurrected' the CME framework, modifying it only to fit both evident patterns in our own data, as well as motivated by the much earlier anthropological evidence of Oppong (1974) among others, who characterizes the southern Ghanaian households as enterprises rather distinct along gender lines. Clearly the cross-reports for the private goods *on their own* do nothing to allow us to estimate the reliability in the private good responses. By contrast, if the 'public' household food items are themselves the direct interest of poverty and well-being studies, the reliability of those responses can be calculated via the conventional means under the CME framework as:

$$\lambda_b = Corr(yb_{HH}^F, yb_{HH}^M) \tag{16}$$

[9] How to treat the zeros, as information in and of themselves, or as missing, is an issue we come to below when we consider the response error model in levels versus logs.

But the proportion of household goods that are 'public' and 'private' are themselves an intra-household choice variable, and so the focus on say λ_b needs to take account that a poverty measure based on 'public' household food items is a mixture of constraint and choice decisions across households. Our intent here is to abstract from such choices, but instead to keep our focus on deriving an estimate of the reliability in total household food consumption even when the presence of 'private' goods appears to undermine the usefulness of the cross-reporting survey design to quantify the importance of measurement error.

As is typical in measurement error (or latent variable) models, the structural parameters governing the model are most readily identified via the covariance implications. For the Round 12 data, we have the 4 own reports on public and private consumption for the Male and Female in each household, $(yb_F^F, yr_F^F, yb_M^M, yr_M^M)$. We also have the 4 cross-reports for each of these measures: $(yb_F^M, yr_F^M, yb_M^F, yr_M^F)$. In all we wish to use the modified CME framework described above, wherein the cross-reports on the private consumptions represent uncorrelated noise terms, to explain the variance-covariance matrix of the 8 element vector: $(yb_M^M, yr_M^M, yb_M^F, yr_M^F, yb_F^F, yr_F^F, yb_F^M, yr_F^M)$. Notice that our modified CME model nests the pure CME framework as a special case, as under the pure CME framework, the delineation into public and private goods is superfluous and the cross-reports on the private goods should behave no differently than the cross-reports on the public goods. Under our model, the variance-covariance matrix of this 8 element vector is:

$$\begin{bmatrix}
\sigma^2_{Mb}+\sigma^2_{Mv,M} & & & & & & & \\
\sigma_{Mbr} & \sigma^2_{Mr}+\sigma^2_{Mv,M} & & & & & & \\
\sigma^2_{Mb} & \sigma_{Mbr} & \sigma^2_{Mb}+\sigma^2_{Mv,F} & & & & & \\
0 & 0 & 0 & \sigma^2_{Mr,F} & & & & \\
\sigma_{Mb,Fb} & \sigma_{Mr,Fb} & \sigma_{Mb,Fb} & 0 & \sigma^2_{Fb}+\sigma^2_{Fv,F} & & & \\
\sigma_{Mb,Fr} & \sigma_{Mr,Fr} & \sigma_{Mb,Fr} & 0 & \sigma_{Fbr} & \sigma^2_{Fr}+\sigma^2_{Fv,F} & & \\
\sigma_{Mb,Fb} & \sigma_{Mr,Fb} & \sigma_{Mb,Fb} & 0 & \sigma^2_{Fb} & \sigma_{Fbr} & \sigma^2_{Fb}+\sigma^2_{Fv,M} & \\
0 & 0 & 0 & 0 & 0 & 0 & 0 & \sigma^2_{Fr,M}
\end{bmatrix}$$

The notation used in this matrix, particularly the subscripts, needs some explanation. In the empirical version of this 8 by 8 matrix, there will be 36 free elements. Our modified CME framework imposes 13 zero restrictions for terms involving the cross-reports on the private food consumptions. Of the remaining 23 elements, they are characterized by a total of 15 structural parameters. Thus, while the modified CME framework as a whole places 21 overidentifying restrictions, the actual degrees of freedom in estimating the structural parameters is 8. It should be visually apparent that all of the structural parameters are identified.

As above, the notational scheme continues to use M and F to refer to Male and Female, and b and r to refer to public and private respectively. Thus, the response error terms like $\sigma^2_{Mv,M}$ refers to the variance in the Male's own report response error. Notice that it is our assumption that the draws on the response errors for a given spouse's public and private reports are drawn from a common variance distribution that allows us to then isolate the variance in the *true* private expenditure responses denoted as σ^2_{Mr}. Similarly, the variance in the true public expenditure reports for Females, say, is given by σ^2_{Fb}, while the covariance between the true public and private consumption is given by σ_{Fbr}. The *cross* report response error variance for Men reporting on Female, for example, is given by $\sigma^2_{Fv,M}$, as we use the indicator after the comma to denote who is giving the report. Finally, two terms that are of no inherent interest for our exercise, but which we treat as essentially 'free' terms, are the cross-reports on the *private* consumptions, which we denote as $\sigma^2_{Mr,F}$ and $\sigma^2_{Fr,M}$. Even if these cross-reports on the private consumption contain no information, and so are uncorrelated with the other reports (hence the rows and column of the zero restrictions), they will have non-zero variance unrelated to the other structural parameters governing the matrix.[10]

The identification of the structural variance components allows us to 'reconstruct' the reliability ratio for total household consumption. The problem with using say $Cov(y^F_{HH}, y^M_{HH})$ to deliver the variance in the true household level consumption is that the two private consumption variance terms, σ^2_{Mr} and σ^2_{Fr}, are not identified. To see this, note that:

$$\begin{aligned} & Cov(y^F_{HH}, y^M_{HH}) = Cov(yb^F_F + yr^F_F + yb^F_M + yr^F_M, yb^M_M + yr^M_M + yb^M_F + yr^M_F) \\ = \quad & \sigma^2_{Mb} + \sigma^2_{Fb} + 2\sigma_{Mb,Fb} + \sigma_{Mbr} + \sigma_{Fbr} + \sigma_{Mb,Fr} + \sigma_{Mr,Fb} + \sigma_{Mr,Fr} \end{aligned} \tag{17}$$

The problem is that if we use an estimator such as $Corr(y^F_{HH}, y^M_{HH})$ to estimate the household level reliability ratio, the denominator *will* involve the private consumption variance terms σ^2_{Mr} and σ^2_{Fr} (in addition to the noise terms), while the numerator does not. Thus, this naive estimator of the reliability ratio based on the *pure* CME framework will be biased down, and the degree of attenuation will be larger the greater is the fraction of the overall variance in household consumption that is private. In essence, using the cross reports in the pure CME framework pushes the private consumption into the error term (as it is uncorrelated in the cross-reports), and so implies too low of a reliability in the survey data when in fact it may just be a manifestation of somewhat separate household 'spheres' along gender lines. Our conceptual framework here shows that such household divisions have the potential to be quite important in the measurement of poverty, both in its level form (women's reports are on average smaller than men's) and in its reliability (which, as we discuss below, plays a highly important role in the measurement of poverty dynamics).

[10] A final important restriction we have made at this point is the assumption of uncorrelated draws in the response errors *even* when the same person is reporting on say their own public and private consumption. We could allow for a person-specific component to the measurement error, as in the final model presented in Ashenfelter and Krueger (1994), for example, but have not investigated this alternative model for now.

4 Measuring the Reliability in an Estimated Poverty Line

The discussion in this section is incomplete for this draft. What follows is a preliminary set of notes, and the numbers in the text may not correspond to the numbers in the tables for this draft. An examination of the incidence of poverty highlights the problems with using a single person's report of household expenditure. In order to illustrate this, we created an arbitrary poverty line of 80 cents per person per day. At the average rate of exchange during the survey, this is approximately 48,000 cedis per capita per month. Using this line, and the expenditure per individual by each household, we can compute the incidence of poverty for each report in each round, using the Foster-Greer-Thorbecke class of poverty measures. The women's reports indicate a striking degree of poverty for every round - ranging from 77 percent of the individuals in round 4 to 56 percent in round 12 . [Add discussion here about the trend - right now it shows an increase from 4-8 and then a sharp drop in round 12, perhaps due in part to the dual reporting strategy used in Round 12.] See Tables 3 through 6 for tabulations on what we discuss below. In addition, see Figures 9 and 10 for the kernel density plots of the empirical consumption distributions using the men's and women's reports (the vertical line is the poverty line we decided on). The two Figures show this empirical distribution with and without using the Village 4 data. Clearly using the Female report leads to a larger measure of poverty incidence.

The own reports indicate a significantly lower level of poverty. In round 4, the own reports yield a poverty level of 44 percent, this number jumps to 54 percent in round 8 and drops back to 42 percent in round 12. Comparing this to the women's reports indicates that the use of a single person report may overstate poverty by 11 to 32 percent. The question of who to ask in single person reports is an issue that has received significant attention in the literature on expenditure measurement (see the World Bank compendium) and we shall discuss this issue further in the next draft.

An important dimension of these differences in the aggregate poverty figures is the fact that the poverty status of any given household may shift based on which report we use. What is striking is that 30 percent of the households change their poverty status based on whether we use the husband or the wife's report of total expenditure. Examining the comparison between the own report and the women and men's reports highlights the difference discussed above. An average of 29 percent of the households change status when we compare the woman's report with the own report, and 13 percent of the households change status when we compare the male report and the own report.

This section illustrates the high level of sensitivity of traditional poverty measurement techniques to whom in the household is asked. Our results indicate that the own report of household expenditure yields a significantly higher level of household expenditure and hence provides a lower level of poverty than conventional measures. Furthermore, single person reports also show significant variation - reports by men show a higher level of expenditure and a lower level of poverty than reports by women. The status of a given household is quite sensitive to which individual within the household is reporting as close to one third of our households changed status from the men's report to the women's report.

5 Conclusion

This paper considers the influence of response error on point-in-time poverty measurement as well as dynamics. While the methods to deal with classical measurement error are well established outside of the development literature, there remain problems that are perhaps specific to the developing country setting. In this paper we consider the particular problems created by the context of a decentralized household structure when dual reporting in the survey instrument is used to correct for the misleading effects of measurement error. We allow for components of asymmetric information or 'private' consumption in modeling the discrepancies in the cross reports of household consumption. This modification is necessary due both to the rejection of the pure measurement error model in our data, as well as prior anthropological and economic evidence that Ghanaian households have a rather decentralized structure.

While the specific implications for poverty transitions from the observed versus the measurement error corrected data are not available for inclusion in this draft, we do find that the degree of discrepancy in the husband and wife reports is on the same order of the *observed*6 month transition rates in and out of poverty. Thus, once our work is completed, these initial findings suggest the corrections for response error may have dramatic effect on the transition rates, if the U.S. literature on measurement error and labor market dynamics is any guide. Furthermore, the econometric structure presented in this paper is adaptable to a variety of developing country settings. Finally, our work also has implications for future fieldwork surveys and their design. While the cross reporting strategy is attractive as a means of characterizing and thus correcting for the confounding effects of classical measurement error, gathering self-reports from each of the principal members of the household will be necessary if the household structure is decentralized. In our data, the incidence of poverty is greatly reduced when both husbands and wives are interviewed as to their consumption as compared to the case where, for example, wives are asked to respond as to the entire household's consumption. Tackling both of these issues simultaneously appears to necessitate a survey which is intensive both on self reporting of consumption as well as on cross reporting strategies.

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Figure 1: Round 12

Includes Village 4

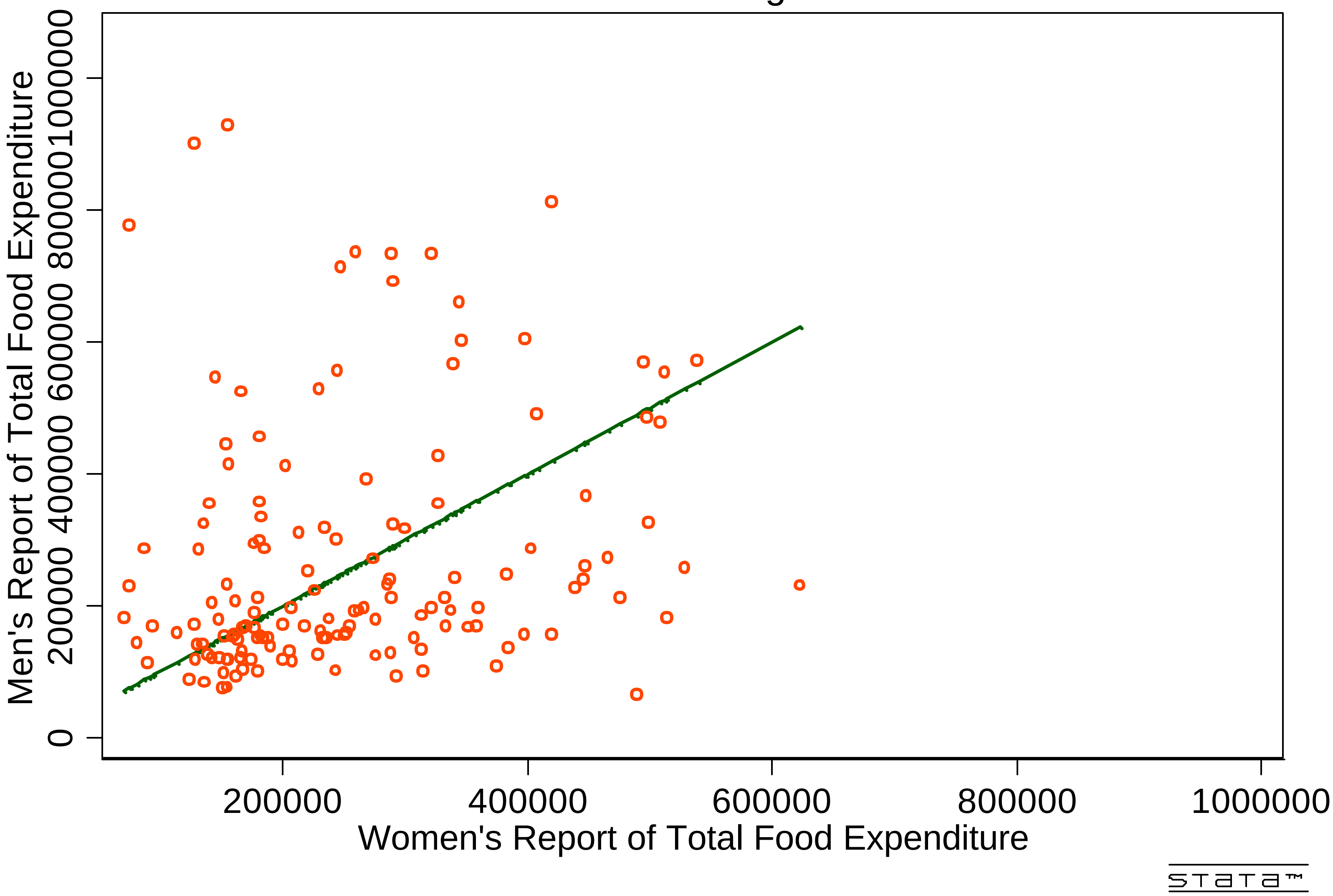

Figure 2: Round 12

Figure 3: Round 12

Excludes Village 4

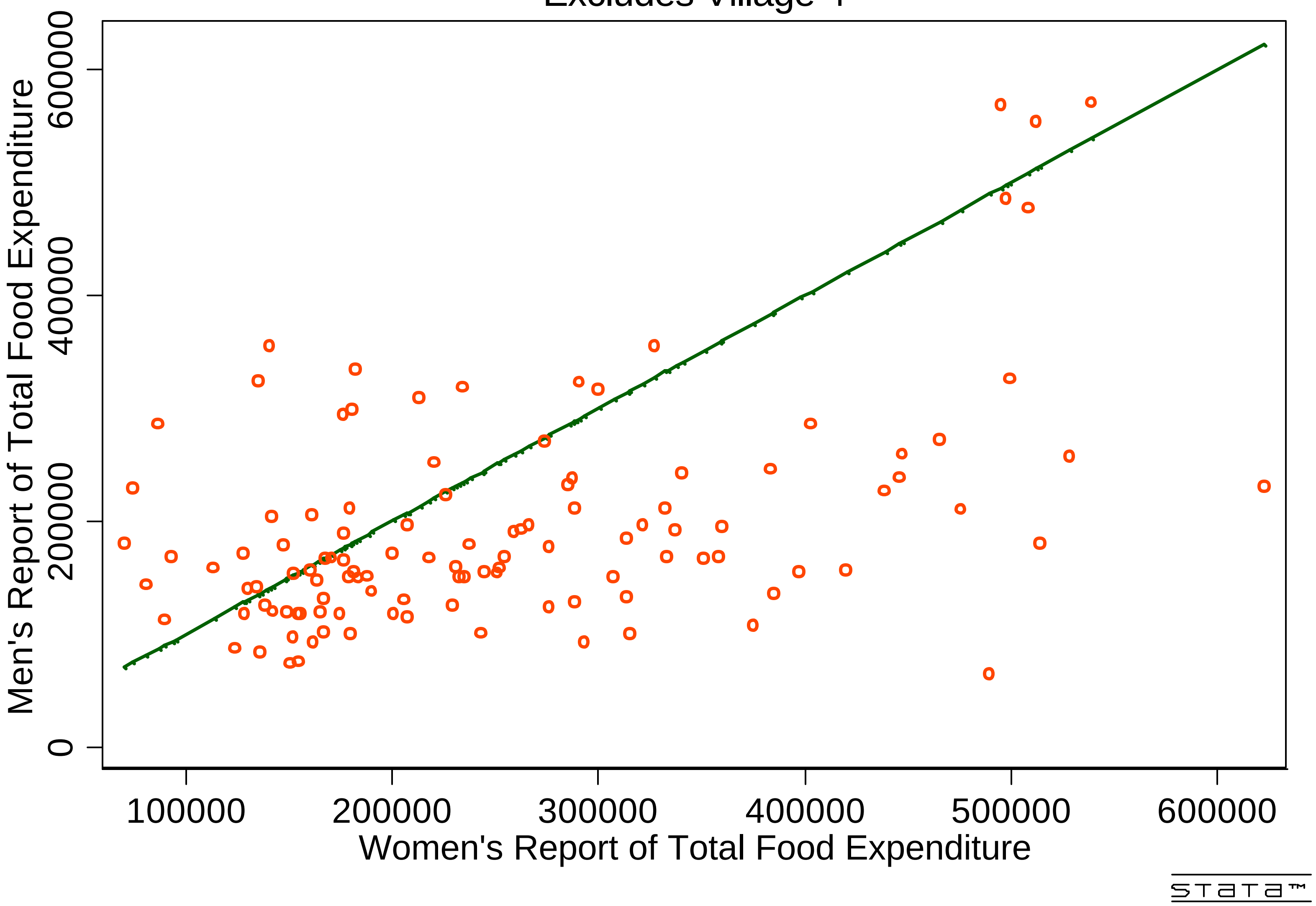

Figure 4: Round 12

Excludes Village 4

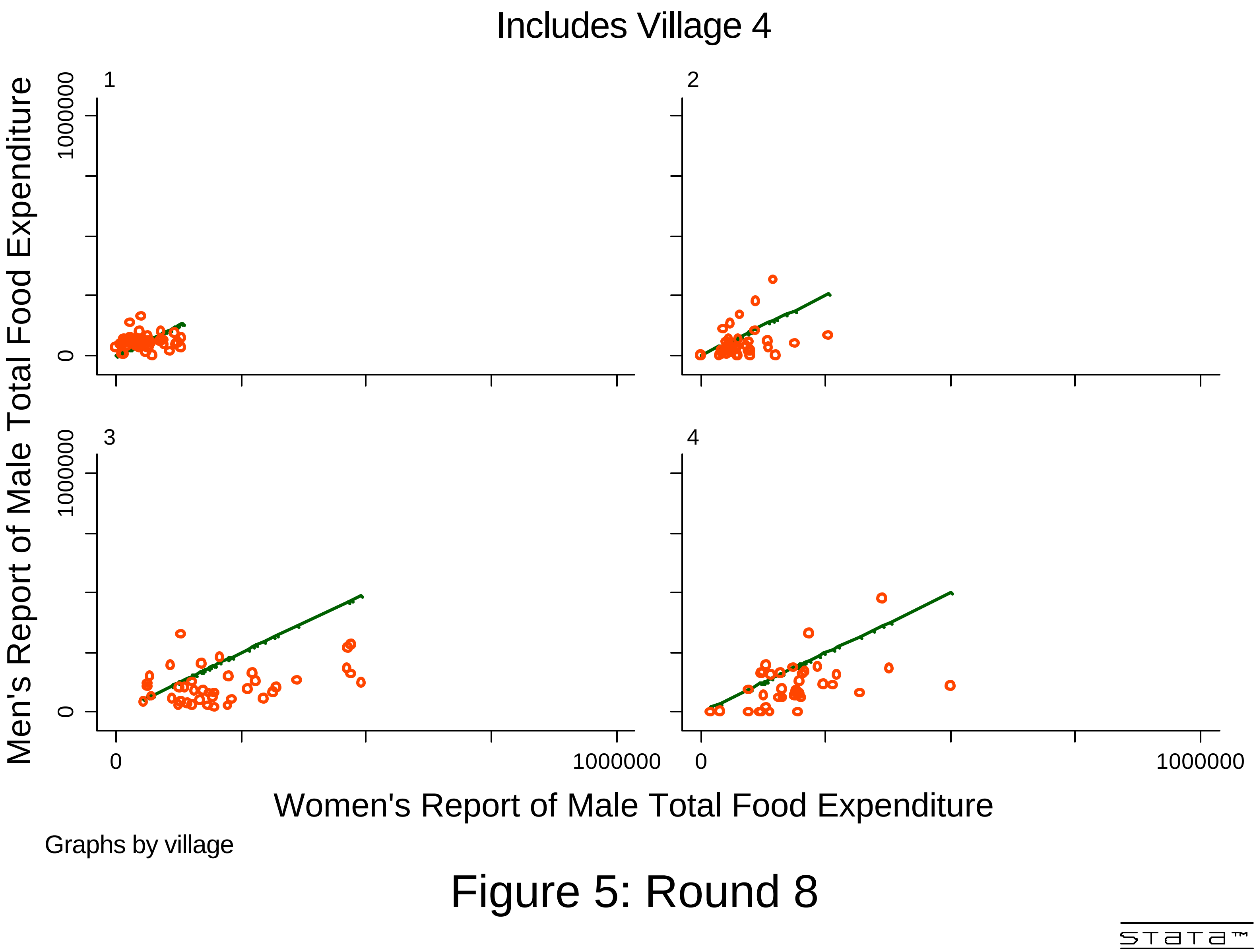

Figure 5: Round 8

Figure 6: Round 8

Includes Village 4

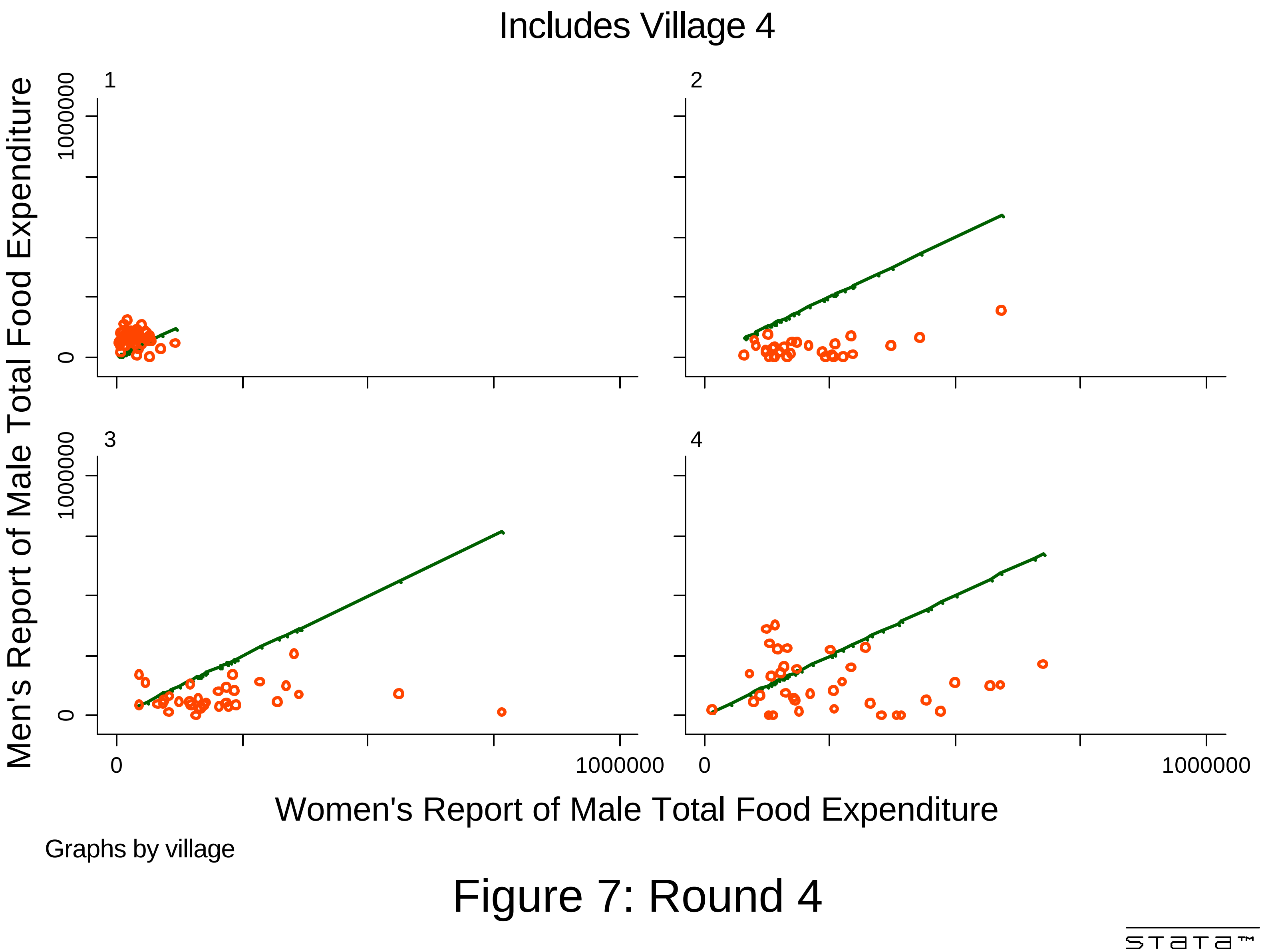

Figure 7: Round 4

Figure 8: Round 4

Includes Village 4

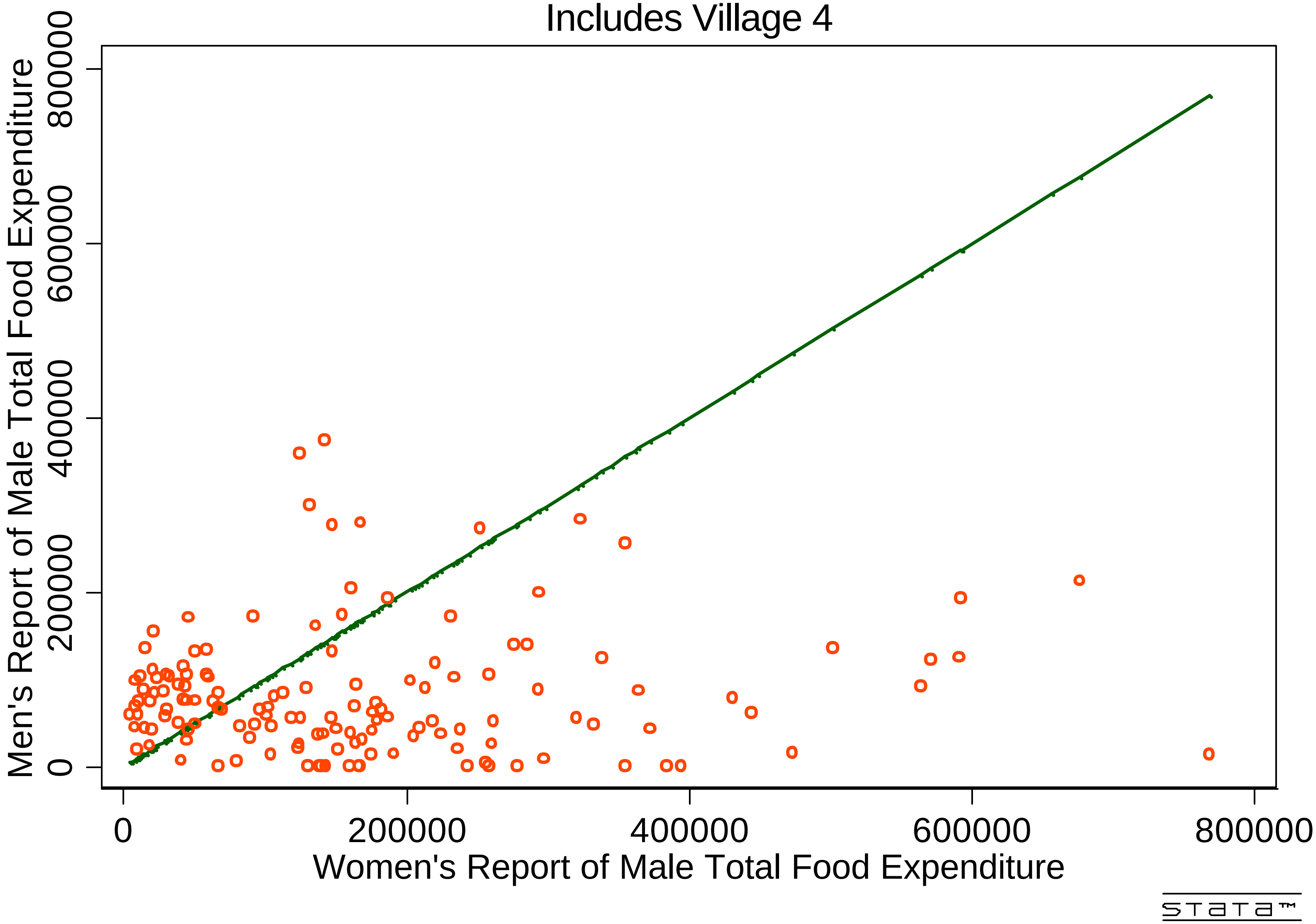

Figure 9: Kernel Density of Female-Male Report Per Capita

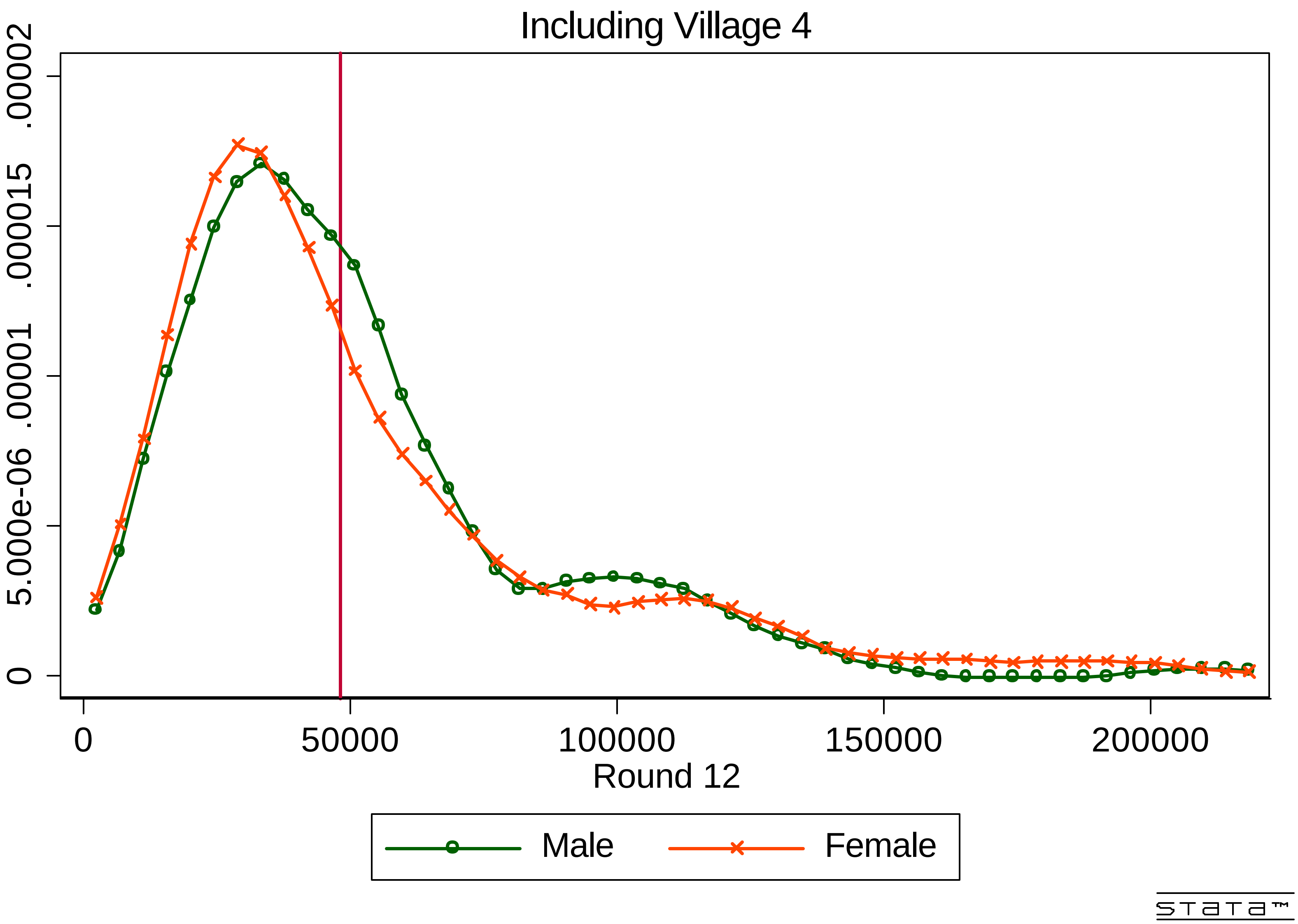

Figure 10: Kernel Density of Female-Male Report Per Capita

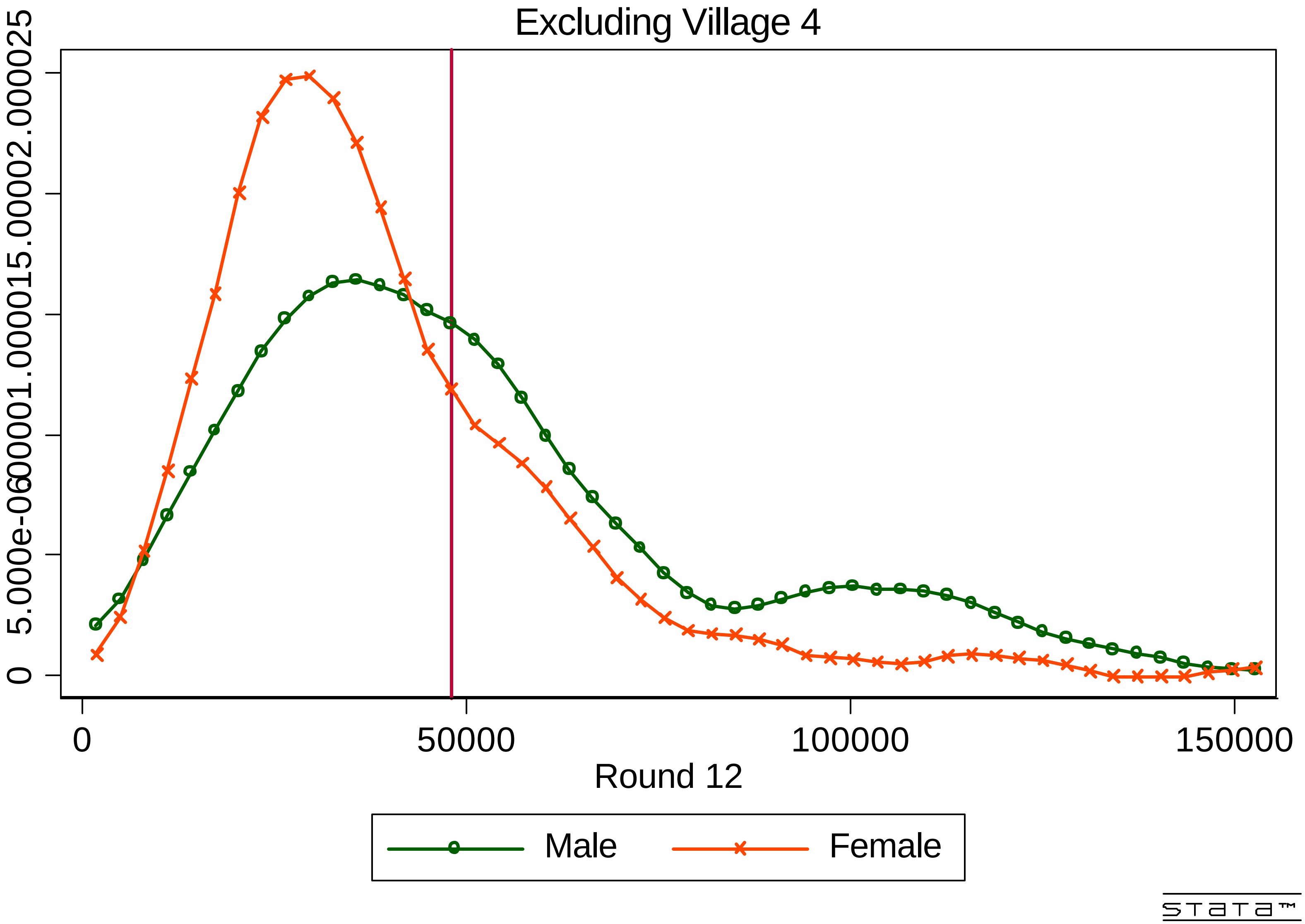

Table 1
Total Household Food Expenditure (including own harvest) in nominal cedis

Report and Round	Mean	Median	Standard Deviation
own, r12	355,668	293,410	222,807
women, r12	280,629	192,267	215,328
male, r12	273,815	233,872	169,762
own, r8	270,165	221,746	182,599
women, r8	195,268	171,465	103,444
own, r4	332,069	260,385	293,107
women, r4	224,976	173,086	202,950

Table 2
Total Household Purchased Food and Own Farm Food, Round 12 Breakdown

Report	Mean	Median	Standard Deviation
own, purchased	201,374	174,930	137,508
women, purchased	159,614	128,992	94,595
men, purchased	158,348	130,158	123,391
own, own farm	155,221	104,478	145,607
women, own farm	120,586	60,870	146,835
men, own farm	108,578	88,942	94,138

Table 3
Women's Reports and Men's Reports: Round 12 Food Expenditure Quintiles

	Male Report				
Female Report	1	2	3	4	5
1	11	7	5	5	3
2	6	11	8	4	2
3	6	3	7	10	5
4	4	6	6	4	10
5	4	4	5	8	10

Table 3a
Women's Reports and Own Reports: Round 12 Food Expenditure Quintiles

	Female Report				
Own Report	1	2	3	4	5
1	12	7	6	5	1
2	11	10	5	4	1
3	7	9	7	5	3
4	1	5	10	9	6
5	0	0	3	8	20

Table 3b
Own Reports and Men's Reports: Round 12 Food Expenditure Quintiles

	Male Report				
Own Report	1	2	3	4	5
1	15	13	1	2	0
2	9	14	7	1	1
3	4	1	3	11	2
4	1	2	7	11	9
5	2	2	3	6	18

Table 4
Total Quintile Shifts Over Time (from Round 4 through Round 12)

Number of Quintiles Changed	Women's Report	Own Report
0	27	12
1	40	49
2	38	31
3	14	20
4	7	13
5	1	3
6	2	1

Table 5
Reports on Poverty Status

Round 12 Poverty: Women's Report		Round 12 Poverty: Men's Report: Not in Poverty	In Poverty	Total
	Not in Poverty	20	5	25
		80.0	20.0	100.0
		40.0	7.69	21.74
	In Poverty	30	60	90
		33.33	66.67	100.0
		60.0	92.31	78.26
	Total	50	65	115
		43.48	56.52	100.0
		100.0	100.0	100.0

Table 6a)
Poverty Transitions: Rounds 8 and 12

Round 8 Poverty: Own Report		Round 12 Poverty: Own Report		
		Not in Poverty	**In Poverty**	**Total**
	Not in Poverty	25	8	33
		75.76	24.24	100.0
		69.44	7.27	22.60
	In Poverty	11	102	113
		9.73	90.27	100.0
		30.56	92.73	77.40
	Total	36	110	146
		24.66	75.34	100.0
		100.0	100.0	100.0

Table 6b)
Poverty Transitions: Rounds 4 and 8

		Round 8 Poverty: Own Report		
		Not in Poverty	**In Poverty**	**Total**
Round 4 Poverty: Own Report	**Not in Poverty**	22	8	30
		73.33	26.67	100.0
		70.97	6.96	20.55
	In Poverty	19	107	116
		7.76	92.24	100.0
		29.03	93.04	79.45
	Total	31	115	146
		21.23	78.77	100.0
		100.0	100.0	100.0

Table 6c)
Poverty Transitions: Rounds 4 and 12

		Round 12 Poverty: Women's Report		
		Not in Poverty	**In Poverty**	**Total**
	Not in Poverty	22	7	29
		75.86	24.14	100.0
Round 4 Poverty: Women's Report		64.71	6.60	20.71
	In Poverty	12	99	111
		10.81	89.19	100.0
		35.29	93.40	79.29
	Total	34	106	140
		24.29	75.71	100.0
		100.0	100.0	100.0