Measuring Economic Mobility and Inequality: Disentangling Real Events from Noisy Data

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Abstract

Estimates of economic mobility and inequality are highly sensitive to measurement error and transitory shocks in micro data. This paper corrects estimates of economic mobility and inequality for the effects of measurement error or transitory shocks using adaptations of instrumental variable methods and decompositions of income into transitory and persistent components. I demonstrate this methodology using household-level panel data for Russia and Poland in the mid-1990s. The results indicate that estimates of inequality that do not correct for noise in the data substantially overstate true inequality but that estimates of economic mobility are especially sensitive to noisy data, with well over half of the measured variance of annual income or expenditure shocks accounted for by measurement error or by transitory shocks.

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1. Introduction

Most empirical research has to face the question of whether the variables used are accurate measures of the quantities of interest. In the case of means of variables or relationships between variables, classical measurement error leads to less precise estimates or biases estimates of relationships between variables towards zero (for a discussion, see Griliches, 1986).[1] Here, classical measurement error leads to a "conservative" bias in the sense that it biases findings towards insignificance or a lack of relationship. In contrast, classical measurement error creates upward biases in estimated measures of dispersion (such as variances, inequality measures, or mobility measures). This bias can lead to spurious findings and unwarranted statistical significance. Of course, researchers in the inequality and mobility literature are well aware of this bias as witnessed for example by Baulch and Hoddinott's (2000) observation that due to measurement error "some of the observed movements in and out of poverty will be a statistical artifact."[2] Research dealing with inequality or mobility has done relatively little to mitigate the

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[1] Classical measurement error in some variable *X* is measurement error that is independent of the true value of *X*. Moreover, it is independent of the other variables in the analysis. Non-classical measurement leads to a host of additional problems beyond the scope of this article. Validation studies, however, have shown that some of the assumptions of classical measurement error are violated in U.S. data. In particular, measurement error seems to be negatively correlated with the true value of the variable reducing the upward bias in estimates of measures of dispersion. For details, see Duncan and Hill (1985), Bound and Krueger (1991), Bound, Brown, Duncan and Rogers (1994), and Pischke (1995). Hyslop and Imbens (2000) analyze non-classical measurement error arising when survey respondents are aware that their own knowledge is imperfect and give their best estimate to the question taking their knowledge imperfections into account.

[2] See Székely and Hilgert (1999) for an interesting discussion of measurement issues in the computation and comparability of inequality statistics in Latin America.

spurious effects of measurement error. To be fair, correcting for the effects of measurement error may often be impossible because of data limitations.

The aim of this paper is to develop two broadly applicable methodologies that make a significant stride towards eliminating the pernicious effects of measurement error in estimated measures of dispersion. I use these methodologies to estimate economic mobility and inequality in Russia and Poland in the mid 1990s. The first methodology decomposes the variable of interest (e.g. consumption expenditure) into two components: (i) a transitory shock, defined as a shock that only affects the current observation and (ii) a persistent component, which is simply the current observation purged of the transitory shock. The second methodology is an adapted instrumental variables (IV) technique that purges estimates of dispersion from the effects of measurement error. These two approaches are complements since they each have different data requirements, strengths and weaknesses. Conceptually related strategies have been used by Solon (1989) and Zimmerman (1992) to obtain more credible estimates of intergenerational earnings mobility, but their approaches are not directly applicable in the current context.

Transitory shocks need not be measurement error nor does measurement error need to show up as a transitory shock. Yet, many sources of measurement error in income or expenditure data, such as coding errors or recall errors, would be specific to one period and show up as a transitory shock. Moreover, often we are not interested in expenditure per se but rather in someone's economic well-being for which we take expenditure as a proxy. Similarly, income is interesting as a proxy for someone's ability to command economic resources. Many transitory fluctuations in measured expenditure do not translate in to fluctuations in economic well-being, such as expenditures related to a replacement of a consumer durable, a funeral or stocking up the food pantry. Other expenditures, especially purchases of consumer durables but also expenditure on items like vacations, insurance or medical care, show up as a large expenditure in the current period (month) but yield a consumption flow over many months or years to come. In these cases, fluctuations in expenditure exaggerate fluctuations in economic well-being.[3] Similarly, many fluctuations in income have little to do with one's command of economic resources, such as the timing of the receipt of a bonus, the receipt of wage arrears, or having a one-time opportunity to earn extra money, e.g. around harvest time. Hence, filtering out transitory shocks and concentrating on the persistent components of expenditure or income is likely to give one a better understanding of variation in true economic well-being or true command over economic resources than focusing on measured expenditure or income.

Decompositions of income or expenditure into persistent and transitory components date back at least until Friedman (1957). The decomposition presented in this paper identifies the type of shocks that can be estimated with minimal assumptions about the time series process generating the income or expenditure pattern. This method is driven by a desire to remain as close as possible to descriptive statistics and to limit the effects of modeling choices on the findings. Where assumptions about the time-series process are unavoidable, I make them explicitly and test the sensitivity of the results to these assumptions. I find that in 4-period panel data, the variance of transitory shocks in the middle two periods are identified as well as the variance of the persistent shocks to period 3. These shocks are persistent in the sense that they last at least two periods. They could possibly last longer than two periods but this cannot be inferred from 4-period panel data without making additional assumptions. To go beyond the

[3] A carefully constructed consumption aggregate, which imputes the consumption flow of durables and accurately takes fluctuations of (food) stocks into account, can reduce the amount of measured transitory shocks that are unrelated true consumption. In practice, however, the data often do not provide the necessary information to make these adjustments.

estimation of variances of transitory shocks, persistent shocks and persistent components of income or expenditure, I develop a simulation technique for the transitory shocks that only imposes a distributional form on the transitory shocks but not on the persistent component. Using this simulation technique, one can purge other inequality statistics (such as Gini coefficients) of the effects of transitory shocks. It also allows one to estimate transition probabilities into and out of poverty where the poverty definition is based on the persistent component of expenditure.

Instrumental variables are widely used to correct attenuation bias caused by measurement error in explanatory variables, but, to my knowledge, they rarely have been used to correct measures of dispersion from the spurious effects of measurement error.[4] I show formally that with two valid instruments, one can correctly estimate the dispersion in a variable of interest. An instrument is valid if it is correlated with the true value of the variable of interest but is uncorrelated with the measurement error in the variable of interest. Moreover, the components in both instruments that are orthogonal to the true value of the variable of interest must be uncorrelated with each other. This paper uses income and a measure of subjective material well-being to instrument for expenditure. One may be concerned that income is not a valid instrument for expenditure since certain types of measurement error in income and expenditure (such as willful underreporting) are likely to be correlated. One can show, however, that in this case the amount of measurement error would be underestimated. The IV technique yields estimates of the variance of measurement error and the variance of measurement-error corrected expenditure

[4] McCulloch and Baulch (2000) are a notable exception. They estimate an AR(1) model of income and determine by how much the estimated AR(1) coefficient changes when lagged income is instrumented with lagged consumption. They use this change to infer the variance of measurement error in lagged income.

and income measures. A simulation technique analogous to the one used in the transitory / persistent decomposition provides estimates of additional statistics of interest, such as the Gini coefficient of measurement-error corrected expenditure or the probabilities of movements into and out of poverty where poverty is based on expenditure purged of measurement error.

I apply these methodologies to Russian and Polish panel data because there are clear measurement issues in these datasets and because of the keen policy interest in inequality and mobility in transition economies in general (see, for example, Galasi, 1998, Spéder, 1998; Lokshin and Popkin, 1999; Okrasa, 1999a,b; World Bank, 2000 and Jovanovic, 2001). The transition from centrally planned economies to market economies has proven to be a difficult process that brought increases in inequality and poverty rates though the increases have been more dramatic in some transition countries than others (Milanovic, 1998; Rutkowski, 1998, Commander et al. 1999; Keane and Prasad, 2000). There is a belief that the process of transition inherently leads to short-term economic dislocations causing shallow poverty of a short-term nature. For example, writing about poverty in Russia between 1992 and 1993, Mroz and Popkin (1995) note that "poverty status was only a temporary state for the majority of households during this period." Transitory poverty (many people experiencing short poverty spells) calls for different policies than persistent poverty (a few experiencing long poverty spells). Transitory poverty can be alleviated by mechanisms that help households smooth their consumption over time, such as formal or informal insurance or loans. The long-run policy response to persistent poverty would be to improve the capacity of the poor to earn income, for example through schooling or by increasing opportunities for the poor in the economy (Lipton and Ravallion, 1995). In the shorter run, persistent poverty can be alleviated through social transfers. Given these important differences between persistent and transitory poverty, empirical evidence on the nature of poverty in transition economies will help inform the policy response.

The paper presents three main findings. First, the correction for transitory shocks and the correction for measurement error yield similar reductions in inequality as measured by the Gini coefficient. Eliminating transitory shocks or eliminating measurement error reduces the Gini coefficient by about 30% in Russia and by about 20% in Poland. This implies that the higher level of measured inequality in Russia compared to Poland can be partly explained by the higher level of measurement error in Russian data. Second, between 75% and 90% of the measured variance of annual shocks to income or expenditure is caused by transitory events that only occur in a single year. Most of these transitory events are due to measurement error, which accounts for 55% to 85% of the measured variance of annual income or expenditure shocks. Finally, the high levels of measured economic mobility are to a large degree driven by transitory events. Only a small minority of those appearing to escape poverty based on measured income or expenditure experience a lasting improvement in their economic fortune. Instead, most of them either are persistently poor and escaped (measured) poverty for just one period or are persistently non-poor and dipped into (measured) poverty for just one period. After accounting for such transitory mobility, around 80% of the poor in Russia and Poland remain in poverty for more than one year, pointing towards the existence of an underclass of long-term poor.

2. Methodology

A. Persistent versus transitory components of income and expenditure

I develop the model below to separate transitory shocks from persistent ones with minimal functional form assumptions.[5] The model identifies shocks purely from the time-series properties of household-level expenditure or income and does not distinguish between household-specific shocks and shocks operating at the community level (for such an analysis, see Stillman, 2001). Using four periods of data, one can in theory infer whether a shock lasts up to three periods: one needs the first two periods to measure a shock, and periods 3 and 4 indicate how much of the shock remains after two or three periods respectively. One could refer to the component of the shock that only shows up in period 2 as the transitory component and calculate what fraction of the shock persists for two or three periods. However, this implicitly assumes that the observation in period 1 is not subject to shocks itself – any transitory shock to the observation in period 1 would lead us to wrongly infer that a persistent shock occurred in period 2 that lasts at least three periods. It turns out that in order to account for such transitory shocks at the beginning of the observation period, one can only identify whether shocks last 2 periods (unless one resorts to functional form assumptions). This motivates the choice of the model below, which distinguishes two types of shocks: (a) transitory shocks, denoted by ε, that only affect the current period and (b) persistent shocks, denoted by η, that last at least two periods (and possibly more but, as explained above, this cannot be inferred from the data).[6]

[5] Blundell and Preston (1998) show that even without panel data one can separately identify the growth in variance of permanent and transitory income shocks because they have a different effect on the growth of consumption variance. Their strategy works in settings where capital markets are sufficiently developed to allow households to smooth consumption. Stillman (2001) shows that Russian households have a very limited ability to smooth consumption, so their strategy seems be less applicable in transition economies.

[6] Lillard and Willis (1978) and Gottschalk (1982) wrote seminal papers in the mobility literature that account for transitory shocks. There are differences in the definitions of transitory shocks. In Lillard and Willis (1978), the

The model is phrased in terms of log consumption expenditure but I also estimate it for log income. Log expenditure of household i in period t, C_{it}, consists of a persistent level, C_{it}^{P}, and a transitory shock, ε_{it}:

(1) $$C_{it} = C_{it}^{P} + \varepsilon_{it}$$

The ε-shocks are transitory in the sense that they only occur in one period and are orthogonal to past or future shocks. Conditional on the persistent expenditure component, the transitory shocks have mean zero and variance $\sigma_{\varepsilon_t}^2$. Because transition economies are undergoing a process of structural change, the variance of the shocks is allowed to vary over time. The persistent component of expenditure evolves subject to a common trend, α_t, and a persistent shock, η_{it}, which lasts at least two periods,

(2) $$C_{it}^{P} = C_{i,t-1}^{P} + \alpha_t + \eta_{it}$$

The trend, α_t, may vary over time but is the same for all households. The persistent shocks, η_{it}, have an unconditional mean of zero and variance of $\sigma_{\eta_t}^2$. Because the persistent component of expenditure contains the sum of all past persistent shocks and because persistent shocks more than two periods apart may be correlated, persistent shocks do not need to be orthogonal to the persistent component of expenditure.

The definition of transitory implies that the ε-shocks must be uncorrelated with future or past shocks whether or not they are transitory or persistent:

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transitory component is the serially correlated error term of an earnings function with random individual components. In Gottschalk (1982), any deviation from a linear time-trend in earnings is called transitory. The decomposition used here is most closely resembles that of Moffitt and Gottschalk (1993), except that the longer panel also allowed them to identify transitory shock that lasted for more than one period.

(3a) $E[\varepsilon_{it}, \varepsilon_{i,t-j}] = 0$ for $j \neq 0$

(3b) $E[\varepsilon_{it}, \eta_{i,t-j}] = 0$ for $j \neq 0$

Because the persistent component of expenditure consists of the sum of past persistent shocks, it follows from (3b) that the transitory shocks are orthogonal to the persistent component of expenditure. Since persistent shocks are defined to last at least two periods, they cannot be undone in expectation by next period's persistent shocks. This means that consecutive persistent shocks cannot be negatively correlated. I impose a somewhat stronger condition, namely that consecutive persistent shocks are uncorrelated:

(4) $E[\eta_{it}, \eta_{i,t-1}] = 0$

Because persistent shocks lying more than one period apart can be correlated, the time-series process for expenditure is more general than a random walk with noise. I discuss below how the results would be affected if consecutive persistent shocks were positively related. Finally, I use the identifying assumption that persistent shocks are uncorrelated with contemporaneous temporary shocks:

(5) $E[\eta_{it}, \varepsilon_{it}] = 0$

While this assumption cannot be tested, I will discuss below to what extent the main results hold if η_{it} and ε_{it} are correlated.

The model is estimated on a four-period panel using the methods of moments. The mean of the persistent expenditure component in the first period C_1^P, as well as the three common trends (α_2, α_3, and α_4) are estimated from the four sample first moments of the vector $\boldsymbol{C}=C_1|C_2|C_3|C_4$. The upper-diagonal elements of the variance-covariance matrix of $\boldsymbol{C}$ provide ten sample second moments which are equated to the ten corresponding second moments of the model. This yields the following estimators for the variance of transitory shocks in periods 2 and 3 and for persistent shocks in period 3:

(6a) $\sigma^2_{\varepsilon_2} = \boldsymbol{V}_{22} - \boldsymbol{V}_{12} - \boldsymbol{V}_{23} + \boldsymbol{V}_{13} = \mathrm{Cov}[C_2 - C_1, C_2 - C_3]$

(6b) $\sigma^2_{\varepsilon_3} = \boldsymbol{V}_{33} - \boldsymbol{V}_{23} - \boldsymbol{V}_{34} + \boldsymbol{V}_{24} = \mathrm{Cov}[C_3 - C_2, C_3 - C_4]$

and

(6c) $\sigma^2_{\eta_3} = \boldsymbol{V}_{12} + \boldsymbol{V}_{34} - \boldsymbol{V}_{13} - \boldsymbol{V}_{24} = \mathrm{Cov}[C_3 - C_2, C_4 - C_1]$

where $\boldsymbol{V}_{ij}$ denotes element (i,j) of the variance-covariance matrix of $\boldsymbol{C}$. The variances of persistent or transitory shocks in the other periods are not separately identified. As detailed in the footnote below, the seven remaining parameters that are identified concern the covariance between persistent shocks and the persistent level of expenditure in the first period, the covariance of persistent shocks more than one period apart and the sum of transitory and persistent shocks in periods 1 and 4.[7]

Since the variance of transitory shocks is only identified in periods 2 and 3, the analysis will focus on economic mobility between periods 2 and 3. The variance of the persistent expenditure component in these two periods is found by subtracting the variance of transitory

[7] The remaining seven parameters or parameter combinations that are identified are:

$(\sigma^2_{C^P_1} + \sigma^2_{\varepsilon_1}) = \boldsymbol{V}_{11}$

$(\sigma^2_{\eta_2} + \sigma^2_{\varepsilon_1}) = \boldsymbol{V}_{11} - \boldsymbol{V}_{13} - \boldsymbol{V}_{12} + \boldsymbol{V}_{23} = \mathrm{Cov}[C_1 - C_2, C_1 - C_3]$

$(\sigma^2_{\eta_4} + \sigma^2_{\varepsilon_4}) = \boldsymbol{V}_{44} - \boldsymbol{V}_{34} - \boldsymbol{V}_{24} + \boldsymbol{V}_{23} = \mathrm{Cov}[C_4 - C_2, C_4 - C_3]$

$(\sigma_{C^P_1,\eta_2} + \sigma^2_{\varepsilon_1}) = \boldsymbol{V}_{12} - \boldsymbol{V}_{11} = \mathrm{Cov}[C_1, C_2 - C_1]$

$\sigma_{C^P_1,\eta_3} = \boldsymbol{V}_{13} - \boldsymbol{V}_{12} = \mathrm{Cov}[C_1, C_3 - C_2]$

$\sigma_{C^P_1,\eta_4} = \boldsymbol{V}_{14} - \boldsymbol{V}_{13} = \mathrm{Cov}[C_1, C_4 - C_3]$

$\sigma_{\eta_2,\eta_4} = \boldsymbol{V}_{24} - \boldsymbol{V}_{23} - \boldsymbol{V}_{14} + \boldsymbol{V}_{13} = \mathrm{Cov}[C_2 - C_1, C_4 - C_3]$

shocks from the variance of measured expenditure ($\sigma^2_{C_2^P} = \mathbf{V}_{22} - \sigma^2_{\varepsilon_2}$ and $\sigma^2_{C_3^P} = \mathbf{V}_{33} - \sigma^2_{\varepsilon_3}$).

Finally, two correlations are of particular interest. First, the correlation between the persistent shock in period 3 and the persistent expenditure component in period 2 is a measure of the amount of mean reversion in persistent shocks. This correlation is given by:

(7) $$\rho_{C_2^P,\eta_3} = (\sigma^2_{C_3^P} - \sigma^2_{C_2^P} - \sigma^2_{\eta_3})/(2\sigma_{C_2^P}\,\sigma_{\eta_3})$$

Second, the correlation between the persistent components of expenditure is a measure of economic mobility purged of the effects of purely transitory shocks. This correlation is given by:

(8) $$\rho_{C_2^P,C_3^P} = \mathbf{V}_{23}/(\sigma_{C_2^P}\,\sigma_{C_3^P})$$

Standard errors on all parameter estimates and derived results are computed by bootstrapping the sample 250 times and calculating the standard deviation of the parameters and derived results across these 250 samples.

If the identifying assumption (5) is violated or if consecutive persistent shocks are correlated, the estimates of $\sigma^2_{\varepsilon_2}$, $\sigma^2_{\varepsilon_3}$ and $\sigma^2_{\eta_3}$ will be biased, and the bias is given by:

(9a) $$\text{Bias}(\hat{\sigma}^2_{\varepsilon_2}) = \text{E}[\varepsilon_2\ \eta_2] - \text{E}[\eta_2\ \eta_3]$$

(9b) $$\text{Bias}(\hat{\sigma}^2_{\varepsilon_3}) = \text{E}[\varepsilon_3\ \eta_3] - \text{E}[\eta_3\ \eta_4]$$

(9c) $$\text{Bias}(\hat{\sigma}^2_{\eta_3}) = \text{E}[\varepsilon_3\ \eta_3] - \text{E}[\varepsilon_2\ \eta_2] + \text{E}[\eta_2\ \eta_3] + \text{E}[\eta_3\ \eta_4]$$

As equations (9a-c) make clear, a positive covariance between contemporaneous transitory and persistent shocks (as seems most likely) would bias the estimate of the variance of transitory shocks upwards but would not bias the estimated variance of persistent shocks as long as this covariance is constant over time. A positive covariance between consecutive persistent shocks (as seems most likely) would bias the estimated variance of transitory shocks downwards and the estimated variance of persistent shocks upwards.[8]

Nothing mechanical guarantees that the parameter estimates take on admissible values, i.e. that estimates of variances are positive or that estimated correlations lie in the [-1,1] range. Indeed, any parameter estimate that takes on an inadmissible value constitutes a rejection of the model.[9] In the unbootstrapped data, all parameter estimates take on admissible values, both for income and expenditure and both in Russia and Poland. When the samples are bootstrapped to calculate standard errors, inadmissible parameter estimates appear in a small fraction of the samples. Because the simulation below is impossible for inadmissible parameter values, these samples are discarded for the standard error calculation. This fraction is below 1% in all cases. Given that this fraction is small, it should be taken as a reflection of sampling variation rather than as a rejection of the model.

B. Simulation of transitory shocks

Simulation of the transitory shocks allows one to calculate inequality indices (such as the Gini coefficient) for the persistent component of expenditure as well as transition probabilities into and out of poverty, where the poverty definition is based on persistent expenditure. Because the variances of the transitory shocks to the first and last periods are not identified, the paths of persistent and transitory income and expenditure can only be simulated for periods 2 and 3.

[8] A possible *negative* correlation between consecutive persistent shocks is not a concern because the part of a persistent shock that is undone by the next persistent shock is observationally equivalent to a transitory shock and I define it as such.

While the estimation of the variances of the various shocks did not rely on any distributional assumptions, in the simulations one needs to make an assumption about the distribution of transitory shocks (but not about the distribution of the persistent component). I assume that transitory shocks to log expenditure are normally distributed.[10] Because transitory shocks are uncorrelated with the persistent component of expenditure, they are correlated with measured expenditure. To incorporate this correlation, I simulate transitory shocks as a linear combination of three standard normal shocks:

(10a) $\tilde{\varepsilon}_2 = a_1\tilde{\psi}_2 + a_2\tilde{\psi}_3 + a_3\tilde{\xi}_2$

(10b) $\tilde{\varepsilon}_3 = b_1\tilde{\psi}_2 + b_2\tilde{\psi}_3 + b_3\tilde{\xi}_3$

where the tilde indicates simulated variables, $\tilde{\psi}_2$ is a standard normal variable with a perfect rank correlation with measured period 2 expenditure (C_2), $\tilde{\psi}_3$ is a standard normal variable with a perfect rank correlation with C_3, and ($\tilde{\xi}_2 \mid \tilde{\xi}_3$) is randomly drawn from a standard bivariate normal distribution with correlation coefficient r.[11] Hence, the simulated transitory shocks are determined by seven parameters, a_1 to a_3, b_1 to b_3 and r. These seven parameters are found by solving the following seven moment conditions:

(11a) $\operatorname{cov}[\tilde{\varepsilon}_2, C_2] = a_1 \operatorname{cov}[\tilde{\psi}_2, C_2] + a_2 \operatorname{cov}[\tilde{\psi}_3, C_2] = \hat{\sigma}^2_{\varepsilon_2}$

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[9] The following seven parameters or parameter combinations need to be tested for admissibility: (1) $\sigma^2_{\varepsilon_2} \geq 0$ (2) $\sigma^2_{\varepsilon_3} \geq 0$ (3) $\sigma^2_{\eta_2} \geq 0$ (4) $(\sigma^2_{\eta_2} + \sigma^2_{\varepsilon_1}) \geq 0$ (5) $(\sigma^2_{\eta_4} + \sigma^2_{\varepsilon_4}) \geq 0$ (6) $|\rho_{C_2^P, \eta_3}| \leq 1$ and (7) $|\rho_{C_2^P, C_3^P}| \leq 1$.

[10] As is explained below, it is possible that the observed distribution of expenditure is inconsistent with normally distributed transitory shocks. In that case, I assume that the shape of the distribution of the transitory shocks is a mixture between a normal distribution and the observed expenditure distribution.

[11] I simulate the transitory shock twenty times for each observation. This ensures that the final results are insensitive to stochastic variation introduced by simulation of transitory shocks.

$$\text{(11b)} \quad \operatorname{cov}[\tilde{\varepsilon}_2, C_3] = a_1 \operatorname{cov}[\tilde{\psi}_2, C_3] + a_2 \operatorname{cov}[\tilde{\psi}_3, C_3] = 0$$

$$\text{(11c)} \quad \operatorname{cov}[\tilde{\varepsilon}_3, C_3] = b_1 \operatorname{cov}[\tilde{\psi}_2, C_3] + b_2 \operatorname{cov}[\tilde{\psi}_3, C_3] = \hat{\sigma}^2_{\varepsilon_3}$$

$$\text{(11d)} \quad \operatorname{cov}[\tilde{\varepsilon}_3, C_2] = b_1 \operatorname{cov}[\tilde{\psi}_2, C_2] + b_2 \operatorname{cov}[\tilde{\psi}_3, C_2] = 0$$

$$\text{(11e)} \quad \operatorname{var}[\tilde{\varepsilon}_2] = a_1^2 + a_2^2 + 2a_1 a_2 \operatorname{cov}[\tilde{\psi}_2, \tilde{\psi}_3] + a_3^2 = \hat{\sigma}^2_{\varepsilon_2}$$

$$\text{(11f)} \quad \operatorname{var}[\tilde{\varepsilon}_3] = b_1^2 + b_2^2 + 2b_1 b_2 \operatorname{cov}[\tilde{\psi}_2, \tilde{\psi}_3] + b_3^2 = \hat{\sigma}^2_{\varepsilon_3}$$

$$\text{(11g)} \quad \operatorname{cov}[\tilde{\varepsilon}_2, \tilde{\varepsilon}_3] = a_1 b_1 + a_2 b_2 + (a_1 b_2 + a_2 b_1) \operatorname{cov}[\tilde{\psi}_2, \tilde{\psi}_3] + a_3 b_3 r = 0$$

where the carets indicate estimated parameters. Conditions (11a–d) ensure that the transitory shocks are orthogonal to the persistent expenditure component, conditions (11e-f) ensure that the transitory shocks have the correct variances and condition (11f) ensures that the transitory shocks are uncorrelated with each other. While a_1 and a_2 can always be solved from the first two equations and b_1 and b_2 from the next two equations, equations (11e) and (11f) might not yield real solutions for a_3 or b_3 and equation (11g) might give a solution for r outside the [-1,1] range. If this happens, it means that the data are inconsistent with normally distributed transitory shocks. In this case, I instead simulate transitory shocks as mixtures of a normal component and a component that has the same distribution as the observed expenditure distribution. In particular, I replace $\tilde{\psi}_2$ in the equations above by $\tilde{\psi}_2^*$ where

$$\text{(12)} \qquad \tilde{\psi}_2^* = \frac{\theta\tilde{\psi}_2 + (1-\theta) z_{C_2}}{\theta^2 + (1-\theta)^2 + 2\theta(1-\theta)\operatorname{cov}[\tilde{\psi}_2, z_{C_2}]}$$

where z_{C_2} is the z-score of observed expenditure in period 2, i.e. expenditure in deviation of its mean and divided by its standard deviation. The denominator of equation (12) merely ensures that the variance of $\tilde{\psi}_2^*$ equals one and the parameter θ defines the weight on the normal component. Similarly, $\tilde{\psi}_3$ is replaced by $\tilde{\psi}_3^*$, which is defined analogously. I numerically search for the highest value of θ for which admissible solutions for a_3, b_3 and r exist. This departure from normality always yielded admissible solutions to a_3, b_3 and r, both in the original income and expenditure data and in the bootstrapped samples where the model was not rejected. The simulated persistent component of expenditure is found by subtracting the simulated transitory shock from measured expenditure: $\tilde{C}_2^p = C_2 - \tilde{\varepsilon}_2$ and $\tilde{C}_3^p = C_3 - \tilde{\varepsilon}_3$. Finally, the simulated persistent shock, $\tilde{\eta}_3$, is simply the difference between the two simulated persistent expenditure components.

C. Measurement error and instruments

Most researchers recognize that measured consumption expenditure is only a rough proxy for the standard of living of a household. This raises the question to what extent variation in measured expenditure reflects true variation in the living standards of households rather than inaccuracies of the proxy. In principle, this question can be answered if one can find two instruments.[12] These instruments need to be correlated with true living standards, but also need to be uncorrelated with measurement error in measured expenditure. The intuition is simple: variation in true living standards tends to result in common movements in measured expenditure and the two instruments whereas measurement error leads to variation in measured expenditure that is unrelated to variation in the instruments. More formally, let the true, but unobserved, living standards of household i be denoted by C_i^c. I will refer to C_i^c as "measurement-error

[12] Note that McCulloch and Baulch (2000) implicitly also use two instruments for this period's income: next period's income (the dependent variable in their auxiliary AR(1) regression) and current consumption (the instrument in the auxiliary AR(1) regression).

corrected" or "corrected" expenditure. Measured expenditure, C_i, is decomposed into a corrected component and orthogonal measurement error:

(13) $C_i = C_i^c + u_i$

Measurement error u_i has a mean of zero and a variance of σ^2_u. At this point, one cannot tell whether movements in C_i are due to movements in true living standards, C_i^c, or simply due to movements in the error term u_i.

Let the two instruments for living standards be denoted by X_i and Y_i, where,

(14) $X_i = \alpha_0 + \alpha_1 C_i^c + v_i$

(15) $Y_i = \beta_0 + \beta_1 C_i^c + w_i$

where the errors terms, v_i and w_i, have zero means and variances given by σ^2_v and σ^2_w. For these instruments to be valid, they must be correlated with C_i^c (i.e. $\alpha_1 \neq 0$ and $\beta_1 \neq 0$). Moreover, the error terms of the instruments and measured expenditure must be mutually uncorrelated: $E[u_i v_i]=0$, $E[u_i w_i]=0$ and $E[v_i w_i]=0$.

To find the variance of corrected expenditure, $\sigma^2_{C^c}$, I first calculate the covariances between measured expenditure and the two instruments:

(16) $\mathrm{Cov}[C_i, X_i] \equiv \sigma_{CX} = \alpha_1 \, \sigma^2_{C^c}$

(17) $\mathrm{Cov}[C_i, Y_i] \equiv \sigma_{CY} = \beta_1 \, \sigma^2_{C^c}$

(18) $\mathrm{Cov}[X_i, Y_i] \equiv \sigma_{XY} = \alpha_1 \, \beta_1 \, \sigma^2_{C^c}$

where σ_{AB} denotes the covariance between two variables A and B. These three equations are solved for $\sigma^2_{C^c}$, which yields:

(19) $\sigma^2_{C^c} = \sigma_{CX} \, \sigma_{CY} \;/\; \sigma_{XY}$

Finally, the fraction of the variance of measured expenditure that is due to measurement error is calculated as:

(20) $\quad$ *Fraction measurement error in* $C = \sigma^2_u / \sigma^2_C = (\sigma^2_C - \sigma^2_{C^C}) / \sigma^2_C$

The fraction measurement error is calculated separately for each year in the panels.

The methodology to estimate the variance of changes in true living standards is analogous:

(21) $\quad \sigma^2_{\Delta C^C} = \sigma_{\Delta C\,\Delta X}\,\sigma_{\Delta C\,\Delta Y} / \sigma_{\Delta X\,\Delta Y}$

where Δ denotes the first-difference operator. The variance of measurement error in changes in expenditure is given by $\sigma^2_{\Delta u} = \sigma^2_{\Delta C} - \sigma^2_{\Delta C^C}$. The intertemporal correlation of measurement error (ρ_u) can be deduced by comparing this variance to the variance of measurement error in the levels of expenditure:

(22) $\quad \rho_u = (\sigma^2_{u_1} + \sigma^2_{u_2} - \sigma^2_{\Delta u}) / 2\sigma_{u_1}\sigma_{u_2}$

where $\sigma^2_{u_1}$ and $\sigma^2_{u_2}$ denote the variances of measurement error in periods 1 and 2.

I use two instruments for expenditure. The first instrument is a measure of subjective living standards and is based on a question in which respondents are asked to rate their standard of living on a discrete scale. This measure is correlated with consumption expenditure and it seems unlikely that it would be correlated with measurement error in measured expenditure. The fact that subjective living standards are measured as an ordinal variable (with 5 categories in the Polish data and 9 categories in the Russian data) makes the assumption that it is linearly related to true expenditure (as in equation 14) problematic. Instead, I adopt the familiar ordered probit model which treats the discrete response to the living standards question (X_i) as a function of a continuous latent variable (X_i^*):

$$(23) \qquad X_i = \begin{cases} 1 & \text{for} \quad X_i^* \le \mu_1 \\ 2 & \text{for} \quad \mu_1 < X_i^* \le \mu_2 \\ \vdots & \\ k & \text{for} \quad \mu_{k-1} < X_i^* \end{cases}$$

where the thresholds μ are parameters to be estimated. Without loss of generality, the variance of X_i^* is normalized to one. To estimate the covariance between X_i^* and some third variable Z_i, I run an ordered probit of X_i on Z_i. The ordered probit models X_i^* as a linear function of Z_i plus an error term: $X_i^* = Z_i \gamma + v_i$. Hence, the covariance between X_i^* and Z_i is given by:

$$(24) \qquad \sigma_{XZ} = \mathrm{E}[(Z_i - \mathrm{E}[Z_i])(Z_i \gamma + v_i)] = \gamma \,\mathrm{var}(Z_i),$$

where γ is estimated by the probit regression.[13]

The second instrument is household income. While this instrument is clearly related to underlying living standards, one may worry that measurement error in income is correlated with measurement error in expenditure. For example, households that systematically underreport income (e.g. due to forgetfulness or for fear of taxation) are likely to also underreport expenditure. Hence, this second instrument is chosen by lack of a better alternative.

Fortunately, it is possible to deduce how a correlation between measurement error in income and expenditure would affect the results. Such a correlation would lead to an upward bias in σ_{CY} (and the analogous expression for first differences). This would lead us to overestimate the fraction of the variance that can be explained by movements in true living

[13] In most software packages, the variance of the error term (v_i) rather than the variance of the latent variable (X^*_i) is normalized to one. In that case, one needs to divide the estimated γ coefficient by a factor of $\sqrt{\gamma^2 \mathrm{var}(Z_i) + 1}$. The variance of the latent variable needs to be normalized rather than the variance of the error term because only the former does not depend on the choice of explanatory variable. This is important because the normalization of X^* should be the same for the covariance calculation between X^* and C and as for the one between X^* and Y.

standards and to underestimate the part due to measurement error. Hence, all the estimates for fractions of measurement error should probably be treated as lower bounds. To assess measurement error in income, I run an IV procedure for income that is identical to the one for expenditure, except that the instruments are now subjective living standards and expenditure.

The formulas (20) and (22) above do not mechanically guarantee that the estimated fraction measurement error lies between zero and one or that the estimated correlation between consecutive measurement error lies in the [-1,1] range. Inadmissible parameter estimates constitute a rejection of the joint validity of the instruments. The validity of the instruments is never rejected in the original Russian and Polish income and expenditure data, nor in the bootstrapped Polish samples. However, in 4% of the bootstrapped Russian expenditure samples and in 18% of the bootstrapped Russian income samples the validity of the instruments is rejected. Bootstrapped samples in which the validity of the instruments is rejected are not used in the standard error calculation, because the simulation below is frequently impossible for these samples. Though these fractions are still low enough that they can plausibly be attributed to sampling variation, they do reduce my confidence in the validity of the instruments in Russia.

D. Simulation of measurement error

The estimation procedure described above decomposes the variance of measured expenditure into two components – the variance of measurement error and the variance of measurement-error corrected expenditure (henceforth referred to as "corrected" expenditure). These estimates do not require any assumptions about the shape of the distribution of measurement error. To calculate inequality indices for corrected expenditure or to estimate probabilities of corrected expenditure rising above or falling below the poverty line, one needs to simulate measurement error. This simulation is similar but not identical to the one described earlier for transitory shocks. I therefore also present this simulation technique in detail. To facilitate comparisons with the decomposition of expenditure into a persistent component and a transitory shock, which is only feasible for periods 2 and 3, measurement error is simulated for periods 2 and 3. I assume that measurement error in log expenditure is normally distributed.[14] By assumption, measurement error is uncorrelated with corrected expenditure. Since measured expenditure is the sum of corrected expenditure and measurement error, measurement error must therefore be correlated with measured expenditure. To allow for this correlation, I simulate measurement error as a linear combination of three standard normal shocks:

(25a) $\tilde{u}_2 = a_1\tilde{\psi}_2 + a_2\tilde{\psi}_3 + a_3\tilde{\xi}_2$

(25b) $\tilde{u}_3 = b_1\tilde{\psi}_2 + b_2\tilde{\psi}_3 + b_3\tilde{\xi}_3$

where the tilde indicates simulated variables, $\tilde{\psi}_2$ is a standard normal variable with a perfect rank correlation with measured period 2 expenditure (C_2), $\tilde{\psi}_3$ is a standard normal variable with a perfect rank correlation with C_3, and ($\tilde{\xi}_2 \mid \tilde{\xi}_3$) is randomly drawn from a standard bivariate normal distribution with correlation coefficient r.[15] Hence, the simulated measurement errors are determined by seven parameters, a_1 to a_3, b_1 to b_3 and r. These seven parameters are found by solving the following seven moment conditions:

(26a) $\text{cov}[\tilde{u}_2, C_2] = a_1 \text{cov}[\tilde{\psi}_2, C_2] + a_2 \text{cov}[\tilde{\psi}_3, C_2] = \hat{\sigma}^2_{u_2}$

[14] Just as was the case with the simulation of transitory shocks, it is possible that the observed distribution of consumption is inconsistent with normally distributed measurement error. In that case, I assume that the shape of the distribution of the measurement error is a mixture between a normal distribution and the observed expenditure distribution. Details are given below.

[15] Again, I simulate the measurement error twenty times for each observation in order to reduce the sensitivity of the final results to the stochastic variation introduced by the simulation.

(26b) $\text{cov}[\tilde{u}_2, C_3] = a_1 \text{cov}[\tilde{\psi}_2, C_3] + a_2 \text{cov}[\tilde{\psi}_3, C_3] = \hat{\rho}_u \hat{\sigma}_{u_2} \hat{\sigma}_{u_3}$

(26c) $\text{cov}[\tilde{u}_3, C_3] = b_1 \text{cov}[\tilde{\psi}_2, C_3] + b_2 \text{cov}[\tilde{\psi}_3, C_3] = \hat{\sigma}^2_{u_3}$

(26d) $\text{cov}[\tilde{u}_3, C_2] = b_1 \text{cov}[\tilde{\psi}_2, C_2] + b_2 \text{cov}[\tilde{\psi}_3, C_2] = \hat{\rho}_u \hat{\sigma}_{u_2} \hat{\sigma}_{u_3}$

(26e) $\text{var}[\tilde{u}_2] = a_1^2 + a_2^2 + 2a_1 a_2 \text{cov}[\tilde{\psi}_2, \tilde{\psi}_3] + a_3^2 = \hat{\sigma}^2_{u_2}$

(26f) $\text{var}[\tilde{u}_3] = b_1^2 + b_2^2 + 2b_1 b_2 \text{cov}[\tilde{\psi}_2, \tilde{\psi}_3] + b_3^2 = \hat{\sigma}^2_{u_3}$

(26g) $\text{cov}[\tilde{u}_2, \tilde{u}_3] = a_1 b_1 + a_2 b_2 + (a_1 b_2 + a_2 b_1) \text{cov}[\tilde{\psi}_2, \tilde{\psi}_3] + a_3 b_3 r = \hat{\rho}_u \hat{\sigma}_{u_2} \hat{\sigma}_{u_3}$

where the carets indicate estimated parameters. While a_1 and a_2 can always be solved from the first two equations and b_1 and b_2 from the next two equations, equations (26e) and (26f) might not yield real solutions for a_3 or b_3 and equation (26g) might give a solution for r outside the $[-1, 1]$ range. If this happens, it means that the data is inconsistent with normally distributed measurement error. In this case, I instead simulate measurement error as a mixture of a normal component and a component that has the same distribution as the measured expenditure distribution. In particular, I replace $\tilde{\psi}_2$ in the equations above by $\tilde{\psi}_2^*$ where

(27) $$\tilde{\psi}_2^* = \frac{\theta \tilde{\psi}_2 + (1-\theta) z_{C_2}}{\theta^2 + (1-\theta)^2 + 2\theta(1-\theta)\text{cov}[\tilde{\psi}_2, z_{C_2}]}$$

where z_{C_2} is the z-score of measured expenditure in period 2, i.e. expenditure in deviation of its mean and divided by its standard deviation. The denominator of equation (27) merely ensures that the variance of $\tilde{\psi}_2^*$ equals one and the parameter θ determines the weight on the normal component. Similarly, $\tilde{\psi}_3$ is replaced by $\tilde{\psi}_3^*$, which is defined analogously. I numerically search for the highest value of θ for which admissible solutions for a_3, b_3 and r exist. In the original income and expenditure data, this departure from normality always yielded admissible solutions to a_3, b_3 and r. Only in the bootstrapped data (used to determine standard errors) occasionally no admissible solutions to a_3, b_3 and r existed. This happened for less than 1% of the bootstrapping rounds in the Russian data and never happened in the Polish data.[16] The simulated measurement-error corrected level of expenditure is found by subtracting simulated measurement error from measured expenditure: $\widetilde{C}_2^c = C_2 - \widetilde{u}_2$ and $\widetilde{C}_3^c = C_3 - \widetilde{u}_3$.

3. Data

The data for Russia come from the Russian Longitudinal Monitoring Survey (RLMS), a nationally-representative socioeconomic survey of the Russian Federation. I use the balanced panel component of waves 5 through 8 which were fielded in the falls of 1994, 1995, 1996 and 1998. This yields a sample of 7,382 individuals in 2,256 households with complete demographic, income and expenditure information. More details about this dataset can be found in Lokshin and Popkin (1998) and on the website of the University of North Carolina at Chapel Hill (www.cpc.unc.edu/projects/rlms/project.html).

The Polish data consists of the 1993-1996 panel component of the Household Budget Survey conducted by the Polish Central Statistical Office. This nationally-representative survey is fielded throughout the year. I use a balanced panel with 16,552 individuals in 4,919 households with complete demographic, income and expenditure information. More details on this dataset can be found in Okrasa (1999a,b).

[16] Examination of the bootstrapping rounds in which the simulation failed showed that this only occurred when the estimate of the correlation between measurement-error corrected expenditure in periods 2 and 3 was extremely close to one. This places very special demands on the simulated distribution of measurement error in periods 2 and 3 because it means that observed expenditure minus simulated shocks in period 2 must be an almost perfectly linear transformation of observed expenditure minus simulated shocks in period 3.

Non-random attrition is a potentially serious problem. The University of North Carolina's website and Okrasa (1999a) analyze attrition in each of the data sets. They find that households with better economic positions and households in urban areas are more likely to drop out of the sample. It is hard to infer whether and how this pattern of attrition affects the results.

The main measures of economic well-being used in this paper are the logarithm of real monthly consumption expenditure and the logarithm of real monthly income. Both measures are adjusted for household size using an equivalence scale.[17] The income data includes the value of home production of agricultural products. In the Polish data, the recall period for all expenditure items is one month, while in the Russian data recall periods vary between one week for food expenditure, one month for services and utilities, and three months for clothes, shoes and durables. The expenditure data includes actual expenditure on durables rather than imputed rental values of these goods. Hence, one might worry that the finding that most shocks are transitory largely reflects sporadic purchases of durables or other lumpy goods. To address this concern, the total analysis was repeated using only food expenditures, which accounts for 47% of total expenditure in Russia and 42% of total expenditure in Poland. The dynamics of food expenditures are broadly similar to those for total expenditure as shown in appendix table A1. For the measurement of shocks, the second period of the 4 period panel is taken as the base period. Hence, the base period is the fall of 1995 in Russia and 1994 in Poland.

One variable of particular interest is the subjective measure of living standards because it is used as an instrument in all the IV estimates. In the Russian data, subjective living standards are measured by the question: *Please imagine a 9-step ladder where on the bottom,(the first*

[17] Following the World Bank (2000) report on poverty and inequality in transition countries, I use an equivalence scale in which equivalent household size = (number of household members)$^{0.75}$.

step), stand the poorest people, and on the highest step, (the ninth), stand the rich. On which step are you today? Ravallion and Lokshin (1999) show that total household income is a significant predictor of the answer to the subjective welfare question, although its explanatory power is low. In the Polish data, living standards are measured by the question: *How would you rate the general material situation of your household? Very good, good, average, rather bad or bad.* Both variables are treated as ordered categorical variables.

4. Results

A. Parameter estimates

Table 1 presents the parameter estimates of the decomposition of income and expenditure into transitory shocks and persistent components. Three findings stand out. First, the variance of measured log expenditure or income is about two to three times higher in Russia than in Poland, indicating a more unequal distribution in Russia. Second, the variance of income and expenditure shocks is considerable in both countries, suggesting high levels of economic insecurity. Third, the variance of the transitory shocks ($\sigma^2_{\varepsilon_3}$) is much larger than the variance of persistent shocks ($\sigma^2_{\eta_3}$), indicating that most of the shocks only affect a single period. Because the variance of the transitory shocks is a substantial fraction of the cross-sectional variance, estimated inequality will be much lower when based on the persistent expenditure component rather than on measured expenditure.

Table 1: Persistent/transitory decomposition: parameter estimates

Parameter	Symbol	Russia 1994-1998 Income		Russia 1994-1998 Expenditure		Poland 1993-1996 Income		Poland 1993-1996 Expenditure	
Variances of logs									
Period 1	σ^2_{C1}	0.736	(0.046)	0.678	(0.043)	0.335	(0.016)	0.227	(0.007)
Period 2	σ^2_{C2}	0.970	(0.051)	0.623	(0.027)	0.355	(0.016)	0.239	(0.006)
Period 3	σ^2_{C3}	1.699	(0.096)	0.769	(0.036)	0.338	(0.017)	0.240	(0.007)
Period 4	σ^2_{C4}	1.110	(0.067)	0.698	(0.029)	0.319	(0.014)	0.248	(0.007)
Variances of first differences of logs									
Period 2-1	$\sigma^2_{\Delta C2}$	1.138	(0.072)	0.678	(0.030)	0.321	(0.017)	0.174	(0.006)
Period 3-2	$\sigma^2_{\Delta C3}$	1.801	(0.105)	0.686	(0.030)	0.294	(0.020)	0.154	(0.006)
Period 4-3	$\sigma^2_{\Delta C4}$	1.950	(0.107)	0.785	(0.033)	0.293	(0.022)	0.161	(0.007)
Decomposition of shocks									
Var[persistent shock]	$\sigma^2_{\eta 3}$	0.130	(0.045)	0.059	(0.025)	0.028	(0.012)	0.022	(0.003)
Var[transitory shock 2]	$\sigma^2_{\varepsilon 2}$	0.560	(0.055)	0.256	(0.021)	0.135	(0.013)	0.065	(0.004)
Var[transitory shock 3]	$\sigma^2_{\varepsilon 3}$	1.111	(0.088)	0.371	(0.024)	0.131	(0.017)	0.067	(0.005)
Derived estimates									
Var[persistent component 2]	$\sigma^2_{C^p 2}$	0.409	(0.043)	0.367	(0.033)	0.220	(0.014)	0.174	(0.005)
Var[persistent component 3]	$\sigma^2_{C^p 3}$	0.588	(0.056)	0.398	(0.028)	0.207	(0.015)	0.172	(0.006)
Correlation[$C^p{}_2$, η_3]	ρ	0.106	(0.159)	-0.094	(0.167)	-0.265	(0.103)	-0.188	(0.046)
Fraction persistent		0.105	(0.036)	0.137	(0.053)	0.178	(0.069)	0.244	(0.032)
Sensitivity of "Fraction Persistent" to the assumption that η_t and ε_t are uncorrelated.									
Fraction. persistent if corr[η_t, ε_t]= 1.0		0.141		0.191		0.256		0.360	
Fraction persistent if corr[η_t, ε_t]= 0.2		0.111		0.147		0.192		0.265	
Fraction persistent if corr[η_t, ε_t]= -0.2		0.098		0.128		0.165		0.223	
Fraction persistent if corr[η_t, ε_t]= -1.0		0.077		0.097		0.120		0.155	
Sensitivity of "Fraction Persistent" to the assumption that consecutive persistent shocks are uncorrelated									
Fraction persistent if corr[η_t, η_{t-1}]= 1.0		0.036		0.050		0.067		0.097	
Fraction persistent if corr[η_t, η_{t-1}]= 0.2		0.076		0.102		0.134		0.187	
Weight (θ) on the normal component of the simulated transitory shock									
Weight on normal component, θ		0.686		1.000		0.958		1.000	

Note: Data consist of balanced panels, 1993-1996 for Poland and 1994-1998 for Russia. The unit of observation is the individual. All measures are for equivalent adults where the equivalent scale equals household size raised to the power of 0.75. Fraction persistent is the fraction of the innovation between period 2 and 3 that is persistent. This fraction is defined by the ratio of the variance of the persistent shocks in period 3 to the sum of the variances of the persistent and transitory shocks in period 3. Standard errors (in parentheses) are computed by bootstrapping the sample 250 times at the household level.

The bottom half of Table 1 explores the sensitivity of the estimate of the fraction of persistent shocks to two modeling assumptions. First, it shows that a positive correlation between contemporaneous persistent and transitory shocks leads to a downward bias in the estimate of the fraction of persistent shocks.[18] Hence, in this case, a larger part of shocks is persistent than the estimates show. However, as long as the correlation between transitory and persistent shocks takes on plausible values (say between –0.2 and 0.2), the bias in the fraction of persistent shocks remains less than 2 percentage points. Next, a positive correlation between consecutive persistent shocks results leads to an overestimate of the fraction of shocks that is persistent. Again, as long as this correlation remains reasonably small (say, 0.2), the resulting bias is about a quarter of the original estimate. Finally, the bottom row shows that in both countries the simulated transitory expenditure shocks are normally distributed and that the simulated transitory income shocks come from a mixing distribution with most of the weight on the normal component.

Table 2 presents the IV estimates of the fraction of the variance that is due to measurement error. The table shows that measurement error is responsible for a large fraction, typically 30%-60%, of cross-sectional variance. Measurement error accounts for an even larger share, generally around 55%-80%, of the variance of income and expenditure shocks. Measurement error seems to be about as important for expenditure as for income and is generally higher in Russia than in Poland.[19] In Poland measurement error in consecutive periods is

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[18] Assuming that the covariance between ε_t and η_t is constant over time, the estimate of the variance of persistent shocks does not depend on the correlation between persistent and transitory shocks, λ. However, the estimate of the variance of transitory shocks does depend on this correlation. Equation (6b) now becomes:

(6b′) $\sigma^2_{\varepsilon_3} + \lambda\, \sigma_{\varepsilon_3}\, \sigma_{\eta_3} = \boldsymbol{V}_{33} - \boldsymbol{V}_{23} - \boldsymbol{V}_{34} + \boldsymbol{V}_{24}$

This is a quadratic equation, which can be solved for σ_{ε_3}.

[19] I find the same pattern of measurement error in food expenditure, though measurement error in food expenditure tends to be higher than that in total expenditure. See appendix table A1 for details.

positively correlated with correlation coefficients around 0.35. Standard errors are too big to make precise inference about correlations in measurement error in Russia.

Table 2: Instrumental variable estimates of measurement error

	Russia 1994-1998		Poland 1993-1996	
	Income	Expenditure	Income	Expenditure
Fraction measurement error in:				
Logs, Period 1	0.573 (0.058)	0.435 (0.071)	0.411 (0.038)	0.337 (0.026)
Logs, Period 2	0.681 (0.059)	0.524 (0.057)	0.367 (0.038)	0.382 (0.025)
Logs, Period 3	0.560 (0.050)	0.518 (0.049)	0.338 (0.033)	0.395 (0.022)
Logs, Period 4	0.623 (0.043)	0.310 (0.064)	0.291 (0.036)	0.376 (0.022)
Log difference, period 2-1	0.794 (0.078)	0.756 (0.109)	0.538 (0.090)	0.680 (0.049)
Log difference, period 3-2	0.864 (0.116)	0.756 (0.184)	0.545 (0.064)	0.711 (0.046)
Log difference, period 4-3	0.703 (0.197)	0.775 (0.108)	0.477 (0.084)	0.725 (0.042)
Correlation between measurement error in consecutive periods				
Correlation, period 1 & 2	0.166 (0.078)	0.131 (0.129)	0.356 (0.102)	0.298 (0.048)
Correlation, period 2 & 3	0.070 (0.131)	0.243 (0.187)	0.345 (0.088)	0.412 (0.038)
Correlation, period 3 & 4	0.172 (0.229)	-0.034 (0.142)	0.328 (0.115)	0.380 (0.035)
Weight (θ) on the normal component of simulated measurement error				
Weight on normal component, θ	1.000	1.000	1.000	1.000

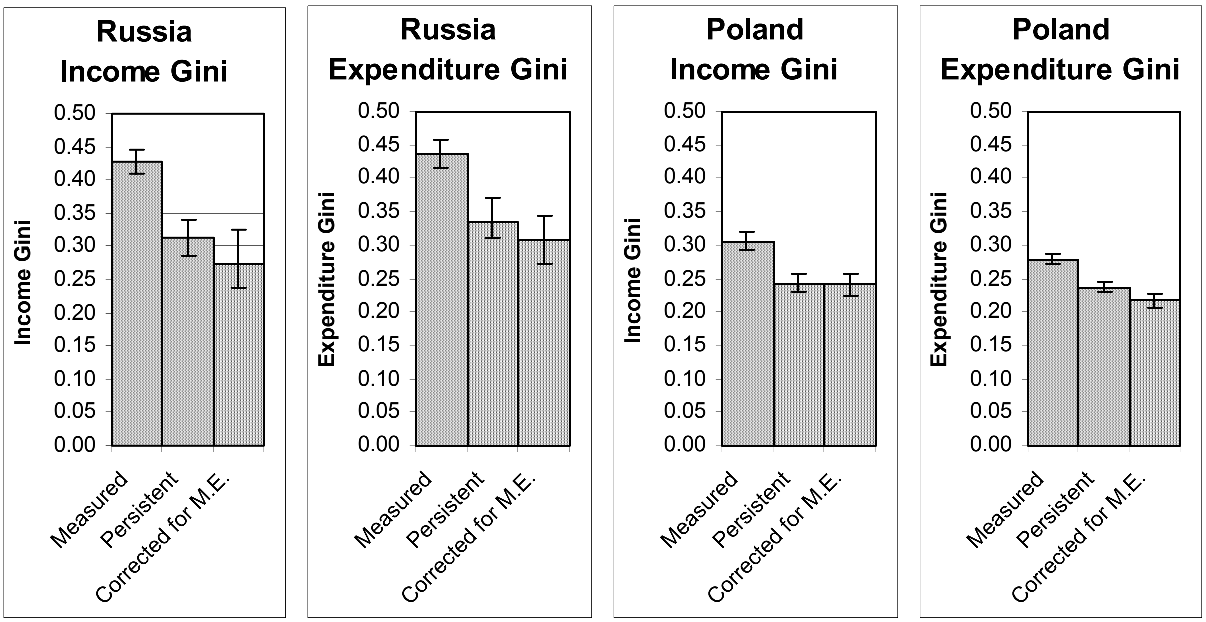
Note: Data consist of balanced panels, 1993-1996 for Poland and 1994-1998 for Russia. All measures are for equivalent adults where the equivalent scale equals household size raised to the power of 0.75. Standard errors (in parentheses) are computed by bootstrapping the sample 250 times at the household level.

This subsection presented the basic parameter estimates that will be used in the simulations of transitory shocks and measurement error. These simulations permit the estimation of additional statistics of interest such as the Gini coefficient and commonly used mobility measures such as poverty transition tables. The next subsections present the results from these simulations.

B. Inequality

Because of data availability, inequality in living standards is often measured by inequality in income or expenditure across individuals in a given month (or other short period of time). However, income or expenditure in a given month is only an imprecise measure of the living standard of a household because of measurement error and/or transitory events. This causes inequality in measured incomes or expenditures to overestimate inequality in underlying living standards.[20]

Figure 1: Gini coefficients after corrections for transitory shocks and measurement error



Note: Data refer to 1995 for Russia and 1994 for Poland. Measured Gini coefficients include transitory variation and measurement error while persistent Ginis are purged of any transitory variation and corrected Ginis are purged of measurement error. Instruments for log income are a measure of subjective living conditions and log expenditure. Instruments for log expenditure are a measure of subjective living conditions and log income. The calculations assume that measurement error in income, expenditure and subjective living conditions are independent. To the extend measurement error in income and expenditure is correlated, the corrected Ginis are biased upwards. Error bars denote 95% confidence intervals. Persistent and corrected Ginis are significantly different from measured Ginis in all cases.

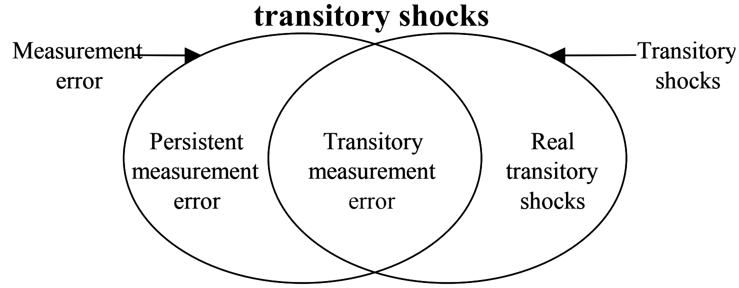
I use the two methodologies described earlier to purge income and expenditure from transitory shocks or from measurement error. The elimination of transitory shocks yields the Gini coefficients of persistent income and expenditure and the correction for measurement error yields the Gini coefficients of corrected income and expenditure. As Figure 1 shows, both the

[20] For example, Gibson, Huang and Rozelle (1999) calculate Gini coefficients for a sample of 232 Chinese urban households using both monthly and annual household expenditure. They find the Gini based on monthly expenditure is about 50% to 80% higher than the one based on annual expenditure, indicating that monthly expenditures are subject to many shocks that get averaged out over the year. This difference reduces to about 23% when the same calculation is done for income instead of consumption (Gibson, Huang and Rozelle, 2001).

removal of transitory shocks and the removal of measurement error causes a large and statistically significant drop in the Gini coefficient. The drop is about 30% in Russia and tends to be slightly higher when measurement error is removed than when transitory shocks are removed, but this difference is not statistically significant. The drop is about 20% in Poland, and for the expenditure data the drop in Gini is significantly larger when measurement error is removed than when transitory shocks are removed. Russia's large drop in the Gini in relation to Poland implies that differences in measured inequality between these two countries stem partly from the larger role of measurement error and transitory shocks in the Russian data.

It is striking that the correction for measurement error and the correction for transitory shocks yield very similar adjustments to the Gini coefficient. As Figure 2 illustrates, this need not be the case a priori; persistent measurement error is not removed when filtering out transitory shocks and real transitory shocks are not removed when filtering out measurement error. If persistent measurement error and real transitory shocks happen to have roughly equal variances, the effects of filtering out measurement error and of filtering out transitory shocks are similar. Alternatively this would be the case if most measurement error is transitory and most transitory shocks are measurement error. Estimates in Table 2 show that the correlation between measurement error

Figure 2: Overlap between measurement error and transitory shocks



in consecutive periods tends to lie in the neighborhood of 0.3, which would indicate that indeed most of the measurement error is transitory.[21]

C. Economic security

Following households over time also allows one to examine the stability, or security, of their economic situation. The raw data shows very large income and expenditure shocks, measured as percentage changes in deviation of the national mean between the reference month and the same month one year later.[22] For example, measured income increased to more than double or fell to less than half for over 40% of the population in Russia, while in Poland fluctuations of this magnitude happened to slightly more than 10% of the population. The standard deviation of measured income shocks is 134 log points in Russia and 54 log points in Poland. Fluctuations in measured expenditures are somewhat smaller, but still very large.

A large part of the fluctuations in measured income or expenditures can be attributed to transitory shocks or measurement error. The estimates in Table 1 show that 90% of the variance

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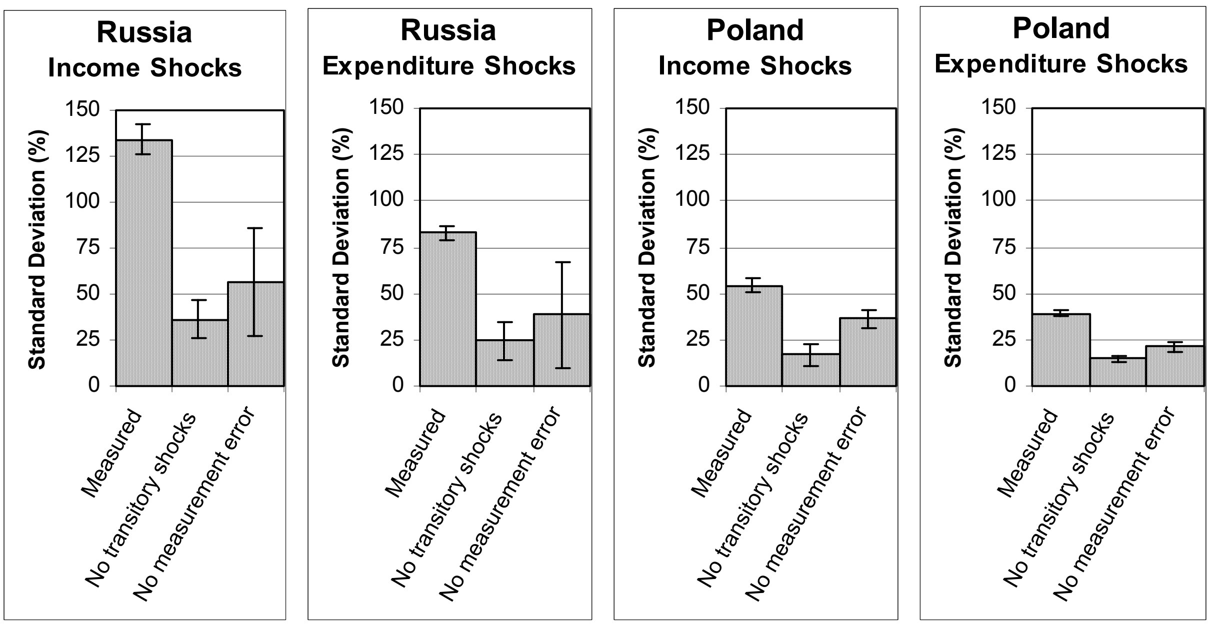
[21] I also tried to use the instrumental variable technique to estimate the fraction of transitory shocks that are measurement error, but the standard errors on the resulting estimates were too big for the estimates to be informative. Nevertheless, one can derive a lower bound for the fraction measurement error in transitory shocks by assuming that all persistent shocks are due to measurement error. Table 2 gives the fraction of measurement error in the total shock between period 2 and 3. Since persistent shocks are relatively small, one can calculate how much of this measurement error must have originated from the transitory components even if persistent shocks were completely due to measurement error. I thus find lower bounds for the fraction measurement error in transitory shocks ranging from 50% for income in Poland to 85% for income in Russia.

[22] The reference month is the month in which the household was observed in wave 2 of the survey. In Russia, wave 2 was fielded in November and December of 1995 while in Poland wave 2 was fielded between January and December of 1994.

of income shocks is transitory while 82% is transitory in Poland. The figures for expenditure shocks are 86% and 76%. This means that shocks are largely transitory, i.e. their effect will be largely undone within a year. For example, a Russian household who used to be earning 2000 Rubles per month and whose income increased to 3000 Rubles in the current month, should expect their income to fall back to 2100 Rubles in the same month one year from now. Moreover, the instrumental variable estimates indicate that well over half of the variance of income or expenditure shocks can be attributed to measurement error. Table 2 shows that in Poland about 55% of the variance of income shocks and 71% of the variance of expenditure shocks can be traced to measurement error. About 80% of the variance of income and expenditure shocks in Russia are due to measurement error, but this estimate is rather imprecise.

Since the estimates indicate that a substantial fraction of the shocks are transitory or are due to measurement error, Figure 3 compares the size of the measured shocks to those purged from transitory shocks or measurement error. Figure 3 shows that if the data is taken at face value, the standard deviation of Russian income shocks is a staggering 134 log points. However, if one only considers the permanent component, it drops to about 36 log points, while if measurement error is removed, it drops to about 57 log points. The pattern for expenditure shocks in Russia is similar. Measured income and expenditure shocks are smaller in Poland, but there too, removing the transitory component or measurement error leads to substantial drops. Hence, the figure indicates that living standards are more stable both in Russia and in Poland than the measured data would indicate. Nevertheless, even after correcting for measurement error or transitory shocks, individuals in Poland and especially in Russia face considerable fluctuations in living standards.[23]

Figure 3: Standard deviation of shocks after correction for transitory component and measurement error.



Note: Shocks are measured as log changes (times 100) and are in deviation of the national mean. Error bars denote 95% confidence intervals.

Unlike the results for inequality where the corrections for transitory shocks and measurement error had roughly the same effect, the correction for transitory shocks causes a

[23] I also estimated the size of income and expenditure shocks as well as their breakdown in transitory and permanent components for different demographic subgroups in both Russia and Poland. I found very few cases where the size or the composition of the shocks for a subgroup differed significantly from the overall mean. The main exceptions are that in both Russia and Poland, individuals with access to land face larger shocks that tend to be less permanent, but these differences are only statistically significant in Poland. In Poland, the size of the shock (as a fraction of expenditure) increases with income, while it displays a U-shaped pattern in Russia. In both countries, households with household heads aged 51-64 with higher education face the smallest shocks, but this difference is only significant in Poland.

notably larger decrease than the correction for measurement error. This should come as no surprise – the correction for transitory shocks both removes transitory shocks that are real and those that are due to measurement error, whereas the correction for measurement error only removes measurement error that is transitory. Indeed, if measurement error is the same in two consecutive periods, it will be differenced out when one examines changes.

D. Economic mobility and poverty transitions

In face of the sizeable income and expenditure shocks, one might wonder how long-lasting economic positions are. Do the rich generally remain rich and the poor remain poor, or do individuals frequently switch positions? While there are many measures of mobility in the empirical literature (for overviews, see Atkinson, Bourguignon and Morrison, 1992, or Birdsall and Graham, 2000), a simple measure of the degree to which individuals keep their position in the income distribution is the correlation between this period's income and the next period's. Table 3 shows these correlations for income and expenditures in Russia and Poland.

Panel A of Table 3 shows the correlation between current log income or log expenditure, and its value in future periods. The correlation between a household's current economic situation and that 12 months from now tends to be slightly less than 50% in Russia and somewhat more than 50% in Poland. This would suggest a lot of mobility. However, these correlations fall only very little if one moves out one, two or three extra years. The explanation for this pattern of correlations is that the correlation between any two years is less than unity for two reasons: (i) the transitory shocks (real or due to measurement error) that occur in each of the two years, and (ii) persistent shocks between the two years. Whether one compares the correlation between incomes that lie one, two or three years apart, the amount by which the correlation is reduced below unity due to transitory shocks is about the same. Hence, the small amount by which these correlations fall as one compares incomes that lie one year apart to incomes that lie 2 years apart indicates that persistent mobility is very low.

Table 3. Correlations in income and expenditure

	Russia				**Poland**			
	Income		Expenditure		Income		Expenditure	
A. Correlations in measured income or expenditure								
Between month 0 and month 12	0.336	(.026)	0.479	(.023)	0.536	(.021)	0.628	(.011)
Between month 0 and month 24	0.254	(.019)	0.431	(.024)	0.549	(.025)	0.626	(.015)
Between month 0 and month 36	..	..	..	..	0.457	(.020)	0.569	(.013)
Between month 0 and month 48	0.266	(.029)	0.380	(.021)	..	..	..	..
B. Correlations between month 12 and 24								
Measured	0.338	(.027)	0.510	(.019)	0.575	(.024)	0.679	(.011)
Persistent (purged of transitory shocks)	0.884	(.038)	0.924	(.031)	0.934	(.027)	0.938	(.009)
Corrected (purged of measurement error)	0.869	(.196)	0.739	(.189)	0.701	(.043)	0.848	(.024)

Notes: Equivalent income and equivalent expenditure are measured in logarithmic form. In panel A, the current month is taken from the first of the 4 waves of the data. The first wave of the Russian data was collected in November/December of 1994 and the first wave of the Polish data was collected between January and December of 1993. Correlations between persistent income/expenditure can only be calculated for waves 2 and 3 of the data. In both countries, wave 2 was collected 1 year after the first wave, and wave 3 was collected 2 years after the first wave. Standard errors (in parentheses) are computed by bootstrapping the sample 250 times at the household level.

More formally, I calculate the correlation between the persistent components of income or expenditure as explained in section 2. As panel B of Table 3 shows, the autocorrelation in the persistent component of income or expenditure ranges from 88% for income in Russia to 94% percent for expenditure in Poland, suggesting that there is relatively little switching of underlying economic fortunes. The estimates of the autocorrelation in income or expenditure purged of measurement error confirm this conclusion; the autocorrelation in expenditure or income increases substantially when measurement error is removed. This increase is somewhat lower than the increase due to removal of transitory shocks because the measurement error correction does not remove real transitory shocks.

An especially important form of mobility is the extent to which the poor can escape poverty. To facilitate comparisons of mobility into and out of poverty in Russia and Poland, I chose a poverty line such that in each year 20% of the population in each country is considered poor. I present results for poverty based on household expenditures, but the findings for income-based poverty are qualitatively the same. Poverty status is based on measured expenditure in a given month ("traditionally measured" poverty), based on the persistent component of expenditure in that month ("persistent" poverty) or based on measurement-error corrected expenditure ("corrected" poverty). Table 4 shows the flows into and out of poverty for these three poverty measures. Panel A shows a lot of movement into and out of measured poverty – 56% of the Russian poor and 39% of the Polish poor are no longer measured to be poor one year later. If the instantaneous probability of leaving poverty is constant over time (i.e. an exponential duration distribution), this translates into poverty spells that last on average 1.2 years in Russia and 2.0 years in Poland suggesting that poverty is fairly short term.

However, for poverty based on the persistent component of expenditure (shown in panel B), mobility is much lower – now only 21% of the Russian poor and 19% of the Polish poor have escaped poverty one year later. If spells durations are distributed exponentially, this translates into average poverty spells of 4.3 and 4.9 years respectively, which indicates a higher incidence of long-term poverty. This sharp difference between measured poverty and persistent poverty is explained by the prevalence of transitory shocks. For example, in Russia only 11% of the people who escape poverty based on their measured expenditure also saw their persistent expenditure component rise above the poverty line. The vast majority of people escaping measured poverty do not experience a lasting improvement of their economic position. Instead, they are either persistently non-poor but had a negative transitory shock in the first period (48% of the total) or are persistently poor but had a positive transitory shocks in the second period (35% of the total). Hence, 83% of the movements out of measured poverty are explained by transitory shocks – events that only affect one period.[24] This shows that care should be taken not to interpret transition matrices as part of a first-order Markov process since most of the movements will be undone in the subsequent period (see also Shorrocks, 1976).

In panel C, poverty is based on expenditure purged of measurement error. Since this expenditure measure includes real transitory shocks but excludes movements due to measurement error, it shows movements into and out of poverty that lie between the estimates of panels A and B. Excluding measurement error, I find that 37% of the Russian poor and 30% of the Polish have escaped poverty one year later. This translates into average poverty spells of respectively 2.2 and 2.9 years –substantially longer than the corresponding figures for traditionally measured poverty. Though these spells are not quite as long as the poverty spells excluding all transitory shocks, they do point towards the long-term nature of much of poverty in these countries.

[24] The remaining 6% consist of those whose persistent poverty status fell below the poverty line but whose traditionally measured status rose above it. These percentages are derived from appendix Table A2. Appendix Table A2 presents the 4x4 joint probability distributions of measured and persistent poverty in the current period and in the next period for Russia while appendix Table A3 presents it for Poland. In addition, these tables report joint probability distributions for measured and corrected poverty.

Table 4. Flows into and out of poverty

A. Movements in "Traditionally Measured" Poverty

(Poverty defined as the bottom quintile of the expenditure distribution)

Russia (1995-96)

Poverty status 12 months ago	This month's poverty status	
	Poor	Non-poor
Poor	0.440 (.023)	0.560 (.023)
Non-poor	0.140 (.006)	0.860 (.006)

Poland (1994-95)

Poverty status 12 months ago	This month's poverty status	
	Poor	Non-poor
Poor	0.609 (.014)	0.391 (.014)
Non-poor	0.098 (.004)	0.903 (.004)

B. Movements in "Persistent" Poverty

(Poverty defined as the bottom quintile of expenditure purged of transitory shocks)

Russia (1995-96)

Poverty status 12 months ago	This month's poverty status	
	Poor	Non-poor
Poor	0.793 (.055)	0.207 (.055)
Non-poor	0.052 (.014)	0.948 (.014)

Poland (1994-95)

Poverty status 12 months ago	This month's poverty status	
	Poor	Non-poor
Poor	0.815 (.015)	0.185 (.015)
Non-poor	0.045 (.004)	0.955 (.004)

C. Movements in "Corrected" Poverty

(Poverty defined as the bottom quintile of expenditure purged of measurement error)

Russia (1995-96)

Poverty status 12 months ago	This month's poverty status	
	Poor	Non-poor
Poor	0.634 (.125)	0.366 (.125)
Non-poor	0.092 (.031)	0.909 (.031)

Poland (1994-95)

Poverty status 12 months ago	This month's poverty status	
	Poor	Non-poor
Poor	0.705 (.024)	0.295 (.024)
Non-poor	0.073 (.006)	0.927 (.006)

Note: Poverty is measured by equivalent expenditure where the equivalence scale is household size raised to the power of 0.75. The poverty line is such that the poverty rate is 20% in all years. Standard errors (in parentheses) are computed by bootstrapping the sample 250 times at the household level.

5. Conclusion

This paper developed revised estimates of economic inequality and mobility by accounting for the effects of transitory shocks or measurement error. These corrections yield substantial downward adjustments of estimates of economic inequality and mobility. The corrections for transitory shocks and for measurement error yield similar and large reductions in inequality estimates. Each resulted in approximately a 30% reduction in the estimated Gini coefficient in Russia and approximately a 20% reduction in Poland. For estimates of mobility, the correction for transitory shocks tends to lead to a bigger reduction than the correction for measurement error. I find that between 75% and 90% of the measured variance of annual shocks to income or expenditure is caused by transitory events that are specific to a single year. Most of these transitory events are due to measurement error, which accounts for 55% to 85% of the measured variance of annual income or expenditure shocks. Finally, the high levels of measured economic mobility are to a large degree driven by transitory events. After accounting for transitory shocks, around 80% of the poor in Russia and Poland remain in poverty for more than one year. Measurement-error corrected mobility is slightly higher with approximately 65% to 70% of the poor remaining in poverty for more than a year. Nevertheless, both indicate that a concern about an underclass of long-term poor is warranted.

Though transitory shocks and measurement error are conceptually distinct, there is large overlap in practice – in the Russian and Polish data most transitory shocks are due to measurement error and most measurement error is transitory. Hence, when no instruments are available for a measurement error correction, a correction for transitory shocks may yield a reasonable approximation.

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Appendix

Table A1. Dynamics of food expenditure versus total expenditure

Parameter	Symbol	Russia 1994-1998 Total Expenditure		Russia 1994-1998 Food expenditure only		Poland 1993-1996 Total Expenditure		Poland 1993-1996 Food expenditure only	
Variances of logs									
Period 2	σ^2_{C2}	0.623	(.023)	1.036	(.050)	0.239	(.006)	0.141	(.004)
Period 3	σ^2_{C3}	0.769	(.036)	1.110	(.053)	0.240	(.008)	0.127	(.003)
Variances of first differences of logs									
Period 3-2	$\sigma^2_{\Delta C3}$	0.686	(.032)	1.059	(.050)	0.154	(.006)	0.094	(.003)
Decomposition of shocks									
Var[persistent shock]	$\sigma^2_{\eta 3}$	0.059	(.024)	0.088	(.036)	0.022	(.003)	0.016	(.002)
Var[transitory shock 2]	$\sigma^2_{\varepsilon 2}$	0.256	(.020)	0.477	(.043)	0.065	(.004)	0.044	(.003)
Var[transitory shock 3]	$\sigma^2_{\varepsilon 3}$	0.371	(.026)	0.494	(.038)	0.067	(.005)	0.034	(.002)
Fraction persistent		0.137	(.051)	0.150	(.055)	0.244	(.032)	0.318	(.031)
Fraction measurement error in:									
Logs, Period 2		0.524	(.057)	0.848	(.032)	0.382	(.025)	0.696	(.020)
Logs, Period 3		0.518	(.049)	0.793	(.035)	0.395	(.022)	0.691	(.021)
Log difference, period 3-2		0.755	(.184)	0.926	(.116)	0.711	(.045)	0.862	(.028)

Note: All measures are for equivalent adults where the equivalent scale equals household size raised to the power of 0.75. Data consist of balanced panels, 1993-1996 for Poland and 1994-1998 for Russia. Fraction persistent is the fraction of the innovation between period 2 and 3 that is persistent. This fraction is defined by the ratio of the variance of the persistent shocks in period 3 to the sum of the variances of the persistent and transitory shocks in period 3. Standard errors (in parentheses) are computed by bootstrapping the sample 250 times at the household level.

Table A2. Joint distribution by current and past poverty status in Russia

Population Fractions by Current and Past Poverty Status.
Effect of transitory component of expenditure.

Poverty Status 12 months ago		Current Poverty Status: Measured poor: "Persistent" poor	Measured poor: "Persistent" non-poor	Measured non-poor: "Persistent" poor	Measured non-poor: "Persistent" non-poor	Row Sum
Measured poor	"Persistent" poor	0.063	0.006	0.039	0.013	0.120
	"Persistent" non-poor	0.006	0.013	0.006	0.054	0.080
Measured non-poor	"Persistent" poor	0.032	0.007	0.025	0.016	0.080
	"Persistent" non-poor	0.014	0.060	0.015	0.632	0.720
	Column Sum:	0.115	0.085	0.085	0.715	1.000

Population Fractions by Current and Past Poverty Status.
Effect of measurement error in expenditure.

Poverty Status 12 months ago		Current Poverty Status: Measured poor: "Corrected" poor	Measured poor: "Corrected" non-poor	Measured non-poor: "Corrected" poor	Measured non-poor: "Corrected" non-poor	Row Sum
Measured poor	"Corrected" poor	0.050	0.010	0.026	0.024	0.110
	"Corrected" non-poor	0.011	0.018	0.007	0.056	0.091
Measured non-poor	"Corrected" poor	0.026	0.007	0.027	0.031	0.091
	"Corrected" non-poor	0.026	0.054	0.029	0.600	0.709
	Column Sum:	0.112	0.088	0.088	0.712	1.000

Note: Poverty is based on equivalent expenditure where the equivalence scale is household size raised to the power of 0.75. The poverty line is such that the poverty rate is 20% in all years. Measured poor are those whose measured equivalent expenditure in the current year falls below the poverty line. "Persistent" poor are those whose expenditure purged of transitory shocks (i.e. the persistent expenditure component) falls below the poverty line. "Corrected" poor are those whose expenditure purged of measurement error (i.e. corrected expenditure) falls below the poverty line.

Table A3. Joint distribution by current and past poverty status in Poland

Population Fractions by Current and Past Poverty Status.
Effect of transitory component of expenditure.

Poverty Status 12 months ago		Current Poverty Status: Measured poor: "Persistent" poor	Measured poor: "Persistent" non-poor	Measured non-poor: "Persistent" poor	Measured non-poor: "Persistent" non-poor	Row Sum
Measured poor	"Persistent" poor	0.093	0.007	0.024	0.009	0.133
	"Persistent" non-poor	0.007	0.012	0.006	0.038	0.063
Measured non-poor	"Persistent" poor	0.024	0.007	0.019	0.014	0.063
	"Persistent" non-poor	0.008	0.039	0.015	0.678	0.741
	Column Sum:	0.132	0.066	0.064	0.738	1.000

Population Fractions by Current and Past Poverty Status.
Effect of measurement error in expenditure.

Poverty Status 12 months ago		Current Poverty Status: Measured poor: "Corrected" poor	Measured poor: "Corrected" non-poor	Measured non-poor: "Corrected" poor	Measured non-poor: "Corrected" non-poor	Row Sum
Measured poor	"Corrected" poor	0.071	0.012	0.020	0.015	0.119
	"Corrected" non-poor	0.012	0.024	0.005	0.038	0.077
Measured non-poor	"Corrected" poor	0.019	0.005	0.029	0.026	0.078
	"Corrected" non-poor	0.014	0.041	0.028	0.643	0.726
	Column Sum:	0.116	0.081	0.081	0.722	1.000

Note: Poverty is based on equivalent expenditure where the equivalence scale is household size raised to the power of 0.75. The poverty line is such that the poverty rate is 20% in all years. Measured poor are those whose measured equivalent expenditure in the current year falls below the poverty line. "Persistent" poor are those whose expenditure purged of transitory shocks (i.e. the persistent expenditure component) falls below the poverty line. "Corrected" poor are those whose expenditure purged of measurement error (i.e. corrected expenditure) falls below the poverty line.