Calculating Comparable Statistics from Incomparable Surveys, with an Application to Poverty in India

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Abstract

We develop an intuitive and easily implemented procedure to recover comparability over time of statistics computed using databases made incomparable by changes in survey design. Our methodology can be adopted whenever the statistic of interest satisfies a certain simple moment condition. The moment condition is satisfied by many interesting economic indicators, including a broad range of poverty and inequality measures. The procedure we propose requires the existence of a set of auxiliary variables whose reports are not affected by the different survey design, and whose relation with the main variable of interest is stable across the surveys. The adjusted estimates can be recovered by using a two-step method of moments framework. Root-n consistency follows easily under regularity conditions. Because most household surveys adopt a multi-stage design, we provide expressions for the asymptotic variance which are robust to the presence of clustering and stratification. We use our adjustment procedure to estimate poverty counts from the 55^{th} Round of the Indian National Sample Survey, a large household survey carried out in 1999-2000. Due to important changes in the adopted questionnaire the unadjusted figures are likely to understate poverty relative to the previous rounds. We provide evidence supporting the plausibility of the identifying assumptions and we conclude that most of the very large reduction in poverty implied by the unadjusted figures is real.

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1 Introduction

Applied economists are often interested in studying changes over time of important economic indicators, such as inequality, poverty, or aggregate measures of private consumption. However, such comparisons are only meaningful insofar as the necessary data are collected consistently over time. Poverty and inequality, in particular, are routinely evaluated using data from household surveys, and it is common to observe changes in the questionnaire adopted by the statistical agency. However, the survey literature convincingly shows that revisions in the questionnaire can affect the pattern of replies in important ways, so that changes in observed economic indicators sometimes reflect changes in the survey, rather than real transformations of the economic environment.[1]

Several papers have recently highlighted the important consequences that the methodology of data collection can have on the estimation of poverty and inequality. Gibson (1999) uses an experiment carried out in Papua New Guinea to study the effects on poverty estimation of collecting expenditure data using diaries instead of recall interviews. Gibson, Huang & Rozelle (2001, 2003) note that changing the reference period in the Chinese Household Income and Expenditure Survey would have dramatic effects on the estimation of poverty and inequality. Jolliffe (2001) studies the large changes in poverty estimates for El Salvador that arise when the list of items included in the expenditure questionnaire is changed. Lanjouw and Lanjouw (2001) perform a similar analysis for Ecuador, Nepal, and Brazil. Others have analyzed the effect of the survey design in expenditure surveys on the estimation of elasticities (Ghose and Bhattacharya, 1995) and economies of scale at the household level (Gibson 2002).

This paper derives its main empirical motivation from a change in data collection methodology that took place recently in India, stirring up an ongoing controversy on the poverty trends in this

[1] See Deaton and Grosh (2000, part II) and references therein for an overview of the methodological issues involved in collecting expenditure data.

country. Because of the still large proportion of poor households within the Indian population, the local poverty numbers frequently appear in economic debates, not only in India, but also within the World Bank. Poverty monitoring in India has historically been based on expenditure data, collected approximately every five years in a large round of the Indian National Sample Survey (NSS). The 55^{th} round of the NSS, carried out between July 1999 and June 2000, was awaited by many, with the expectation that it would at least partly dispel the conflicting evidence on poverty reduction during the nineties, which apparently conflicted with the high rates of economic growth that followed a process of economic liberalization started in 1991.[2] In 1993-94, at the time of the last quinquennial survey carried out adopting the standard questionnaire, the proportion of the Indian population estimated to be poor was 37.3 percent in the rural sector, and 32.4 percent in urban areas. In 1999-2000, the official poverty counts dropped to 27.1 and 23.6 respectively. However, there are a priori arguments suggesting that the unadjusted figures are likely to understate poverty relatively to the previous NSS rounds.

The reasons and consequences of the non-comparability of the 1999-2000 survey with previous NSS rounds have already been analyzed elsewhere, and in the empirical section of the paper we will only briefly summarize the main issues. In particular, the interested reader is referred to Datt, Ravallion and Kozel (2003), Deaton (2001, 2003a, 2003b), Deaton and Drèze (2002), Sen (2000), Sundaram and Tendulkar (2002, 2003) and Visaria (2000).

In this paper we develop an easily implemented adjustment procedure that reestablishes comparability over time for statistics estimated using surveys of different design. We also describe the conditions under which the procedure will perform well, and we study the asymptotic properties of the adjusted estimates in a method of moments framework. The main condition necessary to

[2] The liberalization process started after a Balance of Payment crisis in the summer of 1991. See Sachs, Varshney, and Bajpai (1999), and references therein.

achieve identification is the existence of a set of auxiliary variables whose reports are not affected by the change in survey design, and whose relation with the main variable of interest is stable across the surveys. We also show that the adjustment allows—with appropriate modifications—the estimation of the full comparable distribution of a variable of interest.

The adjustment procedure has a simple intuition. Suppose, for example, that a researcher is interested in estimating a poverty or inequality measure ϕ, which satisfies a population moment condition $E\left[m\left(y\right)-\phi\right]=0$. Typically, y is a meausre of income or expenditure. If the parameter of interest is a simple poverty headcount ratio, $m\left(y\right)$ is a binary variable equal to one is y is below the poverty line. Suppose that a change in the questionnaire makes the latest available data on y non-comparable with analogous data collected in previous periods. Using the law of iterated expectations, one can rewrite the moment condition as $E\left[E\left[m\left(y\right)-\phi\mid \mathbf{v}\right]\right]$, where $\mathbf{v}$ is a set of auxiliary variables. Suppose that the change in the questionnaire did not affect the reports on $\mathbf{v}$, and that the conditional expectation in the latest survey is the same as in a previous one, which adopted a standard questionnaire. Then one can recover a comparable estimate $\hat{\phi}$ by estimating the conditional expectation from the previous survey, and the marginal distribution from the current one.

When the dimension of $\mathbf{v}$ is small, one can obtain the adjusted estimates nonparametrically. Deaton (2003b) adopts this strategy to estimate adjusted poverty counts for the 55^{th} round of the Indian NSS using a single auxiliary variable. Specifically, he uses expenditure per head in a set of miscellaneous items for which the recall period remained the same in all the NSS surveys. Ideally, it would be useful to use also information on several household characteristics likely to be correlated with y, but when the number of auxiliary variables is large, the use of nonparametric estimators becomes computationally intractable. We show that the computational burden is greatly reduced if one rewrites the estimator—through simple repeated application of Bayes' rule—as a function of conditional probabilities that can be estimated using a parametric binary dependent variable model. This transformation implies that a broad spectrum of adjusted poverty and inequality measures will satisfy a modified two step moment condition, with a logit first step. Since most household surveys adopt a complex stratified and clustered design—which implies that observations are neither independent not identically distributed—we study the asymptotic properties of the estimators using the framework developed in Bhattacharya (2002), who studies the large sample properties of method of moment estimators with multi-stage samples.

Clearly, the usefulness of the adjustment relies crucially on the identifying assumptions being appropriate. Since such assumptions involve variables that are *not* observed, they cannot be formally tested. In our application to the 1999-2000 round of the Indian NSS, we provide indirect empirical support to the assumptions by making use of smaller experimental expenditure surveys, carried out in years preceding the 55^{th} round. We also show how these experimental surveys can be used to test the performance of our adjustment procedure. Overall, the evidence suggests that our estimates are useful to recover comparability across NSS rounds of different design. Surprisingly, our results show that most of the reduction in poverty shown by the unadjusted figures is real, even if the adjustment suggests that the change in the questionnaire actually caused a relative underestimation of poverty rates, especially in rural areas.

Even if our emphasis on comparability issues over time is justified by the empirical application, the approach developed here also has other potentially fruitful applications. For example, one can merge information on auxiliary variables from a census (which commonly does not record expenditure) with data on the same variables *and* expenditure from a household survey, to recover welfare measures for geographically small areas, for which a representative sample from the survey is typically not available.[3] More generally, the estimator can be used to mitigate the problem of

[3] For a different approach, see Elbers, Lanjouw and Lanjouw (2003), whose procedure explicitely focuses on the

missing data, when the statistic of interest satisfies a moment condition as the one described above, and when data on an adequate set of auxiliary variables are available. Clearly, in every application the plausibility of the identification assumptions should be carefully scrutinized.

The rest of the paper is organized as follows. Section 2 delineates the theoretical problem, and states the assumptions required by our adjustment procedure. Section 3 introduces the estimator and describes its asymptotic properties. We cover the empirical application in Section 4, and we conclude in Section 5.

2 The Theoretical Problem

In what follows we will refer to the population sampled using a revised methodology as the *target population*, while the *auxiliary population* is one that has been sampled using a standard questionnaire.[4] Target and auxiliary *surveys* are analogously defined. Let τ be a binary variable equal to one when an observation is drawn from the target population, and zero otherwise. Let y be the main variable of interest *as measured in a standard questionnaire*. In poverty or inequality measurement y typically measures consumption, income, or earnings per head.[5] Suppose that the researcher is interested in estimating the value of a parameter ϕ in a target population, where ϕ satisfies the following population moment condition:

$$E\left[n\left(m\left(y\right)-\phi\right) \mid \tau=1\right]=0 \tag{1}$$

estimation of micro-level poverty and inequality measures.

[4] Typically there will be more than one potential auxiliary survey, one per every round of the survey carried out before the changes in methodology.

[5] Taking intra-household allocation into account would require controversial decisions on how to deal with household scale economies and equivalence scales. For an overview of the issues involved see Deaton (1997, Ch. 4), and Deaton and Case (2002).

where n is household size. The population moment (1) explicitely refers to the frequent situation in which the parameter of interest is defined in terms of a *per capita* variable y across individuals, but data are sampled at the household level. For example, ϕ might represent average calorie consumption per head, so that $m(y) = y$, but the survey collects data on total household consumption. When data are collected at the individual level, or when the parameter is not defined in per capita terms, all the results that follow can be obtained as a straightforward special case with $n = 1$.

The moment condition (1) encompasses a broad set of commonly used poverty measures.[6] For example, if ϕ represents a Foster-Greer-Thorbecke (FGT) poverty index, and z is the poverty line, then $m(y) = 1(y < z)\left(1 - \frac{y}{z}\right)^{\alpha}$, with $\alpha \geq 0$, where $(1 - y/z)$ represents the poverty gap, and $1(E)$ is an indicator function equal to one when event E is true, and zero otherwise. When $\alpha = 0$, the index becomes the headcount poverty ratio, while $\alpha = 1$ characterizes the poverty gap ratio. A higher parameter α indicates that large poverty gaps $(1 - y/z)$ are given a larger weight in the computation, so that the poverty index becomes more sensitive to the distribution of y among the poor. The above moment condition is also easily adapted to describe well-known inequality measures like the Atkinson index, if $m(y) = \left(\frac{y}{E[y]}\right)^{1-\epsilon}$, or the Theil index, with $m(y) = \frac{y}{E[y]} \ln \frac{y}{E[y]}$. In these cases, the sample equivalent of equation (1) should contain a sample moment condition for the expected value of y, which is generally not known.

Clearly, if y is not measured in the data collected from the target population, the estimation of the parameter ϕ through the sample equivalent of (1) is infeasible. This is precisely the case if the survey questionnaire changed in such a way that the respondents' reports are no longer comparable with those from previous surveys, so that the researcher can only observe a different variable, say $\tilde{y}$, but not y. For example, y is consumption per head when the recall period is the week before the

[6] For an introduction to the theory and practice of poverty measurement see Deaton (1997, Ch. 3). For poverty estimation, see also Ravallion (1993).

interview, while $\tilde{y}$ is the report corresponding to a different recall period. Since the phrasing of the questions frequently have important effects on the pattern of replies, it should be clear that y and $\tilde{y}$ are actually different variables, even if they are purported to represent exactly the same object.

Suppose that the researcher observes, in both the target and the auxiliary population, a set of variables that we denote $\mathbf{v}$ if they are recorded using 'standard' methodology, and $\tilde{\mathbf{v}}$ if a revised methodology is adopted. Once again, notice that $\mathbf{v}$ and $\tilde{\mathbf{v}}$ are both intended to represent the same quantities.

In what follows we consider each sample unit as drawn from a population encompassing both the target and the auxiliary population. Each observation is characterized by the set of variables $(y, \tilde{y}, \mathbf{v}, \tilde{\mathbf{v}}, \tau)$. The econometrician can only observe either $(y, \mathbf{v}, \tau)$, when $\tau = 0$, or $(\tilde{y}, \tilde{\mathbf{v}}, \tau)$, if $\tau = 1$, which makes the direct estimation of (1) infeasible. However, ϕ can be estimated if the assumptions described in the following proposition hold:

Proposition 1 - Suppose that there exist a set of auxiliary variables $\mathbf{v}$, including household size n, distributed according to $dF(\mathbf{v})$, and suppose that

A1. $dF(\tilde{\mathbf{v}} \mid \tau = 1) = dF(\mathbf{v} \mid \tau = 1)$

A2. $E[m(y) \mid \mathbf{v}, \tau = 1] = E[m(y) \mid \mathbf{v}, \tau = 0]$

A3. $dF(\mathbf{v} \mid \tau = 1)$ and $dF(\mathbf{v} \mid \tau = 0)$ have a common support.

Then ϕ satisfies the following modified population moment condition

$$E[nR(\mathbf{v})\, m(y) - \phi\eta_1 \mid \tau = 0] = 0 \tag{2}$$

where $\eta_1 = E[n \mid \tau = 1]$, and $R(\mathbf{v})$ is the *reweighting function* defined as

$$R(\mathbf{v}) = \frac{dF(\mathbf{v} \mid \tau = 1)}{dF(\mathbf{v} \mid \tau = 0)} = \frac{P(\tau = 1 \mid \mathbf{v})\, P(\tau = 0)}{P(\tau = 0 \mid \mathbf{v})\, P(\tau = 1)} \tag{3}$$

where $P(\tau = 1 \mid \mathbf{v})$ is the probability that an observation belongs to the target population conditional on observing $\mathbf{v}$, and the other probabilities are defined accordingly.

Proof: see appendix.

Assumption A1 requires that the econometrician has access to a set of *auxiliary* variables $\mathbf{v}$ whose marginal distribution is identified by the sampling process both in the auxiliary and in the target population. In other words, $\mathbf{v}$ includes variables whose reports are left unaffected by the change in survey design. Such variables are likely to be available, since questionnaire revisions generally do leave several questions unchanged. When the non-comparability across surveys is caused by changes in the methodology adopted to measure consumption or income, the inclusion in $\mathbf{v}$ of household size and other household characteristics is reasonable, unless their definition changes between surveys. A2 is the most crucial assumption, and the one who should be scrutinized more closely in every empirical application of our methodology. It requires that the conditional expectation of the function $m(y)$ is the same in the target and the auxiliary surveys. For example, if one is interested in estimating a headcount poverty ratio, A2 amounts to assuming that the fraction of households to be counted as poor *conditional on* $\mathbf{v}$ remains constant across the two surveys. Finally, the common support assumption A3 ensures that $R(\mathbf{v})$ is finite and bounded away from zero for each value of $\mathbf{v}$, and that the distributions involved in A2 are defined for every $\mathbf{v}$. Intuitively, this condition ensures that we can estimate $E[m(y) \mid \mathbf{v}, \tau = 1]$ for every $\mathbf{v}$, which is necessary in order to recover the unconditional population moment ϕ.[7]

The problem at hand has clear similarities with the problem of building counterfactuals for causal inference with non-experimental data in program evaluation.[8] In fact, the object we want to

[7] Note that even if A3 does not hold one can still estimate bounds for the parameter of interest treating observations with $\mathbf{v}$ outside the common support as missing values, using the setting described in Horowitz and Manski (1995).

[8] For a review, see for example Heckman, Lalonde, and Smith, 1999, Ch. 7.

estimate is basically a counterfactual distribution, that is, the distribution of per capita expenditure (or a functional of it) as we would estimate it if the survey did not change. Also, assumption A2 is analogous to that of selection on observables (or unconfoundedness) adopted in some cases in the program evaluation literature, while A3 is analogous to the assumption that the sample used by the evaluator contains both treated and untreated individuals for every $\mathbf{v}$.

Proposition 1 shows that, under the stated assumptions, one can estimate a parameter ϕ, 'comparable' with analogous ones estimated when y is observed, without using (unavailable) observations on y from the target survey. Notice that information from the target survey are still necessary for the estimation, since the reweighting function $R(\mathbf{v})$ has to be estimated making use of observations belonging to both the auxiliary and the target population.

A sufficient (but in no way necessary) condition for (2) to hold is that the conditional *density* $f(y \mid \mathbf{v}, \tau)$ is the same in the two surveys. When this assumption can be maintained, it is possible to recover a comparable estimate of the whole distribution of y. Note that here the object of interest is the density of a per capita quantity y which should be defined over *individuals*, while data are typically collected at the *household* level. This implies that the individual-based density of y, which we denote by $f_n(y \mid \tau = 1)$, is described in the population by the following expression:

$$f_n(y \mid \tau = 1) = \frac{E[nf(y \mid n, \tau = 1)]}{E[n \mid \tau = 1]}$$

where $f(y \mid n, \tau = 1)$ is the density defined over *households*.[9] In this case, the comparability issues arise from the presence of y in the above expression, while we maintain the very reasonable assumption that n is measured consistently in both surveys. Here the sampling process identifies the parameter of interest if we can identify $f(y \mid n, \tau = 1)$. The following proposition formalizes this argument.

Proposition 2 - Suppose that there exist a set of auxiliary variables $\mathbf{v}$, including household

[9] It is easy to check that $f_n(y \mid \tau = 1)$ actually integrates to one.

size n, distributed according to $dF(\mathbf{v})$, such that A1 and A3 hold. Let $\mathbf{v}_{-n}$ be a vector of observed variables including all the variables in $\mathbf{v}$ except n. Suppose also that

A2b $f(y \mid \mathbf{v}, \tau = 1) = f(y \mid \mathbf{v}, \tau = 0)$

Then

$$f(y \mid n, \tau = 1) = f(y \mid n, \tau = 0)\, E\left[R(\mathbf{v}_{-n}) \mid y, n, \tau = 0\right] \tag{4}$$

where the reweighting function is now defined as $R(\mathbf{v}_{-n}) = \frac{P(\tau=1|\mathbf{v})P(\tau=0|n)}{P(\tau=0|\mathbf{v})P(\tau=1|n)}$.[10]

Proof: see appendix.

We do not proceed to analyze the estimation of (4), since we are mostly interested in the estimation of parameters identified by a moment condition as the one described in (2).[11] We turn now to analyze the estimation and the asymptotic properties of a two-step estimator for such parameters.

3 Estimation

From the modified moment condition (2) above, it is clear that the estimation of ϕ requires the preliminary estimation of η_1 and $R(\mathbf{v})$, which are generally not known to the econometrician. The estimation of η_1—the average household size in the target population—is trivial, once we maintain the assumption that n is measured consistently across all surveys. The reweighting function $R(\mathbf{v})$

[10] When data are collected at the individual level—so that $f(y \mid \tau = 0)$ is the object of interest—equation (4) becomes, *mutatis mutandis*, analogous to equation (3) in DiNardo, Fortin, and Lemieux (1996). There, the authors use a reweighting procedure to estimate the separate effects on the distribution of wages in the US of different changes in institutional and labor market factors that took place during the eighties.

[11] To estimate the adjusted density one can use a procedure analogous to that developed in DiNardo et al. (1996).

is a function of the unknown probabilities $P(\tau = 1 \mid \mathbf{v})$ and $P(\tau = 0)$, where the latter represents the proportion of *households* belonging to the target population, while the former—conditional—probability can be interpreted as the fraction of *households* whose covariates are equal to $\mathbf{v}$ that belongs to the target survey. We emphasize that both probabilities refer to the distribution of households, and not individuals, so that they have to be estimated without inflating observations by household size.

In what follows we proceed assuming that the researcher samples a total of n observations from a population encompassing both the target and the auxiliary population. We assume that the conditional probability can be estimated parametrically, using a logit or a probit model, regressing the binary variable τ on the vector of auxiliary variables $\mathbf{v}$. If the dimension of the vector $\mathbf{v}$ is small relative to the sample size, one can also estimate the probability nonparametrically, but we do not pursue this estimation strategy here. A flexible functional form can be achieved using polynomials, and in any case we are only interested in obtaining good predictions for the conditional probabilities, while the parameters estimated in the binary variable model will be of little or no interest per se.

Let $\boldsymbol{\beta} \equiv [\phi \;\; \boldsymbol{\theta}_1^T \;\; \theta_0 \;\; \eta_1]^T$ denote the (column) vector containing the true values of all relevant parameters, where $\theta_0 = P(\tau = 0)$, $\eta_1 = E[n \mid \tau = 1]$, $\boldsymbol{\theta}_1$ is the k-dimensional column vector of parameters—including the constant—entering the first step binary regression $P(\tau = 1 \mid \mathbf{v})$, and T indicates transpose. Since we estimate $P(\tau = 1 \mid \mathbf{v})$ using a logit or a probit model, we denote such conditional probability by $P\left(\bar{\mathbf{v}}^T \boldsymbol{\theta}_1\right)$, where $\bar{\mathbf{v}} = \left[1 \;\; \mathbf{v}^T\right]^T$. Similarly, $p\left(\bar{\mathbf{v}}^T \boldsymbol{\theta}_1\right)$ is the density corresponding to the appropriate CDF.

Using Proposition 1 and the parametric assumption on the functional form of $P(\tau = 1 \mid \mathbf{v})$, it is easy to see that $\boldsymbol{\beta}$ satisfies the following population moment condition.

$$
\begin{aligned}
0 &= E\left[\boldsymbol{\mu}\left(y, \mathbf{v}; \boldsymbol{\beta}\right)\right] \\
&= E\begin{bmatrix} \mu_1\left(y, \mathbf{v}, \tau; \phi, \boldsymbol{\theta}_1, \theta_0, \eta_1\right) \\ \boldsymbol{\mu}_2\left(\mathbf{v}, \tau; \boldsymbol{\theta}_1\right) \\ \mu_3\left(\tau; \theta_0\right) \\ \mu_4\left(n, \tau; \eta_1\right) \end{bmatrix}
\end{aligned} \tag{5}
$$

where

$$
\begin{aligned}
&\mu_1\left(y, \mathbf{v}, \tau; \phi, \boldsymbol{\theta}_1, \theta_0, \eta_1\right) = 1\left(\tau = 0\right)\left[n\frac{P\left(\bar{\mathbf{v}}^T\boldsymbol{\theta}_1\right)\theta_0}{\left[1-P\left(\bar{\mathbf{v}}^T\boldsymbol{\theta}_1\right)\right]\left(1-\theta_0\right)}m\left(y\right) - \phi\eta_1\right] \\
&\boldsymbol{\mu}_2\left(\mathbf{v}, \tau; \boldsymbol{\theta}_1\right) = \left[\frac{\tau - P\left(\bar{\mathbf{v}}^T\boldsymbol{\theta}_1\right)}{P\left(\bar{\mathbf{v}}^T\boldsymbol{\theta}_1\right)\left[1-P\left(\bar{\mathbf{v}}^T\boldsymbol{\theta}_1\right)\right]}p\left(\bar{\mathbf{v}}^T\boldsymbol{\theta}_1\right)\right]\bar{\mathbf{v}} \\
&\mu_3\left(\tau; \theta_0\right) = \left[\left(1-\tau\right) - \theta_0\right] \\
&\mu_4\left(n, \tau; \eta_1\right) = 1\left(\tau = 1\right)\left[n - \eta_1\right]
\end{aligned} \tag{6}
$$

In (6), $\boldsymbol{\mu}_2\left(\mathbf{v}, \tau; \boldsymbol{\theta}_1\right)$ represents the set of first order conditions for the Maximum Likelihood Estimator of $\boldsymbol{\theta}_1$, assuming a logit or probit functional form.

Clearly, all parameters can be estimated by solving the system of equation resulting from replacing the population moment conditions in (5) with the corresponding sample moments. If the observations are a simple random sample from a sampling frame encompassing both the target and the auxiliary population, standard errors can be estimated using the standard asymptotic theory for method of moments estimators (see, for example, Newey and McFadden, 1994). However, most surveys adopt a stratified and clustered design, which makes the assumption of i.i.d. observations untenable. This is the case, for example, in the Indian National Sample Survey, which will be the object of our empirical application, but the same holds for many other widely used surveys, like the World Bank's Living Standard Measurement Surveys, or the Current Population Survey and the Panel Study of Income Dynamics in the United States.

Stratification implies that the population is first divided into a fixed number of subpopulations, called strata, which are usually defined following geographical and/or socioeconomic criteria. Then a *predetermined* number of first stage units (households or clusters, see below) are sampled independently from each stratum. Stratification, then, typically reduces the standard errors of the estimates, since all possible samples contain a fixed proportion of observations from each stratum.[12] With clustering, instead, households are not sampled directly from the population (or from a stratum), but are selected in a second stage from clusters, typically villages or urban blocks, which are therefore the first stage units of the survey. Since households selected from the same clusters tend to be relatively homogeneous, intra-cluster correlation is usually positive, which almost always implies an increase in the standard errors of the estimates.[13] In some surveys, including the NSS, the design is further complicated by the presence of second-stage stratification, which is present when each selected cluster is split into separate sub-clusters, and a fixed number of households is selected from each subcluster.

Notice also that in most surveys the sampling scheme is such that households selected from different clusters have a different *ex-ante* probability of being selected. This is the case, for example, if clusters are selected via simple random sampling in the first stage, and a constant number of households is sampled from each selected cluster in the second stage. This implies that, before clusters are selected, households living in larger clusters have a smaller probability of being sampled. So, unless clusters are selected with probability proportional to their size, consistent estimation of population parameters requires the use of sampling weights, which 'inflate' each observation by the inverse of the probability of selection.[14]

For all these reasons, here we use the results in Bhattacharya (2002), who studies the asymptotic

[12] See Deaton (1997, Ch. 1) or Howes and Lanjouw (1995, 1998) for an introduction to the econometrics of stratified and clustered samples.

[13] On the other side, clustering allows the interviewers to visit a smaller number of locations, so reducing the cost of the survey.

[14] See Deaton (1997, Ch. 3) for a discussion of sampling weights.

properties of Method of Moments estimators allowing for the presence of two-stage stratification, clustering, and sampling weights.

Suppose that the population is divided into S strata, and that stratum s contains a mass of H_s clusters. Cluster c is further divided into S_c second-stage strata, each one containing $M(s, c, s_c)$ households. Then, the population moment condition can be written as

$$0 = \sum_{s=1}^{S} H_s E_c \left\{ \sum_{s_c=1}^{S_c} \sum_{h=1}^{M(s,c,\sigma)} \boldsymbol{\mu}\left(y_{scs_ch}, \mathbf{v}_{scs_ch}; \boldsymbol{\beta}\right) \mid s \right\} \tag{7}$$

where the expectation in curly brackets is taken with respect to the distribution of clusters in stratum s, and $\boldsymbol{\beta}$ denotes the true value of the vector of parameters. If n_s clusters are selected from the s^{th} stratum, and m_{scs_c} households are selected in the s_c-th second-stage stratum in cluster c belonging to stratum s, the sample equivalent of (7) is

$$0 = \sum_{s=1}^{S} \frac{H_s}{n_s} \sum_{c=1}^{n_s} \sum_{s_c=1}^{S_c} \frac{M(s, c, s_c)}{m_{scs_c}} \sum_{h=1}^{m_{scs_c}} \boldsymbol{\mu}\left(y_{scs_ch}, \mathbf{v}_{scs_ch}; \hat{\boldsymbol{\beta}}\right) \tag{8}$$

where $(n_s m_{scs_c})^{-1} H_s M(s, c, s_c)$ represents the sampling weight.[15]

Bhattacharya (2002) obtains the asymptotic results letting the total number of selected *clusters*—denoted by n—grow to infinity, while the number of households selected in every second-stage stratum is kept constant, as well as the *proportion of clusters selected per stratum*, denoted by $a_s = n_s/n$. This is appropriate for our empirical application, since in the Indian NSS the total number of clusters selected is much higher than the number of households selected per cluster.

[15] It is instructive to note that the expected value of the sum of the sample weights is an estimate of the total mass of households in the population. In fact:

$$\begin{aligned} & E\left[\sum_{s=1}^{S}\sum_{c=1}^{n_s}\sum_{s_c=1}^{S_c}\sum_{h=1}^{m_{scs_c}} \frac{H_s}{n_s}\frac{M(s,c,s_c)}{m_{scs_c}}\right] \\ = & \sum_{s=1}^{S} H_s E\left[\frac{1}{n_s}\sum_{c=1}^{n_s}\sum_{s_c=1}^{S_c}\sum_{h=1}^{m_{scs_c}} \frac{M(s,c,s_c)}{m_{scs_c}} \mid s\right] \\ = & \sum_{s=1}^{S} H_s E\left[\sum_{s_c=1}^{S_c} M(s,c,s_c) \mid s\right] \end{aligned}$$

Besides complicating the notation, the presence of the complex survey design complicates the asymptotics, since observations are now neither independent—since observations coming from the same cluster are typically correlated—nor identically distributed, since observations are selected from different strata, which are typically characterized by different distributions. However, Bhattacharya (2002) notes that the clusters *are* independent, and so (8) can be rewritten as the average of n independent terms. So, re-indexing clusters by i, we have

$$\begin{aligned} \frac{1}{n}\sum_{i=1}^{n} m_i\left(\hat{\boldsymbol{\beta}}\right) &= 0 \quad \text{where} \\ m_i\left(\hat{\boldsymbol{\beta}}\right) &= \sum_{s=1}^{S} \frac{H_s}{a_s} 1\left(i \in s\right) \sum_{s_i=1}^{S_i} \frac{M\left(s,i,s_i\right)}{m_{sis_i}} \sum_{h=1}^{m_{sis_i}} \boldsymbol{\mu}\left(y_{sis_ih}, \mathbf{v}_{sis_ih}; \hat{\boldsymbol{\beta}}\right) \end{aligned} \tag{9}$$

Then Bhattacharya (2002) identifies sufficient regularity conditions for consistency and asymptotic normality, and shows that the following result holds:

$$\sqrt{n}\left(\hat{\boldsymbol{\beta}} - \boldsymbol{\beta}_0\right) \xrightarrow{d} N\left(0, \Gamma^{-1} W \left(\Gamma^{-1}\right)^T\right) \tag{10}$$

where note that the asymptotic covariance matrix has a form analogous to the standard robust variance allowing for arbitrary heteroskedasticity and group serial correlation. In the appendix we describe the functional forms or Γ and W in our context, as well as their consistent estimators, and we report—for the sake of completeness—the full proposition in Bhattacharya (2002).

Since we use observations sampled from two different databases, one has to be explicit about how the sampling from the two populations is done. We consider the case where first stage strata are the same across the two subpopulations, and we take the states to represent different strata.[16] To preserve independence across clusters, we maintain the assumption that, in each stratum, clusters are selected randomly from a sampling frame encompassing both the target and the auxiliary

[16] In the NSS, strata are actually defined at a much finer level. Broadly, strata are districts in the rural sector sectors, and town, or sections of large towns, in urban areas. In the NSS, allowing for stratification has typically negligible effects on the standard errors, while allowing for clustering is important.

population.

4 An application to the estimation of Poverty in India

For decades, the Planning Commission of the Government of India has regularly published "official" headcount poverty ratios, separately for rural and urban areas of every Indian state and Union Territory. The poverty counts are computed as the fraction of the population living in households in which total consumption per person is below the poverty line, which is kept constant over time in real terms.[17] The poverty lines are price-inflated using two different state-specific price indexes: the Consumer Price Index for Agricultural Labourers (CPIAL) for rural areas, and the Consumer Price Index for Industrial Workers (CPIIW) for the urban sector.[18] Data on household expenditure are collected by the Indian National Sample Survey Organization (NSSO) approximately every five years, from a large sample of Indian households interviewed over a one-year period. Each NSS round contains information on a wide spectrum of socioeconomic variables, but the largest section of the database consists of records of household consumption of a very detailed list of items.[19]

[17] In the early seventies, NSS expenditure data have also been used to estimate the calorie Engel curves the poverty lines are based on. The lines have been computed to represent the total monthly per capita expenditure associated, on average, with a sector-specific minimum calorie intake, as recommended by the Indian National Institute of Nutrition. At the time of writing, official poverty estimates that use such lines are available for 1973-74 (28^{th} NSS Round), 1977-78 (32^{nd}), 1983-84 (38^{th}), 1987-88 (43^{rd}), 1993-94 (50^{th}), and 1999-2000 (55^{th}).

[18] These price indexes are available through a number of publications issued by the Government of India (for example the Statistical Abstract). For a detailed overview of the issues related to the choice of poverty lines in India see GOI, Planning Commission (1993), or Deaton and Tarozzi (2000), who also criticize the appropriateness of the indexes used to price inflate the lines, and propose alternatives. For a different set of poverty lines, developed within the World Bank, see also Datt (1999).

[19] The item list is extremely detailed, and purportedly exhaustive. The questionnaire lists, for example, approximately 200 different food items.

Until the 50^{th} round, carried out in 1993-94, all NSS surveys adopted a 30-day recall period for all expenditure items. This choice of recall period is unusual, and most statistical agencies use a much shorter recall period for items—like food—that are typically purchased frequently, and a longer recall periods for infrequent expenditures like clothing, footwear, educational expenses, and durables. Several experimental studies find that expenditure reports for frequently purchased items are on average proportionally lower when the recall period becomes longer.[20] According to some, a switch to more standard recall periods would have helped reconciling the high rate of growth measured by the National Accounts Statistics in the nineties, and the unimpressive rates of poverty reduction estimated using NSS data over the same period. To explore further the issue, the NSSO experimented with different recall periods using the smaller ('thin') NSS rounds that followed the 1993-94 survey. Such surveys were not specifically designed for poverty monitoring, so some doubts remain on the comparability of their sampling frames, but each survey did also gather information on expenditure on a list of items basically identical to the standard one.[21] However, the NSSO adopted *two* different expenditure questionnaires and only one of them was assigned at random to all households living in a given primary stage unit of the survey. One questionnaire type—*schedule 1*—was the standard one, with a 30-day recall period for all items, while in the other type—*schedule* 2—the recall period was set equal to the 7 days before the interview for food, beverages and some other items generally bought frequently, and to 365 days for durables, clothing, footwear and some

[20] See, in particular, Scott and Amenuvegbe (1990). The 30-day recall period was adopted because of an early experimental study by Mahalanobis and Sen (1954). This study does find a negative relation between reported expenditure and length of the recall period, but finds that reports based on a 7-day recall period are *too* high. A new study carried out by the NSSO (2003) suggests instead that the shorter recall period is more appropriate for many high-frequency expenditures.

[21] Most NSS rounds do not focus specifically on expenditure, and are carried out over a smaller sample than the one typical of a 'quinquennial' round. For example, the main focus was on education and fertility in the 52^{nd} round, and on informal sector enterprises in the 51^{st}.

other low-frequency purchases. For what follows, it should be kept in mind that the standard 30-day recall period was instead kept *in both schedules* for a list of items accounting for a substantial share of the budget. This list included fuel and light, miscellaneous goods and services, rents and consumer taxes, and non-institutional medical expenses.[22] We will generally refer to this list of items as '30-day' or 'miscellaneous' items.

The experimental surveys once again confirmed the finding that reported expenditure in food is significantly higher if the recall period is shortened. The opposite result was found for most durables: even if more households report some purchases when a one year recall period is adopted, average monthly expenditure is lower than the one observed using a 30-day reference period.[23] Given the large average fraction of the budget spent in food, the net effect in all surveys and in both sectors is a larger estimate of total per capita expenditure (pce) when the experimental questionnaire is used. Table 1 contains summary statistics for the major Indian states.[24] In all surveys and both sectors, average total monthly pce is systematically 10-20 percent higher for households in the experimental group, and the differences are always statistically significant at standard statistical levels. Row (5) shows that, if one keeps the poverty line constant, this gap would translate into a fifty percent drop in poverty 'achieved' through a change in the survey methodology.[25] However, the results in row

[22]The distinction between institutional and non-institutional medical expenses lies in whether the expenses were incurred for medical treatment as an in-patient at a medical institution or otherwise.

[23]For details, see Deaton (2001) and Sen (2000).

[24]Such states include Andhra Pradesh, Assam, Bihar, Gujarat, Haryana (urban sector only), Jammu & Kashmir, Karnataka, Kerala, Madhya Pradesh, Maharashtra, Orissa, Punjab, Rajasthan, Tamil Nadu, Uttar Pradesh, West Bengal, and Delhi. These states account for more than 95 percent of the total Indian population.

[25]The fact that the poverty lines are based on predicted calorie consumption given total pce might suggest that if the revised questionnaire had to be used for poverty monitoring, the lines should be recalculated. Deaton (2001) notes that doing so would, if anything, further *increase* the gap. Since a shorter recall period causes a larger proportion of the reported total budget to be spent in food, then a lower level of pce would be necessary to achieve the recommended minimum calorie intake. This would further reduce poverty. However, after analyzing the Engel

(3) show that the differences in reports on miscellaneous items are always very small, and in most cases not statistically significant. This is crucial for our purposes, since it gives some preliminary support to the claim that reports on a set of items are not influenced by reports in a different set of items. This observation makes expenditure in miscellaneous items an excellent candidate for the inclusion in the vector of auxiliary variables. Moreover, the figures in row (5) show that 30-day items are likely to be good predictor of total pce, since the corresponding average budget share is above 20 percent in the rural sector—using standard questionnaires—and above 25 percent in urban areas. In each one of the surveys represented in Table 1, a simple log-linear regressions of total pce on pce in 30-day expenditure produces an R^2 above 0.65.

However, close examination of the figures in row (3) shows that in the rural sector mean pce in miscellaneous items is systematically higher when computed using the standard questionnaire, even if the differences are always small. The sign of the differences is reversed in the urban sector. This empirical regularity is likely to be related to sectoral differences in consumption patterns and in household characteristics. The cognitive processes adopted to remember expenditure should be expected to be related to the item specificity, and to the characteristics of the respondent. This suggests that it might be important to include household characteristics among the auxiliary variables. Here we will make use of information on household size, completed education of the household head, a categorical variable for land holdings, main economic activity of the household, and whether the household belongs to special social groups (called 'Scheduled Castes and Tribes' in NSS). We will generally refer to these variables as (household-specific) controls.

Finally, notice that the figures in Table 1 show no apparent trend in poverty reduction over the examined period. However, the thin rounds were not specifically designed as expenditure surveys. The relatively small samples, coupled with the choice of sampling frames more suited to the different

curves for food, Deaton (2001) argues that the poverty lines should remain the same.

main purpose of these surveys, induced many observers to look at these poverty figures with some suspicion, and to wait for the next quinquennial expenditure survey, that is, for the 55^{th} round of the NSS.

In the 55^{th} round of the survey, the NSSO decided to adopt a questionnaire combining *both* sets of recall periods used in the thin rounds. Expenditure in food was to be recorded using *both* the 30-day *and* the 7-day recall periods for *all* households, while for durables and other infrequently purchased items only a 365-day recall period was to be used. The unadjusted results estimated by the Indian Planning Commission showed an impressive reduction in poverty with respect to the early nineties: using the data collected with a 30-day reports for food, in the rural sector the counts dropped from 37.2 in 1993-94 to 27.1 six years later, while in urban areas the proportion dropped from 32.6 percent to 23.6. In both cases this amounts to a reduction of one third in poverty rates, in less than a decade. However, the changes in the questionnaire cast serious doubts on the comparability of the more recent figures with previous poverty estimates, especially if one considers the results of the thin experimental rounds.

On the one hand, the thin rounds showed that reports on durables are on average lower when a 1-year recall period is used, so that the new questionnaire would overstate poverty. At the same time, more respondents reported some expenditure in durables, with the consequence that the corresponding distribution is much more spread out when the shorter recall period is used. Keeping the average report constant, this would cause the opposite result of *lower* poverty estimates when the experimental questionnaire is used. The two conflicting effects combine with the fact that durables typically account for a small share of the total budget, especially among poor households, making it unlikely that important comparability issues arise as a consequence.

On the other hand, the new questionnaire recorded the two separate reports on food expenditure in two parallel columns printed next to each other. One can therefore expect that this format prompted the respondents (or the interviewers) to reconcile the two different reports. So, consumption of food reported with the traditional 30-day recall period would be disproportionately *high* (since the respondent would tend to avoid large discrepancies with the 7-day reports, which are typically higher), *and/or* the corresponding reports based on a 7-day recall period would be disproportionately *low* (by a symmetric argument). The plausibility of this argument is strengthened by the fact that in the 55^{th} round average pce in food as estimated with a 7-day recall period exceeded the corresponding figure calculated using the 30-day recall period by about 6 percent, while in all the thin rounds the gap was consistently above 30 percent. Since for most Indian households food accounts for a very large share of the total budget, these arguments lead to the expectation that the unadjusted figures might significantly overstate total expenditure, and therefore significantly understate poverty. Surprisingly, we find that our adjustment procedure does not change considerably the poverty estimates, suggesting that the reconciliation between the sets of reports on food expenditure worked mostly in one direction, shifting the 7-day reports downwards towards the 30-day reports, but not vice versa.

4.1 Validating the Assumptions

In each thin round, every household received only one questionnaire type, so for each respondent we observe either $(y, \mathbf{v}, \tau)$ or $(\tilde{y}, \tilde{\mathbf{v}}, \tau)$. However, the questionnaire type was assigned randomly to households, so that the distribution of both $(y, \mathbf{v}, \tau)$ and $(\tilde{y}, \tilde{\mathbf{v}}, \tau)$ are identified in each survey. This fact can be used to provide support to the identifying assumptions needed for our adjusted estimates to be reliable. In the rest of the paper, we will not use data from the 54^{th} round. This survey was carried out over a six-month period, while all other rounds are carried out over a whole year, so seasonality issues might cause comparability problems.

Assumption A1 requires that the reports on the variables included in $\mathbf{v}$ be independent of the questionnaire type. This assumption is easily tested for the discrete household specific controls that we include in $\mathbf{v}$, all of which are discrete. For each sector and survey we test the hypothesis that the distribution of each of these variables is independent upon the questionnaire types, by using a Pearson χ^2 statistic modified to take into account stratification and clustering.[26] Under the null hypothesis, once we tabulate observations across values and schedules, the 'joint' proportion of observations in the cell related to the the i-th value and the k-th schedule, should be the same as the product of the 'marginal' proportion of observations having the i-th value, and the 'marginal' proportion of observations in the k-th schedule. The test rejects the null when a normalized sum of the differences between joints and products of marginals is large. We report the p-values of each test in Table 2. The results strongly support the null. Only 3 out of 30 tests reject the null, and even in these cases the proportions are very similar across the schedules, as one can see from the cross tabulations reported in the lower part of Table 2. These results are hardly surprising, since the two different questionnaires are assigned randomly, and there is no obvious reason why differences in the recall periods should affect respondents' reports on the household controls we use here.

Let m and $\tilde{m}$ denote the (log of) pce in miscellaneous items, as measured respectively in a standard and experimental questionnaire. Assumption A1 requires the equality of the densities of m and $\tilde{m}$ in the same round and sector. In Figure 1 we draw nonparametric kernel estimates of the densities of $m, \tilde{m}, y$ and $\tilde{y}$ for each survey and sector.[27] It is apparent that in all cases the distribution of $\tilde{y}$ is shifted to the right with respect to the distribution of y. This is consistent with the results in Table 1, which showed that mean total pce is systematically higher for households in

[26] More precisely, we use a second-order corrected Pearson statistic, as in Rao and Scott (1984). This test can be performed using the "svytab" command in STATA™.

[27] We use the robust bandwidth proposed by Silverman (1986) for the estimation of approximately normal densities with a biweight kernel.

the experimental group. However, there is no such large and systematic gap between the distributions of m and $\tilde{m}$. Only in the urban sector of the 51^{st} round does the difference between the two curves appear visually not negligible, but even in this case the two densities closely coincide for low values of expenditure, which is the relevant range when one is interested in poverty counts. We perform a simple test for the equality of the distributions of m and $\tilde{m}$ using the Pearson χ^2 statistic described above.[28] First we divide the range of m into bins of equal length, and then we test the null hypothesis that the distribution of the observations across bins is independent upon the schedule used to measure m. To avoid the presence of many empty cells, we consider only observations included between the first and the last percentile of the round-sector distribution. Table 3 shows the estimated p-values computed using 10, 15, or 20 bins, as a robustness check. In most cases the null hypothesis cannot be rejected, and the p-values are above 0.2. The null is never rejected if we use a one percent significance level, while using a five percent level we reject in the rural sector of the 52^{nd} Round, with 10 or 15 bins, and in both sectors of the 51^{st} Round with 10 bins.

Overall, assumption A1 appears to hold well for the variables we plan to include in $\mathbf{v}$, so we move to analyze assumption A2, which in this context requires the stability over different rounds of the probability of being poor, conditional on the observed $\mathbf{v}$. In Figure 2 we plot the estimated probabilities conditional on m only. Each line represents a nonparametric locally weighted regression on m of a dummy variable equal to one when a household's pce is below the poverty line z.[29] Because we are interested in the stability of $P(y < z \mid m)$, all the lines are constructed

[28] We do not use a Kolmogorov-Smirnov test for the equality of two densities estimated nonparametrically since this test does not allow for the presence of a complex survey design.

[29] In a locally weighted regression a "local" OLS regression is run at every point where the conditional expectation is evaluated (see Fan, 1992). The regression is local since at every point we use only observations for which the regressor is inside a neighborhood of the point itself, defined by a bandwidth. The observations are weighted by using a kernel, so that observations closer to the point have more weight in the regression itself. We prefer locally weighted regressions to the traditional Nadaraya-Watson estimators since the former tends to reduce the bias arising in the

using only observations from households that received the standard questionnaire. Even if the lines do present some systematic gaps in some areas of their range, they look extremely similar overall, suggesting that the assumption of a constant conditional probability is at the very least a sensible working hypothesis. Note also that there is no apparent time trend in the way the curves differ from each other, suggesting that no important gradual change is affecting the relation between m and y.

In our context, the availability of empirical evidence supporting the stability of the conditional probability is extremely important, since there are several reasons why such assumption might fail. The major concern is that movements in the relative price of m should be expected to affect the conditional probability of being poor, whose stability ultimately depends upon the stability of the Engel curve linking y to m. Also, changes in tastes—or other demand shocks—might change the relation between one survey and another. All these concerns are likely to be less pressing if the auxiliary and the target survey are carried out in consecutive years. In any case, note that our procedure is flexible enough to accommodate many of these factors, as long as they are observable, and therefore can be included in the vector $\mathbf{v}$.

As a further check, we impose a logit functional form to the conditional probability $P(y < z \mid \mathbf{v})$, and we test for the equality of the coefficients across surveys. In Table 4 we report robust tests for the equality of coefficients across surveys for each pair of rounds, for the rural and urban sectors separately, and using different sets of variables included in $\mathbf{v}$. Over a total of 60 tests, the joint null hypothesis of equal coefficients is rejected in 21 cases using a five percent significance level, and in 11 cases using a one percent level. Overall, the conditional probabilities appear remarkably stable, especially taking into account the very large size of the samples (each tests uses a minimum of

estimates when the density of the regressor is not flat. For a clear treatment of the Nadaraya-Watson estimator see Pagan and Ullah (1999, Ch. 3). Deaton (1997, Ch. 3) provides an intuitive treatment of locally weighted regressions.

18153 of observations, and a maximum of 72528), and the fact that in most cases the null imposes more than twenty restrictions.

4.2 Performance of the Estimator

The previous section shows that the assumptions needed for the good performance of our adjustment procedure are reasonable, in our empirical context. As a further check. we use the thin rounds to perform an empirical exercise in order to evaluate the performance of the estimator. For each thin round we can estimate a 'comparable' poverty count using data on total pce from the subset of households who received a standard questionnaire. Then we can attempt to replicate these 'benchmark' poverty rates, using data on the auxiliary variables collected from households who received an experimental questionnaire, while deriving information on $P(y < z \mid \mathbf{v})$ from any of the available standard surveys. Using the robust standard errors estimated using the procedure described earlier in the paper, we test for the significance of the difference between the benchmark and the adjusted poverty ratios. If the reweighting procedure performs well, one should not be able to reject the hypothesis that the two estimates are equal.

We report the results of the tests in Table 5. Columns 1 and 2 report the benchmark and the unadjusted poverty count for each round. As we already pointed out, the poverty counts are approximately halved when the experimental surveys are used. In every case the differences are statistically significant at any standard significance level. Columns 3 to 6 report the adjusted poverty ratios and their standard errors, while asterisks denote the cases in which the null of no difference between benchmark and adjusted ratios is rejected. Each test is simply based on the ratio between the difference and its standard error. Since in each cell the benchmark and the adjusted ratio are computed using data from independent surveys, such standard errors are trivial to compute, once one has the standard errors for each poverty count. For each sector in each thin round, ordered along the rows of the tables, we experiment using auxiliary data from the same sector, in either the 50^{th} round, or from the standard survey in each of the thin rounds. For each target-auxiliary pair we use three different sets of auxiliary variables: a polynomial in m and household size, a polynomial in household size and dummies for household controls, or all of the above.

The results are mixed, but overall encouraging. Using a five percent significance level, the null is not rejected in 48 out of 72 tests. The null is never rejected when we use the standard survey in a thin round as auxiliary survey for the experimental survey in the same round. This can be interpreted as further indirect evidence of the validity of assumption A1, since the random assignment of questionnaire type should guarantee that, within the same round, assumption A2 holds. In all cases, the absolute differences between the benchmark and the adjusted figures are much lower than the differences across questionnaire types in the same round. However, in the rural sector, when m is included among the auxiliary variables, the adjusted figures are systematically above the corresponding benchmark, by 3-5 percentage points. However, this should not suggest that excluding m from the auxiliary variables would improve the performance of the estimator. In fact, careful examination of the adjusted estimates obtained excluding m reveals that the adjustment does little more than reproducing the poverty ratio from the corresponding auxiliary survey. For example, the headcount ratio for the rural sector of the 51^{st} round is 41.8, and adjusted ratios calculated using this as auxiliary survey are 41.6 in the 52^{nd} round, and 40.9 in the 53^{rd} round. If the auxiliary survey is the 53^{rd} round, in which the benchmark poverty ratio is 35.7, the adjusted headcounts are 35.3 for the 51^{st} round, and 35.6 for the 52^{nd} round. One plausible explanation is the fact that since the household controls do not explain a large fraction of the total variance of y, so that the reweighting function $R(\mathbf{v})$ is everywhere close to one, and as a consequence the adjustment merely reproduces the poverty count in the auxiliary survey. For this reason, when applying our adjustment procedure to the 55^{th} NSS round, we always include m among the auxiliary variables.

As one would expect, there is a close correspondence between the performance of the estimator and the results of the tests for the validity of the identifying assumptions that we described in the previous sections. For example, in the urban sector of the 53^{rd} round the hypothesis that the benchmark and the adjusted ratios are the same is rejected in all specifications, when the 52^{nd} round is used as auxiliary survey. At the same time, the figures in the bottom panel of the last column in Table 4 show that, for this pair of rounds, the assumption of equal conditional probability in the same urban sector always fails. We observe the opposite if we use the 51^{st} round as auxiliary survey for the 53^{rd} (or vice-versa). These results clearly stress that in empirical applications much care should be taken in evaluating the credibility of the identifying assumptions.

4.3 Adjusted Poverty Estimates from the 55^{th} Round of the National Sample Survey

The questionnaire adopted in the 55^{th} round of the NSS is different from any previously adopted one. We already mentioned that the questionnaire asked all respondents to report consumption in food using two different recall periods, while a 365-day recall period was introduced for consumption on durables and some other items. However, expenditure in miscellaneous items, a good predictor of total pce, was reported only using the standard 30-day recall period, so we can use this variable, together with other controls, to implement our adjustment procedure. In the previous section we provided indirect evidence supporting the stability of the conditional probability of being in poverty given the auxiliary variables, and we showed that changes in some sections of the questionnaire seem to have only mild consequences on reports recorded in unchanged sections.

Table 6 contains the sector-specific adjusted estimates for the poverty counts. As a robustness check, we use all the NSS surveys between the 50^{th} and the 53^{rd} Round as auxiliary surveys. As usual, we make use of the official poverty lines for all India in 93-94 Rupees. All monetary values from subsequent Rounds are deflated using state and sector specific official Consumer Price Indexes. Here we exclude Jammu and Kashmir from our estimates since we do not have information on the relevant price indexes for the latest survey period. We report two sets of estimates. In Column 1 $\mathbf{v}$ includes a cubic in m and household size. In Column 2 we also include the other household-specific controls mentioned earlier in the paper. We obtain all adjusted poverty ratios using the expression in (8), while the standard errors are computed as the first element along the diagonal of the robust covariance matrix in (10).

The unadjusted poverty figures for the states included in our analysis are 28.4 percent in rural areas and 24.5 percent in urban areas. In all cases but one, our adjustment procedure produces higher estimates of poverty, as expected. The one exception is the point estimate for urban areas when the 52^{nd} round is used as auxiliary survey, and household specific controls are not included among the auxiliary variables. In the rural sector, adjusted poverty counts range from 30.4 percent to 32.5 percent. In urban areas, the adjusted figures range from 24 to 27.3 percent. In all cases the increase in point estimates is more pronounced if the controls are included. The inclusion of such controls also slightly increases the estimated standard deviations.

Expenditure data from the previous quinquennial round, carried out in 1993-94, showed that the proportion of the population in poverty in the states considered here was 33.4 percent in urban areas, and 38.2 percent in the countryside. So, even if the adjustment delivers—as expected—higher headcount ratios than the official ones, our estimates confirm a very large poverty reduction in India during the nineties. This conclusion is consistent with the results obtained by Deaton (2003b) and Sundaram and Tendulkar (2003). The latter authors, together with Deaton and Drèze (2002), also argue that such large decline in poverty is consistent with evidence from employment surveys, the National Accounts and from data on agricultural wages. So, our conclusion is that most of the poverty reduction measured by using NSS data is real, and not simply a statistical artifact due to a change in the survey design.

The relatively small difference between adjusted and unadjusted estimates suggests that most of the reconciliation between 7-day and 30-day reports comes from a bias of the 7-day reports towards the 30-day ones, and not vice versa.

5 Caveats and Conclusions

In this paper we introduce an intuitive and easily implemented method to compute comparable statistics for differently designed surveys, when the different design causes the respondents' reports to be incomparable across the surveys. Our emphasis is in the estimation of poverty headcount ratios, but the framework can be generally applied to a broad set of parameters satisfying a population moment condition of the form $E[m(y) - \phi] = 0$, where y is the variable measured in a non-comparable way in the different surveys. The procedure we propose requires the existence of a set of auxiliary variables whose reports are not affected by the different survey design, and whose relation with the main variable of interest is stable across the surveys.

The reliability of the adjusted estimates depends crucially on the reliability of the necessary identifying assumptions, which should be carefully evaluated by the researcher on a case by case basis. With this caveat, the procedure introduced here should be a very useful tool for the researcher interested in the evolution over time of welfare or aggregate economic indicators, since changes in survey methodology are frequent, and can easily lead to non-comparability issues.

We estimate adjusted poverty counts from the 55^{th} round of the Indian National Sample Survey, a large expenditure survey carried out in 1999-2000, for which comparability issues arose due to changes in the adopted questionnaire. The identifying assumptions needed for the good performance of our estimator involve unobserved variables, and therefore cannot be directly tested. However, by using previous NSS rounds, we provide indirect evidence supporting to a large extent their validity. According to our estimates, poverty counts in India in 1999-2000 were close to 30 percent in rural areas, and 25 percent in urban areas. Even if these figures are slightly higher than the unadjusted ones, they still show an impressive poverty decline in the nineties, since the previous estimates—from the 1993-94 round of the NSS—were approximately 30 percent higher.

Even if our emphasis on comparability issues over time is justified by the empirical application, the approach developed here also has other potentially fruitful applications. For example, one can merge information on auxiliary variables from a census (which commonly does not record expenditure) with data on the same variables *and* expenditure from a household survey, to recover welfare measures for geographically small areas, for which a representative sample from the survey is typically not available.[30] More generally, the estimator can be used to mitigate the problem of missing data, when the statistic of interest satisfies the moment condition described above, and when data on an adequate set of auxiliary variables are available. Clearly, in every application the plausibility of the identification assumptions should be carefully scrutinized.

The identifying assumptions will typically *not* hold if the target and the auxiliary surveys refer to very different populations, so, for example, our methodology is unlikely to be useful if one needs to make cross-country comparisons of welfare indicators.

As a final caveat, it should be stressed that in our empirical application we do *not* suggest the superiority of one set of recall period over the others. Our scope is to ensure comparability over time for otherwise incomparable statistics, but our methodology should not be interpreted as a tool to solve a measurement error problem. In fact, even when the adjustment works perfectly,

[30] For a different approach, see Elbers, Lanjouw and Lanjouw (2003), whose procedure explicitely focuses on the estimation of micro-level poverty and inequality measures.

the adjusted estimates will not recover the *true* value of the parameter of interest. Rather, the estimator aims at recovering the estimate for such parameter that one would have obtained if the questionnaire change did not take place.

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Appendix

Proof of Proposition 1:

Using the law of iterated expectations, and A2, we can rewrite the initial moment condition as

$$E\left[n\left(m\left(y\right)-\phi\right)\mid\tau=1\right]=E\left[E\left[nm\left(y\right)\mid\mathbf{v},\tau=0\right]\mid\tau=1\right]-\phi\eta_{1}.$$

Then the conclusion follows rewriting the first term on the right-hand side using Bayes' rule:

$$\begin{aligned}
E\left[E\left[nm\left(y\right)\mid\mathbf{v},\tau=0\right]\mid\tau=1\right] &= \int_{\mathbf{v}}\left[\int_{y}nm\left(y\right)dF\left(y\mid\mathbf{v},\tau=0\right)\right]dF\left(\mathbf{v}\mid\tau=1\right)\\
&= \int_{\mathbf{v}}\int_{y}nm\left(y\right)\frac{dF\left(y,\mathbf{v}\mid\tau=0\right)}{dF\left(\mathbf{v}\mid\tau=0\right)}dF\left(\mathbf{v}\mid\tau=1\right)\\
&= \int_{\mathbf{v}}\int_{y}nm\left(y\right)dF\left(y,\mathbf{v}\mid\tau=0\right)\frac{dF\left(\mathbf{v}\mid\tau=1\right)}{dF\left(\mathbf{v}\mid\tau=0\right)}\\
&= \int_{\mathbf{v}}\int_{y}nR\left(\mathbf{v}\right)m\left(y\right)dF\left(y,\mathbf{v}\mid\tau=0\right)\\
&= E\left[nR\left(\mathbf{v}\right)m\left(y\right)\mid\tau=0\right]. \qquad\blacksquare
\end{aligned}$$

Note that if $\mathbf{v}$ does not include n, A2 should be modified as $E\left[nm\left(y\right)\mid\mathbf{v},\tau=0\right]=E\left[nm\left(y\right)\mid\mathbf{v},\tau=1\right].$

Proof of Proposition 2:

$$\begin{aligned}
f\left(y\mid n,\tau=1\right) &= \int_{\mathbf{v}_{-n}}dF\left(y,\mathbf{v}_{-n}\mid n,\tau=1\right)\\
&= \int_{\mathbf{v}_{-n}}f\left(y\mid\mathbf{v}_{-n},n,\tau=1\right)dF\left(\mathbf{v}_{-n}\mid n,\tau=1\right)\\
\text{by A2b} &= \int_{\mathbf{v}_{-n}}f\left(y\mid\mathbf{v}_{-n},n,\tau=0\right)dF\left(\mathbf{v}_{-n}\mid n,\tau=1\right)\\
&= \int_{\mathbf{v}_{-n}}f\left(y,\mathbf{v}_{-n}\mid n,\tau=0\right)\frac{dF\left(\mathbf{v}_{-n}\mid n,\tau=1\right)}{dF\left(\mathbf{v}_{-n}\mid n,\tau=0\right)}\\
&= \int_{\mathbf{v}_{-n}}f\left(\mathbf{v}_{-n}\mid y,n,\tau=0\right)f\left(y\mid n,\tau=0\right)R\left(\mathbf{v}_{-n}\right)\\
&= f\left(y\mid n,\tau=0\right)\int_{\mathbf{v}_{-n}}R\left(\mathbf{v}_{-n}\right)f\left(\mathbf{v}_{-n}\mid y,n,\tau=0\right)\\
&= f\left(y\mid n,\tau=0\right)E\left[R\left(\mathbf{v}_{-n}\right)\mid y,n,\tau=0\right] \qquad\blacksquare
\end{aligned}$$

Note that here the reweighting function has a different form, since all probabilities are now conditional. So

$$\begin{aligned}
R(\mathbf{v}_{-n}) &= \frac{dF(\mathbf{v}_{-n} \mid n, \tau = 1)}{dF(\mathbf{v}_{-n} \mid n, \tau = 0)} \\
&= \frac{dF(\mathbf{v}_{-n}, n, \tau = 1)}{dF(n, \tau = 1)} \frac{dF(n, \tau = 0)}{dF(\mathbf{v}_{-n}, n, \tau = 0)} \\
&= \frac{P(\tau = 1 \mid \mathbf{v}_{-n}, n)\, dF(\mathbf{v}_{-n}, n)}{P(\tau = 0 \mid \mathbf{v}_{-n}, n)\, dF(\mathbf{v}_{-n}, n)} \frac{P(\tau = 0 \mid n)\, dF(n)}{P(\tau = 1 \mid n)\, dF(n)} \\
&= \frac{P(\tau = 1 \mid \mathbf{v})\, P(\tau = 0 \mid n)}{P(\tau = 0 \mid \mathbf{v})\, P(\tau = 1 \mid n)}
\end{aligned}$$

The main Theorem in Bhattacharya (2002)

Let the population moment condition be

$$0 = \sum_{s=1}^{S} H_s E_c \left\{ \sum_{s_c=1}^{S_c} \sum_{h=1}^{M(s,c,\sigma)} \boldsymbol{\mu}(y_{scs_ch}, \mathbf{v}_{scs_ch}; \boldsymbol{\beta}_0) \mid s \right\} \tag{11}$$

where s is the stratum, c the cluster, s_c the second-stage stratum, and h is the household. Define $m_i(\boldsymbol{\beta})$ as follows

$$\mathbf{m}_i(\boldsymbol{\beta}) = \sum_{s=1}^{S} \frac{H_s}{a_s} 1(i \in s) \sum_{s_i=1}^{S_i} \frac{M(s, i, s_i)}{m_{sis_i}} \sum_{h=1}^{m_{sis_i}} \boldsymbol{\mu}(y_{sis_ih}, \mathbf{v}_{sis_ih}; \boldsymbol{\beta})$$

Let $m_i^j(\boldsymbol{\beta})$ be the j-th element in the J-dimensional column vector $\mathbf{m}_i(\boldsymbol{\beta})$.

A1 $\mathbf{m}^j(y, \mathbf{v}; \boldsymbol{\beta})$ is continuous at each $\boldsymbol{\beta}$ w.p. 1, for each $j = 1, ..., J$.

A2 $\exists d(y, \mathbf{v})$ with $E[d(y, \mathbf{v})] < \infty$ such that $\left\| m^j(y, \mathbf{v}; \boldsymbol{\beta}) \right\| \leq d(y, \mathbf{v})$ for each $j = 1, ..., J$.

A3 The parameter space is compact.

A4 $\boldsymbol{\beta}_0$ is an interior point of the parameter space, and $\boldsymbol{\beta}_0$ solves (11) uniquely.

A5 $E[\mathbf{m}(y, \mathbf{v}; \boldsymbol{\beta})]$ is continuously differentiable at $\boldsymbol{\beta}_0$ and Γ is nonsingular, where

$$\Gamma = p \lim \frac{1}{n} \sum_{i=1}^{n} \frac{\partial}{\partial \boldsymbol{\beta}^T} E(\mathbf{m}_i(\boldsymbol{\beta}_0))$$

A6 Stochastic equicontinuity holds for the following sequence

$$\nu_n(\boldsymbol{\beta}) = \frac{1}{\sqrt{n}} \sum_{i=1}^{n} \left[\mathbf{m}_i(\boldsymbol{\beta}) - E(\mathbf{m}_i(\boldsymbol{\beta}))\right]$$

A7 $E\left|\mathbf{m}_i(\boldsymbol{\beta})\right|^3 < \infty$

$$\textbf{A8} \begin{cases} \text{a.} & \lim_{n\to\infty} \sum_{i=1}^{n} \frac{Var(\mathbf{m}_i(\boldsymbol{\beta}))}{i^2} < \infty \\ \text{b.} & \lim_{n\to\infty} \frac{1}{n} \sum_{i=1}^{n} Var(\mathbf{m}_i(\boldsymbol{\beta})) = W < \infty \end{cases}$$

The main theorem in Bhattacharya shows that under assumptions A1 through A4 and A8a

$$p \lim_{n\to\infty} \hat{\boldsymbol{\beta}} = \boldsymbol{\beta}_0$$

while under assumptions A1 through A7 and A8b

$$\sqrt{n}\left(\hat{\boldsymbol{\beta}} - \boldsymbol{\beta}_0\right) \xrightarrow{d} N\left(0, \Gamma^{-1} W \left(\Gamma^{-1}\right)^T\right)$$

where

$$\begin{aligned} \Gamma &= p \lim \frac{1}{n} \sum_{i=1}^{n} \frac{\partial}{\partial \boldsymbol{\beta}^T} E(\mathbf{m}_i(\boldsymbol{\beta}_0)) \\ W &= \lim_{n\to\infty} \frac{1}{n} \sum_{i=1}^{n} Var(\mathbf{m}_i(\boldsymbol{\beta}_0)) \end{aligned}$$

In our context, the vector of parameters to be estimated is $\boldsymbol{\beta} \equiv [\phi \;\; \boldsymbol{\theta}_1^T \;\; \theta_0 \;\; \eta_1]^T$, and the population moment conditions are constructed taking the expectation of the following functions:

$$\begin{aligned} \mu_1(y, \mathbf{v}, \tau; \phi, \boldsymbol{\theta}_1, \theta_0, \eta_1) &= 1(\tau = 0)\left[n \frac{P\left(\bar{\mathbf{v}}^T \boldsymbol{\theta}_1\right)\theta_0}{\left[1 - P\left(\bar{\mathbf{v}}^T \boldsymbol{\theta}_1\right)\right](1 - \theta_0)} m(y) - \phi \eta_1\right] \\ \boldsymbol{\mu}_2(\mathbf{v}, \tau; \boldsymbol{\theta}_1) &= \left[\frac{\tau - P\left(\bar{\mathbf{v}}^T \boldsymbol{\theta}_1\right)}{P\left(\bar{\mathbf{v}}^T \boldsymbol{\theta}_1\right)\left[1 - P\left(\bar{\mathbf{v}}^T \boldsymbol{\theta}_1\right)\right]} p\left(\bar{\mathbf{v}}^T \boldsymbol{\theta}_1\right)\right] \bar{\mathbf{v}} \\ \mu_3(\tau; \theta_0) &= \left[(1 - \tau) - \theta_0\right] \\ \mu_4(n, \tau; \eta_1) &= 1(\tau = 1)\left[n - \eta_1\right] \end{aligned}$$

Bhattacharya (2002) shows that a consistent estimator for Γ_{dl} is

$$\begin{aligned}\hat{\Gamma}_{dl} &= \sum_{s=1}^{S}\sum_{i=1}^{n_s}\sum_{s_i=1}^{S_i}\sum_{h=1}^{m_{sis_i}} W_{sis_ih}\boldsymbol{\gamma}_{dl}\left(scs_ch\right) \\ \text{where } \gamma_{dl} &= \frac{\partial\mu_d\left(y_{scs_ch}, \mathbf{v}_{scs_ch}, \tau_{scs_ch}; \hat{\boldsymbol{\beta}}\right)}{\partial\boldsymbol{\beta}_l^T}\end{aligned}$$

and $W_{sis_ih} = \left(n_s m_{scs_c}\right)^{-1} H_s M\left(s, c, s_c\right)$ is the sampling weight. When $P\left(\bar{\mathbf{v}}^T\boldsymbol{\theta}_1\right)$ is estimated using a logit model, the sample equivalents of the μ functions become

$$\begin{aligned}\mu_1\left(y_{scs_ch}, \mathbf{v}_{scs_ch}, \tau_{scs_ch}; \widehat{\phi}, \hat{\boldsymbol{\theta}}_1, \hat{\theta}_0, \widehat{\eta}_1\right) &= 1\left(\tau_{scs_ch} = 0\right)\left[n_{scs_ch}R\left(\mathbf{v}_{scs_ch}; \hat{\boldsymbol{\theta}}_1, \hat{\theta}_0\right)1\left(y_{scs_ch} < z\right) - \widehat{\phi}\widehat{\eta}_1\right] \\ \boldsymbol{\mu}_2\left(\mathbf{v}_{scs_ch}, \tau_{scs_ch}; \hat{\boldsymbol{\theta}}_1\right) &= \left[\tau_{scs_ch} - P\left(\bar{\mathbf{v}}_{scs_ch}^T\hat{\boldsymbol{\theta}}_1\right)\right]\bar{\mathbf{v}}_{scs_ch} \\ \mu_3\left(\tau_{scs_ch}; \hat{\theta}_0\right) &= \left[\left(1 - \tau_{scs_ch}\right) - \hat{\theta}_0\right] \\ \mu_4\left(n, \tau_{scs_ch}; \widehat{\eta}_1\right) &= 1\left(\tau_{scs_ch} = 1\right)\left[n_{scs_ch} - \hat{\eta}_1\right]\end{aligned}$$

so that

$$\hat{\Gamma} = \begin{bmatrix} \hat{\gamma}_{11} & \hat{\boldsymbol{\gamma}}_{12} & \hat{\gamma}_{13} & \hat{\gamma}_{14} \\ \mathbf{0} & \hat{\boldsymbol{\gamma}}_{22} & \mathbf{0} & \mathbf{0} \\ 0 & \mathbf{0} & \hat{\gamma}_{33} & 0 \\ 0 & \mathbf{0} & 0 & \hat{\gamma}_{44} \end{bmatrix}$$

where (omitting, for simplicity, the subscripts)

$$
\begin{aligned}
\gamma_{11} &= -1(\tau = 0)\,\hat{\eta}_1 \\
\boldsymbol{\gamma}_{12} &= 1(\tau = 0)\,n\widehat{R}(\mathbf{v})\,m(y)\,\bar{\mathbf{v}}^T \\
\gamma_{13} &= 1(\tau = 0)\,n\frac{\hat{R}(\mathbf{v})}{\hat{\theta}_0\left(1-\hat{\theta}_0\right)}m(y) \\
\gamma_{14} &= -1(\tau = 0)\,\hat{\phi} \\
\boldsymbol{\gamma}_{22} &= -P\left(\bar{\mathbf{v}}^T\hat{\boldsymbol{\theta}}_1\right)\left[1-P\left(\bar{\mathbf{v}}^T\hat{\boldsymbol{\theta}}_1\right)\right]\bar{\mathbf{v}}\bar{\mathbf{v}}^T \\
\gamma_{33} &= -1 \\
\gamma_{44} &= -1(\tau = 1)
\end{aligned}
$$

The elements of the matrix W can be consistently estimated using the following expressions

$$
\begin{aligned}
\hat{W}_{ll} &= \sum_{s=1}^{S}\sum_{i=1}^{n_s}\left(m_{si}^l\right)^2 - \sum_{s=1}^{S}\frac{1}{n_s}\left[\sum_{i=1}^{n_s}m_{si}^l\right]^2 \\
\hat{W}_{lj} &= \sum_{s=1}^{S}\sum_{i=1}^{n_s}m_{si}^l m_{si}^j - \sum_{s=1}^{S}\frac{1}{n_s}\left[\sum_{i=1}^{n_s}m_{si}^l\right]\left[\sum_{i=1}^{n_s}m_{si}^j\right]
\end{aligned}
$$

where

$$
\underset{(1\times 1)}{m_{si}^l} = \sum_{s_i=1}^{S_i}\sum_{h=1}^{m_{sis_i}} W_{sis_ih}\mu_l\left(y_{sis_ih}, \mathbf{v}_{sis_ih}, \tau_{sis_ih}; \widehat{\phi}, \hat{\boldsymbol{\theta}}_1, \hat{\theta}_0, \widehat{\eta}_1\right)
$$

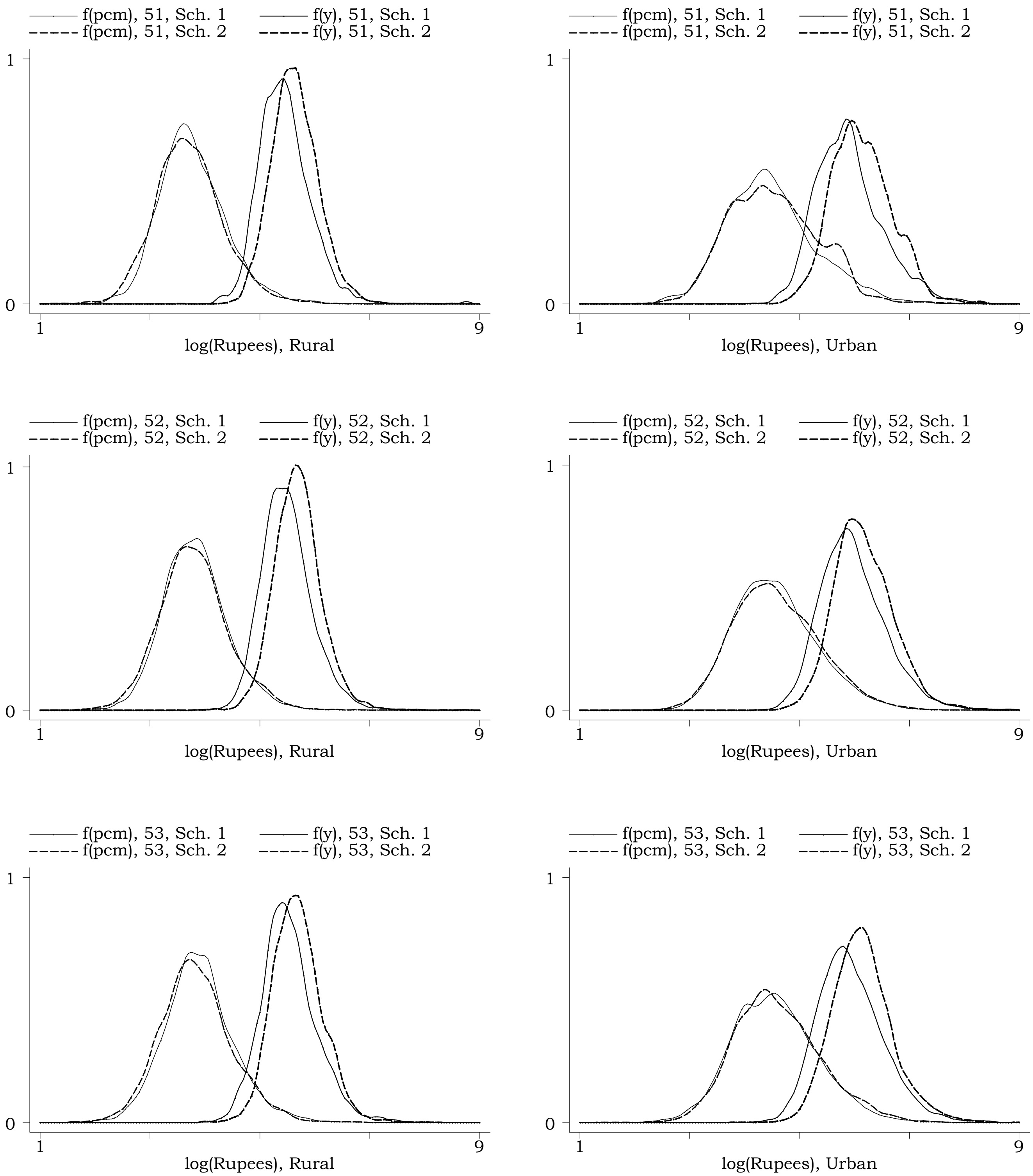
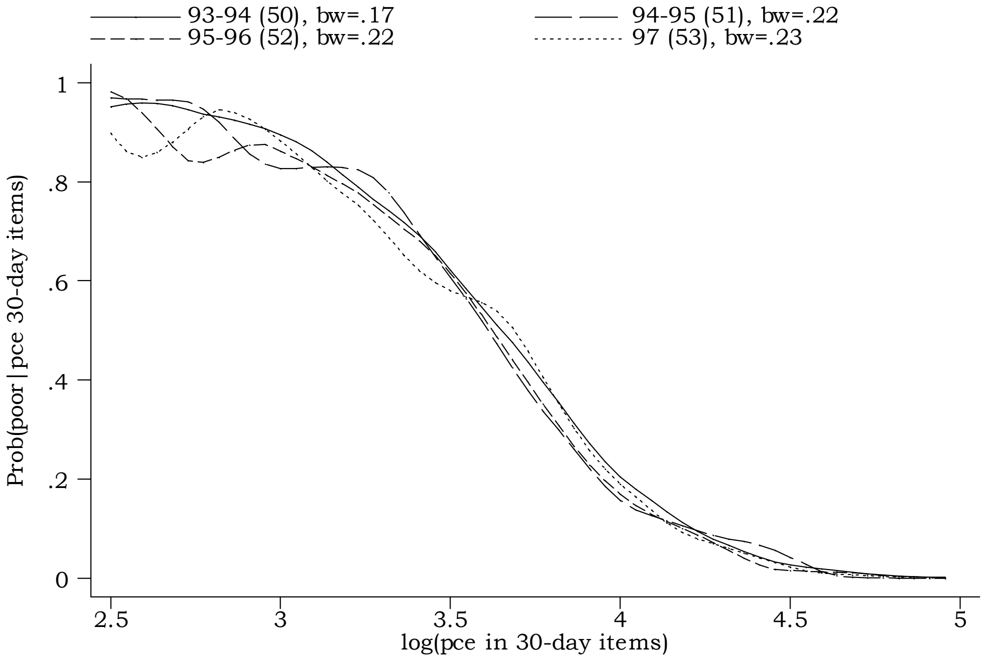
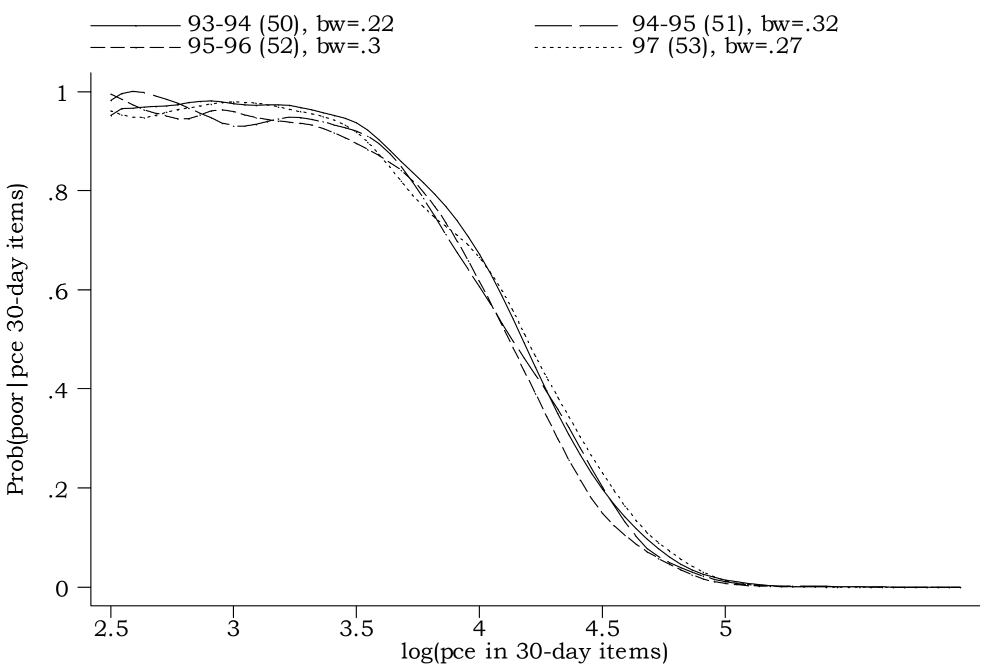


Figure 1 - Source: author's computation from NSS - All major Indian states. In each graph the two rightmost lines represent kernel estimates of the densities of total pce as measured with the two different schedules. The other two lines represent kernel estimates of the densities of pce in 30-day items. All values are in 1993-94 Rupees, deflated using CPIAL for the rural sector, and CPIIW for the urban sector. All densities are estimated using Silverman's robust bandwidth adapted for a biweight kernel, which is the one we use here.



Rural Sector



Urban Sector

Figure 2 - Conditional probability of being poor given log(PCE in 30-day items)
Source: author's computations from NSS, rounds 50-53, all major Indian States. Locally weighted regressions. The dependent variable is a dummy variable equal to one when the household's per capita monthly expenditure is below the sector-specific poverty line. The bandwidth used in each line is indicated in the corresponding label.

Table 1 - Summary statistics, Indian NSS Rounds 51-54

		NSS 51				NSS 52				NSS 53				NSS 54			
	Deflator is CPIIW for urban sector and CPIAL for rural sector	July 1994 - June 1995				July 1995 - June 1996				January-December 1997				January-June 1998			
		rural		urban		rural		urban		rural		urban		rural		urban	
	Schedule (Questionnaire Type)	S1	S2	S1	S2	S1	S2	S1	S2	S1	S2	S1	S2	S1	S2	S1	S2
(1)	Sample size (households)	13606	13415	9283	9214	12253	12047	8870	8749	12313	9214	16418	10555	8676	8545	2946	2911
(2)	Mean per capita total monthly expenditure	273.4	310.5	461.6	545.9	278.8	328.1	495.3	560.1	294.1	328.2	323.16	377.43	265.83	316.22	457.27	520.77
	s.e.	(13.17)	(3.45)	(18.78)	(20.05)	(3.91)	(3.35)	(10.84)	(7.54)	(3.25)	(3.21)	(5.21)	(5.86)	(2.98)	(2.78)	(14.44)	(21.35)
	t-ratio for equality across sch.	2.73		3.07		9.57		4.91		7.46		6.92		12.37		2.46	
(3)	Mean per capita expenditure in 30-day items (in 1993-94 Rs.)	54.4	50.8	131.4	133.4	57.9	56.6	130.4	138.0	61.9	58.9	91.8	96.3	58.0	57.3	131.8	143.2
	s.e.	(1.53)	(0.80)	(7.75)	(7.64)	(2.46)	(1.07)	(2.28)	(2.81)	(0.79)	(0.93)	(2.49)	(2.32)	(0.78)	(0.83)	(4.67)	(17.79)
	t-ratio for equality across sch.	2.09		0.18		0.48		2.10		2.46		1.34		0.56		0.62	
(4)	Mean budget share of 30-day items	20.1	16.0	26.3	21.7	20.3	16.4	26.1	22.4	21.1	17.3	27.3	23.4	21.6	17.4	28.2	22.8
	s.e.	(0.22)	(0.17)	(0.40)	(0.34)	(0.16)	(0.14)	(0.17)	(0.18)	(0.23)	(0.22)	(0.21)	(0.21)	(0.16)	(0.13)	(0.41)	(0.36)
(5)	Headcount Poverty Ratio	41.8	22.7	36.3	18.5	38.2	18.4	30.7	15.4	35.7	21.1	33.1	17.5	41.8	22.4	35.3	21.3

Source: author's computations from NSS. Robust standard errors in parenthesis. All values in 1993-94 Rupees. Only the major Indian states are included. All statistics are weighted using inflation factors. S1 is the standard questionnaire, with a 30-day reference period for all items. S2 is the experimental questionnaire, with a 7-day recall period for food and other high frequency items, and a 365day recall period for durables, clothing, footwear, educational and institutional medical expenses. The category "30-day items" includes: fuel and light, miscellaneous consumer goods and services, rents, consumer taxes and non-institutional medical expenses. The mean budget shares are averages of household-specific ratios between expenditure in 30-day items and total expenditure. The poverty counts are the proportion of individuals living in households where per capita expenditure is below the poverty line. The poverty lines are the official ones published by the Planning Commission for 1993-94, and are expressed in 1993-94 Rupees (the exchange rate at the time was approximately (1US$/32 Rs). The poverty line for All India is Rs 205.7 for the rural sector, and Rs 283.4 for the urban sector.

Table 2 – Tests for equality of distributions across schedules

(1) p-values	51st round		52nd round		53rd round	
	Rural	Urban	Rural	Urban	Rural	Urban
Household size	0.200	0.486	0.438	0.569	0.350	0.219
Education of head	0.837	0.651	0.577	0.533	0.014	0.208
Main activity	0.007	0.442	0.302	0.479	0.009	0.253
Scheduled caste/tribe	0.235	0.919	0.333	0.698	0.473	0.541
Land holdings	0.738	0.262	0.867	0.491	0.373	0.330

(2) Cross-tabulation of covariates for cells in panel (1) with p-value<0.05

	NSS 51, Rural		NSS 53, Rural			NSS 53, Rural	
	Sch. 1	Sch. 2	Sch. 1	Sch. 2		Sch. 1	Sch. 2
self-employed in non-agriculture	0.156	0.126	0.128	0.113	illiterate	0.490	0.486
agricultural labor	0.266	0.305	0.275	0.319	lit. no schooling	0.050	0.035
other labor	0.051	0.059	0.068	0.059	lit. below primary	0.114	0.095
Self-empl. in agr. and others	0.527	0.510	0.530	0.509	primary	0.131	0.141
					middle	0.111	0.133
					secondary	0.058	0.052
					above secondary	0.046	0.058

Source: author's computations from Indian NSS, rounds 51-53. All major Indian states.

The figures in panel (1) are p-values for Pearson chi-squared statistics corrected for the survey design. The null hypothesis is that, for every sector-round pair, the distribution of the selected variable is the same across the two different schedules. "Education of head" is one of the following: illiterate, literate with no schooling, literate below primary, primary, middle, secondary, above secondary. In the rural sector "Main activity of the household" is one of the following: self-employed in non-agriculture, agricultural labor, other labor, self-employed in agriculture and others; in the urban sector the categories are: self-employed, regular wage/salaried, casual labor, others. "Land holdings" are recorded as a categorical variable, with different codes for different intervals: code 1 is for land holdings below 0.01 acres, code 2 for the interval [0.01,0.2) and so on.

Table 3 - Tests for equality of distribution of log(pce in 30-day items) across schedules p-values - design based adjusted F-tests

NSS Round		obs.	number of bins: 10	15	20
51st - 7/94-6/95	Rural	26339	0.0281	0.0570	0.0931
	Urban	18168	0.0483	0.2698	0.1455
52nd - 7/95-6/96	Rural	23682	0.0426	0.0307	0.0770
	Urban	17224	0.4220	0.2770	0.6345
53rd - 1/97-12/97	Rural	20819	0.3555	0.3922	0.5637
	Urban	26119	0.4352	0.2231	0.2841

Source: author's computations from Indian NSS, rounds 51-53, all major Indian states.
The null hypothesis is that the distribution across bins is the same across the two different schedules. To avoid the presence of cells with very few observation, for every round-sector we use only observations included in the range between the first and the last centile of the round-sector specific distribution. All tests take into account the presence of clustering and stratification, except in the 51st round, for which we have no information on strata.

Table 4 - Tests for equality of conditional probabilities of being poor across different surveys

Surveys compared		50 - 51	50 - 52	50 - 53	51 - 52	51 - 53	52 - 53
		Rural Sector					
	obs.	72528	71175	71235	25859	25919	24566
m, household size[a]	$\chi^2(3)$	2.87	9.36	2.46	0.58	0.41	1.41
		[0.4126]	[0.0248]	[0.4830]	[0.9010]	[0.9391]	[0.7031]
Polynomials in m and hh. size	$\chi^2(7)$	48.51	13.2	14.32	20.94	24	3.66
		[0.0000]	[0.0674]	[0.0458]	[0.0039]	[0.0011]	[0.8182]
m, hh. size, controls[b]	$\chi^2(23)$	24.73	36.22	32.63	28.54	24.18	22.19
		[0.3642]	[0.0392]	[0.0878]	[0.1962]	[0.3941]	[0.5089]
Polynomial in hh. size, controls	$\chi^2(24)$	29.15	25.16	35.78	27.53	42.53	25.49
		[0.2145]	[0.3971]	[0.0576]	[0.2803]	[0.0113]	[0.3797]
Polynomial in m and h. size, controls	$\chi^2(27)$	80.77	37.16	38.88	48.04	53.18	23.81
		[0.0000]	[0.0922]	[0.0650]	[0.0076]	[0.0019]	[0.6407]
		Urban Sector					
	obs.	47995	47582	55130	18153	25701	25288
m, household size	$\chi^2(3)$	5.06	25.23	1.21	3.26	2.64	23.29
		[0.1672]	[0.0000]	[0.7512]	[0.3536]	[0.4500]	[0.0000]
Polynomials in m and hh. size	$\chi^2(7)$	5.91	24.44	7.82	4.82	6.22	27.72
		[0.5508]	[0.0010]	[0.3491]	[0.6822]	[0.5141]	[0.0002]
m, hh. size, controls	$\chi^2(23)$	33.04	44.15	39.86	31.18	28.17	41.08
		[0.0804]	[0.0050]	[0.0159]	[0.1185]	[0.2093]	[0.0116]
Polynomial in hh. size, controls	$\chi^2(24)$	36.34	32.12	39.71	41.45	29.48	36.82
		[0.0509]	[0.1241]	[0.0230]	[0.0149]	[0.2025]	[0.0456]
Polynomial in m and h. size, controls	$\chi^2(27)$	32.54	38.66	39.38	31.09	28.31	45.49
		[0.2127]	[0.0680]	[0.0585]	[0.2675]	[0.3952]	[0.0144]

Source: author's computations from NSS, rounds 50-53. Major Indian states only. The tests are robust to the presence of arbitrary heteroskedasticity and correlation within clusters. P-values in parenthesis.

[a] *m* is (log) per capita expenditure in miscellaneous items, in 93-94 Rs

[b] The controls are categorical variables for education of the household head, main economic activity of the household, whether the household belongs to a "scheduled caste or tribe", and land ownership.

Table 5 - Adjusted and unadjusted poverty rates in thin rounds using a logit first step

			(1)	(2)		(3)	(4)	(5)	(6)
			Schedule 1 (Benchmark)	Schedule 2 Unadjusted		**Auxiliary Survey** - Schedule 2 - adjusted			
			H1	H2		50	51 - Sch. 1	52 - Sch. 1	53 - Sch. 1
Target survey (Sch. 2)	NSS 51 - 94/95	Rural	41.8	22.7	m only	46.4 (2.83)**	45.6 (1.70)	45.3 (1.77)	44.6 (1.22)
		s.e.	(1.42)	(1.19)	all except m	38.0 (2.55)**	41.7 (0.13)	37.8 (2.31)*	35.3 (3.42)**
					all	45.5 (2.27)*	45.3 (1.55)	45.0 (1.65)	43.9 (0.9)
		Urban	36.3	18.5	m only	36.8 (0.27)	33.8 (0.94)	33.4 (1.33)	36.1 (0.06)
		s.e.	(1.88)	(1.34)	all except m	33.4 (1.37)	36.6 (0.14)	32.0 (2.04)*	35.0 (0.56)
					all	36.4 (0.08)	34.7 (0.6)	34.6 (0.77)	36.8 (0.28)
	NSS 52 - 95/96	Rural	38.2	18.4	m only	41.7 (3.43)**	41.0 (1.70)	39.7 (1.31)	39.9 (1.06)
		s.e.	(0.89)	(0.74)	all except m	38.4 (0.23)	41.6 (2.04)*	37.9 (0.16)	35.6 (1.63)
					all	41.7 (3.42)**	41.2 (1.8)	40.4 (1.67)	39.8 (1.02)
		Urban	30.7	15.4	m only	33.0 (2.27)*	30.7 (0.01)	30.4 (0.22)	33.3 (2.19)*
		s.e.	(0.83)	(0.69)	all except m	32.0 (1.31)	35.5 (2.6)**	30.7 (0.01)	33.8 (2.64)**
					all	32.6 (1.88)	31.2 (0.31)	30.8 (0.14)	33.6 (2.46)**
	NSS 53 - 97	Rural	35.7	21.1	m only	40.1 (3.22)**	39.4 (2.06)*	39.0 (2.12)*	38.3 (1.53)
		s.e.	(1.27)	(1.06)	all except m	37.3 (1.3)	40.9 (2.77)**	37.0 (0.95)	34.9 (0.34)
					all	39.9 (3.09)**	39.7 (2.19)*	39.0 (2.1)*	38.4 (1.57)
		Urban	33.1	17.5	m only	32.5 (0.90)	30.3 (1.88)	30.1 (3.01)**	32.9 (0.46)
		s.e.	(0.83)	(0.75)	all except m	31.8 (1.61)	35.1 (0.87)	30.3 (2.8)**	33.9 (0.33)
					all	32.1 (1.3)	30.7 (1.62)	30.3 (2.86)**	33.3 (0.14)

Source: author's calculations from NSS rounds 50-53. Only major Indian states are included. Columns 1 and 2 report robust standard errors in parenthesis. Columns 3 to 6 report in parenthesis the robust t-ratio for the null that the adjusted poverty ratio and the benchmark are the same. ** indicates rejection using 1% significant level, while * indicates rejection using 5% level.

[a] m is (log) per capita expenditure in miscellaneous items, in 93-94 Rs

[b] The controls are categorical variables for education of the household head, main economic activity of the household, whether the household belongs to a "scheduled caste or tribe", and land ownership.

Table 6 - Adjusted Poverty Counts - 55th NSS Round (1999-2000)

Auxiliary survey	(1)	(2)
Rural	m[a]	m and controls[b]
50 - 7/93-6/94	31.8 (0.34)	32.5 (0.35)
51 - 7/94-6/95	30.7 (1.05)	32.0 (1.09)
52 - 7/95-6/96	30.4 (0.77)	31.7 (0.80)
53 - 1/97-12/97	30.5 (1.05)	31.0 (1.07)
Urban		
50 - 7/93-6/94	25.9 (0.47)	26.1 (0.47)
51 - 7/94-6/95	24.9 (1.17)	27.1 (1.27)
52 - 7/95-6/96	24.0 (0.54)	25.0 (0.56)
53 - 1/97-12/97	26.5 (0.67)	27.3 (0.68)

Unadjusted Poverty Counts, only larger Indian States, Standard Questionnaire
Rural 28.4 (0.40), Urban 24.5 (0.58)

Source: author's computation from NSS, Rounds 50-51-52-53-55. Robust standard errors in parenthesis.
The poverty lines are the official ones for the 50th round (205.67 for the rural sector, and 283.44 for the urban sector). All monetary values from subsequent Rounds are deflated using state and sector specific official Consumer Price Indexes (CPIAL for households living in rural areas, and CPIIW for those living in urban areas). All estimates are computed for all major Indian states: Andhra Pradesh, Assam, Bihar, Gujarat, Haryana (urban only), Karnataka, Kerala, Madhya Pradesh, Maharashtra, Orissa, Punjab, Rajasthan, Tamil Nadu, Uttar Pradesh, West Bengal, Delhi (urban only). The adjusted poverty counts are estimates using the estimator developed in the paper, using a logit first step. In both columns, the logit first step also includes a polynomial in household size.

[a] m is (log) per capita expenditure in miscellaneous items, in 93-94 Rs
[b] The controls are categorical variables for education of the household head, main economic activity of the household, whether the household belongs to a "scheduled caste or tribe", and land ownership.