The effects of disasters on income mobility: Bootstrap inference and measurement error simulations

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1. Introduction

Despite the crucial role played by crises in poor rural people's lives, the literature on income mobility has largely ignored the potential cumulative effect of disasters. The scarcity of detailed household studies on such spillover effects represents a particularly serious lack of knowledge of processes of economic mobility (Baulch and Hoddinott 2000). This paper contributes to this knowledge by drawing on several "natural experiments" to evaluate the impact of natural disasters on income mobility of rural households in Pakistan.

The gap in the literature is unfortunate because government interventions to mediate the impact of a disaster necessitate knowledge of its nature. Firstly, if there are effects from these short-time shocks also in the aftermath of the crises, then the duration of crisis relief programmes might have to be reconsidered. Secondly, if the disaster causes long-term reduction in income, for example due to distress depletion of capital, then government protection against such short-term adverse adaptation to the crisis may yield immense long-term benefits.

A main reason for the lack of attention to how crises impact on economic mobility is that this requires detailed panel surveys with data collected both before and after a crisis. Also, to distinguish between the impact of the disaster and other events, one would prefer data on a comparison group that was not affected by the crisis. These requirements pose a considerable obstacle to such impact studies, especially in areas that are likely to be most vulnerable to disasters since there are few panel surveys in poor countries (Fields 2001).[1]

[1] Poor people in the rural areas of developing countries are frequently subjected to large income shocks, and the consequences might be starvation, or worse (Lipton and Ravallion 1995). Since a large fraction of this population is dependent on agriculture as their main source of income, several studies suggest that the severe impact of natural disasters such as floods, droughts or storms is deepened by missing insurance markets for these types of covariant shocks (Besley 1995). However, the poor are often able to mitigate risk through risk management strategies like diversification of crops, fields and employment decisions. They also engage in risk coping through saving and informal risk-sharing arrangements and adjusting labor supply (Morduch 1995). However, for large covariant shocks such as natural disasters, neither of these strategies may be sufficient for avoiding starvation, and it is found that local informal insurance networks are put under considerable strain because all members will draw on the arrangement when the shock is covariant (Morduch 1999). Foster (1995) in Bangladesh makes this point, where he finds that the body size of children suffers after a flood due to the parents' inability to borrow or get insurance. For a survey of the literature on risk and consumption in developing countries, see Alderman and Paxon (1992).

Our data from rural Pakistan are particularly well suited to analyzing the effects of large covariant shocks. The panel covers almost 800 households that were surveyed in 14 rounds over a period of five years, and during the panel period three different natural disasters occurred in three different districts at three different points in time. To the extent that these disasters hit the rural population randomly, the data provides us with "natural experiments". Hence, by comparing the income mobility of the households that were subjected to crises with the mobility of the others, we may be able to evaluate the impact of the disaster on income mobility. However, both measurement error and statistical inference are issues that are important for what conclusions we can draw from this type of studies. We address both issues here before we turn to the conclusions.

The first issue is that transition matrices, a powerful tool often used to evaluate economic mobility and an instrument employed in this study, is only descriptive if not accompanied by the variance of the estimates of the transition probabilities. Hence, it is impossible to evaluate whether differences between transition probabilities are statistical significant or not. However, this issue is seldom addressed in the literature. In general, most poverty statistics are usually computed from one sample of the population, which implies that having a large number of samples would provide us with the standard error of point estimates of poverty measures. However, it is a well-known problem that the analytic estimate of standard errors of estimators can be very difficult or impossible to calculate. This complexity seems to be the main reason why several mobility studies do not carry out statistical inference when making use of transition matrices (see for example Bane and Ellwood 1986, Hentschel and Lanjouw 1996, Dercon and Krishnan 2000, Scott 2000, Birchenall 2001, Parker and Gardner 2002).

The difficulties in calculating the standard errors have spurred the application of the bootstrap in recent econometric studies because it provides a tractable method of estimating the sampling distribution of a statistic (see Mills and Zandvakili 1997, Osberg and Xu, 2000). By generating random samples with replacement from the original sample, it is possible to simulate the original sampling procedure and hence statistical inference can be based on the bootstrapped distribution of the estimator. A comprehensive treatment of the validity of the bootstrap for a variety of different aggregated inequality, poverty and mobility indices is provided in Biewen (2002).

Much information is lost in aggregated indices, so transition matrices may be preferred for studying a range of empirical issues. Our objective is to provide a straightforward application of the bootstrap to construct confidence intervals for transition probabilities based on absolute income. Due to the complexity of the analytical derivation of the standard errors of the transition probabilities, this method facilitates statistical inference based on absolute mobility matrices.

The second issue important for our ability to conclude from studying transition probabilities is that they may depend on the accuracy of the measurement of income. Since there are many sources of errors in micro household data on income, one should always be concerned with the quality of data when studying economic mobility. One important contribution towards assessing the impact of poor data quality is Cowell and Victoria-Feser (2002), who investigate how data contamination influences welfare rankings.[2] On the other main problem of data quality, that variables are measured with error, it is widely recognized that this may cause bias in several poverty and mobility measures. This is particularly the case for income data from developing country household surveys where agriculture accounts for a large share of income. Despite its importance, little has been done to investigate how this error influences absolute mobility matrices. This is unfortunate because transition matrices are a powerful tool for making rigorous statistical inferences (Schluter 1997). An objective of this paper, therefore, is to simulate the standard model of measurement error to evaluate the potential influence on absolute transition matrices.

Our results indicate that evaluating the crisis by the immediate effects on income mobility substantially underestimates the impact of natural disasters such as droughts, hailstorms and flooding compared to including the effect in the subsequent year. We find that the spillover effect is large for the poor. Investigating the recovery phase, i.e. the households moving out of a crisis and into a normal year, we find that the probability of remaining poor

[2] Data contamination occurs if a proportion of false observations is added to the true data set, for example by miscoding or other types of mistakes.

increases by approximately 15% compared to households that were not hit by these disasters. During the "crisis phase", i.e. for those that move from a normal year and into a crisis, we find that the probability of remaining poor increases by 40% compared to normal income mobility. Taken together, these results indicate that poor households experiencing a crisis also have their incomes substantially depressed in the subsequent year. Households in the upper income classes seem not to be much affected by the presence of disasters. We find only a small negative lagged impact of the disaster on the income mobility of these households.

Our simulations of measurement error show that this usually increases the income mobility in the transition matrices in our data, as expected.[3] More surprisingly, this turns out not to be a general rule since the error seems to reduce mobility for some middle income groups. This implies that policy recommendations based on these types of analysis warrants a thorough investigation of the nature and impact of measurement error. However, for comparison of the poorest (and the richest) groups across states, as in our investigation, measurement error seems to have little impact. Moreover, our simulations find that the error induces a downward bias in the probability estimates of remaining poor (rich) that is of similar size between the group that was hit by a crisis and the group that was not. Hence, the estimated difference in the probability of remaining poor (rich) across states seems to be a close approximation to the true difference for plausible values of measurement error. Irrespective of the size of the errors, however, we find that the estimated differences in the probabilities represent a lower bound of the true difference. Our simulations also support the finding that the probability of remaining poor is underestimated by measurement error (see for example McGarry 1995).

We describe the data in more detail in the next section, and then in section 3 we explain the methodology that lies behind our results. Our analysis of how shocks influence income mobility is contained in section 4, and some final comments and tentative conclusions are drawn in section 5.

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[3] A mean zero independently distributed error would cause attenuation bias in a regression of income on past income. Since a low coefficient implies high mobility, because past income then explains little of present income, this error increases mobility. This is explicitly illustrated in Ashenfelter et al. (1986) for mobility analysis, but is implicitly treated in most text books of econometrics (see for example Greene 1997, pp. 436-437).

2. Data

The Pakistani panel data set used in this study was collected in 14 survey rounds from 1986 to 1991 in four different districts, where around 800 households in 52 villages were tracked.[4] Three of the districts, Badin in Sind, Dir in North-West Frontier Province, and Attock in Punjab, were chosen purposively (so each district represents a stratum) as some of the poorest areas of rural Pakistan. The data is therefore not representative for Pakistan or rural Pakistan, but except for Faisalabad, which was selected as a more prosperous control district, it can be regarded as representative for the poor rural areas in Pakistan. Two markets within each district were chosen at random, making these market clusters the primary sampling units. For each of these markets, villages were divided into three categories[5] according to their proximity to the particular market, and villages were then randomly chosen from each category. Then the households were drawn from a complete list of all families in each village.

The survey was conducted by the International Food Policy Research Institute (IFPRI) and the data were collected on a wide range of topics important to gauging the well-being of poor people. Considerable weight was put on measuring the rural households' income from different sources, and also recorded a wide range of factors important to income change over time (Adams and He, 1995). An imputed value for income in kind was calculated, as well as for household consumption of crops and crop by-products and home-consumed livestock. It has been argued that expenditure is preferred over income as a more accurate measure of long-term economic well-being because of consumption smoothing. However, if the aim is to analyze the impact of disasters, and to compensate for the shock, then using consumption as the measure may blur the true impact due to informal insurance arrangements. Also, the procedure for collecting consumption data differed between the three first and the two last years. Hence, the expenditures were rendered incomparable between the two time periods of

[4] Some households where observed only once, while other observations did not contain all the required information for our purposes. Deleting these observations can be seen as random, and causes no bias in the remaining sample. Comparing the income data in our sample with the original data, we find no significant differences, see appendix 2 and 3. From the original data set, our sample consists of 685 households which are tracked each year.

[5] Villages were categorized by the following distances to the market: Those within 5 kilometres of the market, those within 5 to 10 kilometres and those within 10 to 20 kilometres.

different sampling methods, so we are unable to compare the income dynamics with the consumption dynamics.

Since many people in these areas live in severe poverty, small income changes can determine whether or not a family is subjected to starvation. Thus, we are particularly interested in the household's absolute income, as opposed to the relative rankings of households implicit in quintile analysis.[6] Most government poverty statistics also focus on absolute income (Zheng 2001), and it is a clear advantage to provide policy recommendations on indicators regarded as important by the decision-makers.

It is widely recognized that children need fewer calories than adults in order to function normally. However, there is no agreement on how this should be used to improve comparisons of households with different numbers of adults and children. One argument against adjusting for caloric requirements has been that they differ according to the activity level of the individual and to individual heterogeneity for given activity level, but also that other nutrients are important in determining equivalence scales. A more important argument has been that even though the child needs fewer calories than adults, the child needs more of the household expenditure for education, clothing and medicine. So if the food component of total expenditure is low, there is little reason for adjusting for caloric needs (Lanjouw 2002). In our sample, food expenditure for the 3 first years accounts for 70% of total expenditure.[7] Hence, on average, food is dominant in the household budgets. Then the poor use an even larger proportion of their resources to meet the required food intake, and in this situation it seems necessary to adjust for different caloric requirements between adults and children.

[6] Most studies of economic mobility ignore absolute income in transition matrices, and focus solely on relative income (quintiles). This is unfortunate because the relative categorisation will not capture general welfare trends, as for example increasing or decreasing living standards for the whole population over time. In addition, a relative poverty classification is quite arbitrary, and it is not clear why one should use a certain percentage point cut-off instead of another. Moreover, the percentage point chosen can influence the characteristics of those defined to be poor (Lanjouw, 2002).

[7] Reliable expenditure data where only collected for the first three years in our Pakistan panel data, so food's share of total expenditure is calculated using the data from 1986/87 to 1988/89.

We therefore grouped the households by their absolute per-adult-equivalent[8] reported yearly income, which hereafter will be denoted income. According to the alternative food energy intake method an individual needs Kcal 2,100 per day, which could have been achieved with around Rs 2000 in income with the prices that prevailed in our time frame (McCulloch and Baulch, 2000). Hence, we take an income of Rs 2000 to serve as the poverty line in this study.[9] We classify those who have an income below Rs 2,000 as income class 1, and will frequently denote this group as "the poor". Taking this class as a starting point and looking at the income distribution, it seems natural to divide the rest of the sample into the following groups: those who have an income between Rs 2000 - Rs3000, Rs 3000 – Rs 4000, Rs 4000 – Rs 5000 and those who earned more than Rs 5000, respectively. The distribution of households

Table 1. Yearly distribution of households in each income category, by the absolute number of households.

Year / Income Category	1	2	3	4	5
1	138 (20%)	145 (21%)	173 (25%)	188 (27%)	205 (30%)
2	162 (24%)	160 (23%)	162 (24%)	187 (27%)	153 (22%)
3	122 (18%)	108 (16%)	131 (19%)	116 (17%)	105 (15%)
4	83 (12%)	74 (11%)	77 (11%)	66 (10%)	68 (10%)
5	180 (26%)	198 (29%)	142 (21%)	128 (19%)	154 (22%)
Sum	685 (100%)	685 (100%)	685 (100%)	685 (100%)	685 (100%)

[8] The adult-equivalent income is found by using the WHO caloric equivalent scale in table 4, and we also adjust for gender. Hence, we assume that the costs of supporting a child decrease with the age of the child, but are linear in the number of children at each age. If one does not take account of the different costs, or if households face economies of size, then the standard method of dividing income according to household size would deem larger households to be poorer than what might be the case (Lanjouw and Ravallion 1995).

[9] Malik (1993) uses a similar absolute poverty line for rural Pakistan: Rs 1800 (in 1984/85 prices) per capita yearly expenditure, and also provides an overview of the work on Pakistan national household expenditure data. Alderman and Garcia (1993) uses the poorest quintile of a range of different per capita variables (expenditure, income calorie consumption, landless, food share and so on) to classify the poor in our IFPRI data, while Adams and He (1995) uses income per capita as the measuring rod for the same data.

As we can see from table 2 below, the mean income for the full sample seems to be fairly constant for the two first years, and then drops by approximately 11% from the second to the third year. For the three latter years, the income remains almost constant on the lower level. The picture of the poor is somewhat different. While the first year is the best in terms of mean income for the poor, the second year brings about a 12% reduction making this the worst year for this group. In the subsequent years, the poor household's mean income level does not fluctuate much.

Table 2 also reveals the unfavorable position of the poor; the mean income over the 5 years for those who live below the poverty line is close to a quarter of the mean income for the non-poor. Also, it seems as though the fluctuations of the income of the poor over the years are negatively correlated with the variation of the full sample.

Table 2. Yearly mean income for the full sample, for the non-poor and for the poor.

Year	**Mean income for the full sample (std. deviation)**	**Mean income for the non-poor (std. deviation)**	**Mean income for the poor (std. deviation)**
1	4364 (3989)	5101 (4145)	1443 (450)
2	4411 (3815)	5259 (3871)	1251 (534)
3	3963 (3678)	4853 (3860)	1330 (453)
4	3802 (3866)	4725 (4143)	1359 (924)
5	4025 (4725)	5182 (5221)	1317 (567)
1 to 5	4113 (4036)	5027 (4261)	1338 (625)

To see whether there are any differences for the poor and the rich during the crises compared to normal times, we categorized households in income group 1 and 5 according to whether they lived in a district hit by a disaster. In 1986/87, crops were damaged in

Faisalabad due to a hailstorm at harvest time, Badin experienced flooding in September 1988 and Attock was hit by drought in 1987/88 (Alderman and Garcia 1993, EIU 1988a,c,d, 1989). Irrigation and water supply are crucial factors in determining the impact of a drought, and most of Pakistan's agricultural production came from irrigated areas in 1987/88. So even if larger parts of Pakistan were hit by the drought that started in 1987 (EIU 1988a), Attock was particularly vulnerable because of the very low ratio of irrigated lands. It is noted by EIU (1988b) that "The unprecedented drought which affected Pakistan last year during the summer monsoon season and which continued through the winter rain period has made its impact felt upon the unirrigated areas, but in the irrigated areas water supply seems to have been sufficient to overcome the worst effects." In our sample, only 2 % of the land owned by the inhabitants of Attock was irrigated in 1987/88, while the ratio of irrigated land in Faisalabad, Badin and Dir was 100 %, 76 % and 27 %, respectively. Good weather conditions just before harvest time and a new record in cotton yields in Sind and Punjab in the third quarter of 1988 indicates that the drought was limited to 1987 and early 1988 (EIU 1988c,d). Hence, our income data collected in 1987/88 corresponds exactly to the period of the drought.

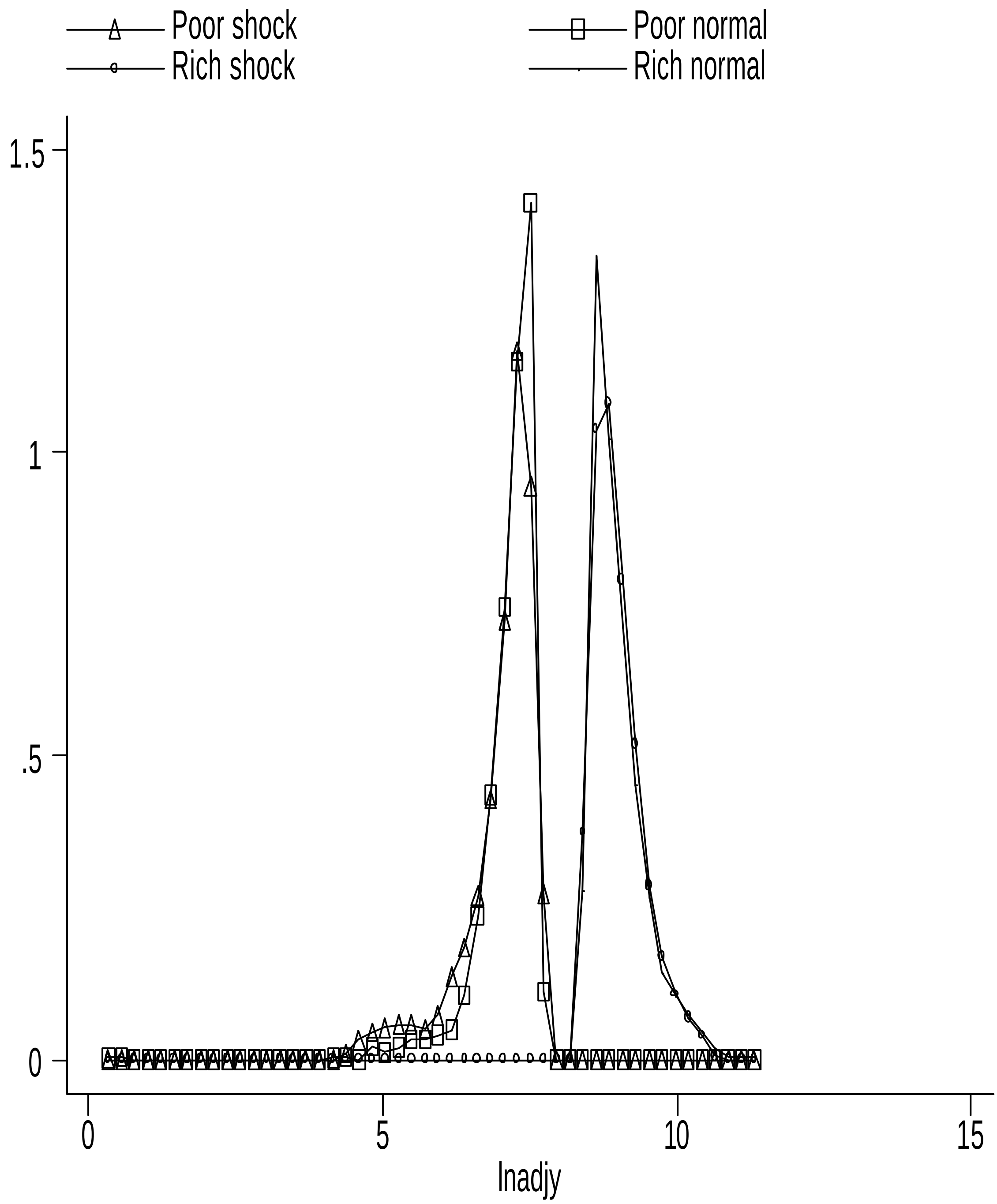
We can see from table 3 that the mean income for the rich is approximately the same for households subjected to the crisis compared to those that experienced normal times. On the other hand, the poor households subjected to a disaster had a 10% lower mean income than the other poor.

Table 3. Income statistics during normal times and during crisis: for the poor and for those households with an income above 5000 Rupees.

Income and state	Number of observation s	Mean income	Standard deviation	Minimum value	Maximum value
All Rich	802	9015	5808	5003	73222
Rich Normal	664	9043	6014	5003	73222
Rich Crises	138	8881	4709	5006	33108
All Poor	849	1338	626	-9481	1999
Poor Normal	720	1360	634	-9481	1999
Poor Crises	129	1216	561	-354	1987

From the kernel densities for the poor and the rich displayed in figure 1 below, it appears that the external shape of the income distribution for both groups was almost unaltered when a crisis occurred. We find no traceable effect of the crisis from the income distribution of the rich, while there is a somewhat larger proportion of the poor with lower incomes than in normal times. However, the actual role of the crisis for the poor is not easily traced by these static measures. Households that were better off may fall into income category 1 because of the disaster, and this effect may actually lead to an increase in the mean income for that group. Hence, we need to make use of the panel dimension of the data and evaluate intra-distributional mobility to explore the full effect of the crisis.

Figure 1.



Income densities for the rich and the poor in normal times and in a crisis.

3. Methodology

We are interested in evaluating how large covariant shocks affect the income mobility of the rural poor compared to the rich. Then transition matrices can be a powerful tool to get a picture of income movement over time across states. Let $S = \{1, 2, 3, 4, 5\} \times \{0, 1\}$ be the state space where a state $s \in S$ is defined by (y, k) where *y* is income class and *k* is an indicator of whether a crisis occurs, $k = 1$, or not, $k = 0$. The change in the crisis indicator over time can then be indexed by 0 and 1, so that the transition matrix between normal years can be denoted $M_{00} = [p_{ij}(t_{00})]$ where i denotes income class in year t and j the class in *t+1*. Then the matrices for movements into and out of crisis can be denoted $M_{01} = [p_{ij}(t_{00})]$ and $M_{10} = [p_{ij}(t_{00})]$, respectively. Each of the elements in M then represents the probability of movement between income classes from year *t* to *t+1*, conditional on *y* at *t* and change in *k*.

Assuming that the unknown probability of being hit by a disaster is q, then the true Markov transition matrix can be denoted $\Lambda_q = \begin{bmatrix} (1-q)M_{00} & q\, M_{01} \\ M_{10} & M_{11} \end{bmatrix}$ where M_{11} is the transition matrix for two consecutive disasters.[10] However, since we do not have any observations of households experiencing a crisis in two following years, we let $\Pr(k_t = 0 \mid k_{t-1} = 1) = 1$, which implies that $M_{11} = \boldsymbol{0}$. So if a disaster strikes at *t*, then *y* of the affected households follows M_{01} from *(t-1)* to *t*, and then *y* follows M_{10} with certainty from *t* to *(t+1)*. In case no disaster occurs, which has the probability of $(1-q)$, then *y* follows M_{00} between any years.

The true number of households with p_{ij} can be denoted $n_{ij}(t)$, and the total number of transition counts in each row i of *M* can be denoted $n_i(t) = \sum_{j=1}^{n} n_{ij}(t)$. The first order Markov transition probabilities can then be estimated by maximum likelihood, and this estimator of $p_{ij}(t)$ is given by

$$(1) \qquad \hat{p}_{ij}(t) = \frac{\hat{n}_{ij}(t)}{\hat{n}_i(t)}$$

[10] There is not enough information in our sample to estimate q.

where $\hat{n}_{ij}(t)$ and $\hat{n}_i(t)$ are the observed number of households moving from i to j and the observed number of households that started out in income class i, respectively.[11] That is, $\hat{p}_{ij}$ is the observed share of those which started in income class i that ends up in income class j. Then let $\hat{M}(t) = [\hat{p}_{ij}(t)]$ denote the estimated transition matrix.

Inference from a single transition matrix on a sample requires strong simplifying assumptions (Atkinson et al. 1992). First, one assumes that the same transition probabilities apply to all households (population homogeneity). We divide our sample into two main categories, those hit by a disaster and those who are not, and thus assume a different mobility pattern for each group. The second standard assumption is that transition probabilities are constant over time. In our framework, however, we not only allow the matrices to vary over time, but more importantly, we investigate whether the probabilities for those affected by a crisis differ over time. We calculate the transition probabilities of households moving into a disaster and compare them with households moving out of the same state.

The last assumption usually implicitly employed in work using these matrices is the first order Markov assumption: A transition probability is independent of past history. We also use this assumption, but see Schluter (1997) for some evidence that the income in year *(t-1)* also influences the transition probability from year t to year *(t+1)*. However, even if second-order Markov processes are better approximations to reality than first order processes in most circumstances, it is reasonable to believe that the first order impact of natural disasters like the ones present in our data will be much larger than eventual second order effects. Due to the usual constraints, the application of the second-order model is left for future research.

When constructing the income transition matrices, we separate the income movement of the households in the three districts that were hit by a natural disaster. Excluding these observations, we construct a matrix that represents the movement for the households when no crisis occurred, and let this represent the "normal" movement. In order to distinguish between the mobility experiences when households enter or exit a year of crises, one matrix is calculated for each event. The first matrix includes only observations where households move from a normal year and into a year of crises, the "entering crises matrix". The second, the

[11] See Schluter (1997) for the explicit derivations of the ML estimator.

"exiting crises matrix", is restricted to those observations where households move from a year of crisis and into a normal year.

Calculating $\hat{M}$ is straightforward, but evaluating the statistical significance of the transition probabilities can be more cumbersome because the analytical estimate of the standard deviation of $\hat{p}_{ij}(t)$ is very difficult to calculate. We therefore propose a simple bootstrap procedure to obtain estimates of these standard errors of each of the estimators of the transition probabilities.[12] In order to preserve the original panel's potential information about the impacts of crises on household mobility, each household must be treated as a cluster. So when we draw households randomly with replacement to create a new sample N^i with the same size as the original sample N, then each draw not only contains that particular household's 5-year income category record but also indicators of whether the movement between years is classified as normal, into-crisis or out of crisis. The new sample is then used to calculate the particular transition matrix $\hat{P}^i(t) = [\hat{p}_{ij}(t)]^i$, which we store.[13] Replicating this procedure as many times as practically feasible yields a set of estimates of each transition probability of $M(t)$. Thus, T bootstrap replications yields $\hat{p}_{ij}^1, \hat{p}_{ij}^2, ...\hat{p}_{ij}^i ..., \hat{p}_{ij}^T$ for $i, j \in [1, n]$, which represents an estimate of the true distribution of each of the transition probabilities. This estimate of the distribution of $p_{ij}(t)$ is then used to calculate confidence intervals.

Even if we are able to apply the bootstrap and construct an estimate of the distribution of the transition probabilities, the possibly severe problem of data imperfections still remains. It is very likely that our income data, as is frequently the case with household income data from developing countries, is measured with error (Alderman and Garcia, 1993).[14] This stems not only from the difficulty of measuring all the relevant variables that compile to household income, as for example to calculate the correct return from assets, or from recall bias, seasonality and long questionnaires. More important, especially for the societies we study where agriculture is a major income source, seems to be that personal and farm incomings and

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[12] The idea of using the sample data to generate an estimate of the true distribution stems from Efron (1979).

[13] The computation was performed in Stata, but the *xttrans* command does not allow storage of each element in $\hat{P}(t)$. The do-file modifying the *xttrans* command and the bootstrap programs are available upon request.

[14] Note also that as long as the income bins in the matrix are exogenously determined, transition matrices are robust against contamination (Cowell and Schluter 1998).

outgoings are often mixed (Deaton 1997). Since it is not necessary for a household to separate consumption expenditure from the outlays on farm inputs, errors in income data may arise from the difficulty in deducting the correct cost of production from the receipts. In addition, the net value of home-produced food, which also tends to be an important component of a poor rural household's income, is difficult to measure particularly when there are no well functioning markets for these items.

A few studies have attempted systematically to evaluate the impact of measurement error on mobility measures. Rendtel et al. (1998) categorize households according to two independent measures of the same income variable. Then if the two variables classify a household in two different categories, for example as poor by one and non-poor by the other, this is taken as evidence of measurement error. However, their assertion that identical categorizations by the two measures provide evidence for a true change between poverty states requires rather strong assumptions. It is straightforward to show that if the measurement error is correlated in the two measures of income, this could result in identical but false classifications. So even if one posits two independently measured income variables, this approach requires measurement errors in the two income variables to be relatively uncorrelated.

Bound et al. (1991) correct for measurement error by eliminating one-period spells of poverty, which is also the approach in Bane and Ellwood (1986) for changes in income less than one-half of the income to poverty-line ratio. Eliminating several one-year spells of poverty seems not to be plausible for studies of societies with a high true inter-year mobility because it would omit a large part of the actual changes in welfare. Moreover, it is not clear that one can attribute short spells of poverty to measurement errors even in societies where income is more stable.

A general framework for analyzing the impact of measurement error on income mobility is proposed in McGarry (1995), where a variance components model containing a white noise error term is estimated. This component is then treated as an approximation of the error in observed income. Because this method includes any true random income shocks in the measurement error term, correcting for the stochastic variance provides a lower bound of poverty rates. Hence, this approach is appropriate as long as the true random shocks are small compared to measurement error, which may be hard to verify.

Taking the static headcount index of poverty as a starting point, we know from Ravallion (1988) that a measurement error that increases variability in the welfare variable causes the expected value of the index to increase if the poverty line is less than the mode welfare and if the individual welfare function is quasi-concave. Looking at this index for a panel, one can classify the households that are defined as poor in every period as the "always poor". Now, even if measurement error increases the number of poor in every period, the empirical study in McGarry (1995) suggests that the same error causes the number of "always poor" to be biased downwards. She states that the reason is that the increased variability from the error causes several false transitions out of poverty, in at least one period, for those who in reality did not move upwards. However, the analytical derivation of this result seems to be impossible to calculate, and one might wonder if this error also works in the opposite direction, i.e. that some non-poor are classified as always poor due to measurement errors.

In order to assess the direction and magnitude of measurement errors on the transition matrices derived from our data, assume now that our measured income variable, y^*, represents the true structure of the data. Then we can simulate how a typical measurement error influences mobility matrices by calculating a new "observed" income, y, which has been influenced by a mean zero normal distributed random error term u_{it} that is uncorrelated over years and has a variance s_u^2:

(2) $y_{it} = y_{it}^* + u_{it}$, where $u_{it} \sim N(0, s_u^2)$

The error may either be multiplicative or additive, so we simulate each type for a wide range of plausible variances to see if any differences in the impact on transitions emerge. The impacts of the errors on the matrices are discussed next.

4. Impacts of disasters on income mobility using noisy data

Since we are particularly interested in the mobility of the poor in the aftermath of the disasters, we set up two hypotheses for the movement from a year of crisis into a normal year. The first is that the year after the disaster may be a recovery year, in which many households experiencing temporary bad fortune during the shock would move upwards when their incomes returned to a normal level. If life went back to normal after the disaster, we could expect the probability of remaining poor to be equal to or lower than the normal-to-normal transition probability. This is because we expect the number of poor to be larger during the crisis, and when things return to normal, ordinary income movement suggests that (1) will be lower than normal since the denominator is larger. Those affected by the disaster might also work harder than normal to compensate for the loss, or they might receive transfers or benefits from other formal or informal insurance mechanisms. In this case, the poor might get a higher income the year following the shock compared to normal times, which also implies a lower than normal probability of remaining poor. Equality between the two might be the result if the chances of improving their income were exactly as before the shock. Thus, the recovery hypothesis cannot be rejected if we find that the exiting matrix is not significantly different from the normal matrix or if the probability of escaping poverty is larger compared to a normal situation.

Our second hypothesis was that a disaster in one year might lead to depressed incomes also in the subsequent year. This might be the result if the disaster led to erosion of productive capital, either by depletion of capital for consumption purposes or by direct damage to assets.[15] In addition, floods may damage important infrastructure like roads, and the aftermath of natural disasters may bring pest infestation.[16] If productive assets were run down, we would expect the probability of rising out of poverty to be lower than normal in the aftermath of the crisis. Another reason for the spillover may be that in the year after the disaster, there

[15] Livestock died both during the drought in Attock in 1987/88 and the flood in Badin in 1988 (EIU 1988c). Floods are known to cause erosion of productive soil and damage trees.
[16] One example of pests following in the aftermath of natural disasters is the 1988 flood in Pakistan, where the following pest infestation damaging cotton production was believed to have been caused by of the flood (EIU 1988c).

will often be a reduced demand for individual providing services. This may reduce the incomes of the poor, since these groups get a substantial share of their incomes from such occupations.[17] From this reasoning, we would expect the probability of remaining poor to be somewhere between the probabilities in the entering matrix and the normal matrix: Worse than a normal transition, but better than being hit by a shock.

The transition matrices for the three states are displayed in table 4. The bootstrap, as described in section 3, was performed to construct estimates of the true distribution of each transition probability. The resulting estimates of the transition probabilities' standard errors are displayed below together with the estimate of the transition probability from the original sub-sample.[18]

[17] As Sen (1981) notes, one will frequently see a dramatic fall in the demand for barbers and tailors after drought. For the decomposition of non-farm income sources, at least for the three first years of our panel, see Adams (1994).

[18] Note that the estimates of the transition probabilities are from the original sample, and not from the bootstraps. This is because any bias in the estimates from the original sample will be exaggerated in estimates from the bootstrapped samples (StataCorp 2001).

Table 4. Transition matrices according to movement relative to the crisis.

Income class 1: $y \le Rs\ 2000$

2: $Rs\ 2000 < y \le Rs\ 3000$

3: $Rs\ 3000 < y \le Rs\ 4000$

4: $Rs\ 4000 < y \le Rs\ 5000$

5: $y > Rs\ 5000$

Normal matrix

		Income class in year (t+1)					
		1	2	3	4	5	Total
Income class in year t	1	**48 (2.5)**	27 (2.2)	11 (1.5)	6 (1.2)	7 (1.3)	100
	2	31 (2.0)	**32 (2.2)**	17 (1.7)	9 (1.3)	12 (1.5)	100
	3	14 (1.9)	28 (2.5)	**24 (2.4)**	15 (2.0)	20 (2.3)	100
	4	12 (2.4)	23 (3.0)	21 (2.8)	**14 (2.5)**	30 (3.3)	100
	5	5 (1.1)	10 (1.6)	15 (1.8)	16 (1.8)	**54 (2.8)**	100
	Total	24	25	17	11	23	100

Entering shocks matrix

		Income class in year (t+1)					
		1	2	3	4	5	Total
Income class in year t	1	**67** **(5.4)**	16 (4.2)	9 (3.4)	5 (2.6)	3 (1.8)	100
	2	35 (5.4)	**30** **(5.1)**	20 (4.6)	9 (3.2)	6 (2.7)	100
	3	18 (4.9)	34 (5.9)	**18** **(4.8)**	12 (4.1)	17 (4.7)	100
	4	11 (4.7)	22 (6.2)	20 (6.0)	**17** **(5.7)**	30 (6.9)	100
	5	6 (2.3)	11 (3.0)	17 (3.7)	11 (3.0)	**56** **(4.9)**	100
	Total	28	21	17	10	24	100

Exiting shocks matrix

		Income class in year (t+1)					
		1	2	3	4	5	Total
Income class in year t	1	**55** **(4.4)**	25 (3.8)	12 (2.8)	4 (1.7)	5 (1.9)	100
	2	38 (4.7)	**33** **(4.6)**	19 (3.8)	6 (2.3)	6 (2.3)	100
	3	20 (4.1)	32 (4.8)	**20** **(4.1)**	11 (3.1)	18 (4.0)	100
	4	27 (6.3)	35 (6.7)	22 (5.8)	**6** **(3.3)**	10 (4.2)	100
	5	9 (2.5)	10 (2.6)	20 (3.5)	12 (2.8)	**49** **(4.2)**	100
	Total	30	25	18	8	20	100

4.1 Implications for the poor

In order to compare the magnitude of any post-crisis effects with the actual disaster, we start by analyzing the income mobility for households that are in a normal situation in one year and experience a disaster in the next.

4.1.1 Direct effects of disasters

In a given normal year, almost half of the households below the poverty line (income class 1) remain poor in the proceeding year. However, this group has a much lower probability of rising out of poverty when the sample enters a disaster. More than two-thirds of those below the poverty line remain in that group when the negative shock occurs in the subsequent year. To evaluate the robustness of this result, we use the bootstrapped distribution of the probability estimates. Constructing 99% confidence intervals from this estimated distribution shows that the large discrepancy between the two estimates of transition probabilities for the poorest group moving from a normal to normal situation contra moving from a normal into a shock year is highly significant. Moreover, we find that even very large measurement error would not influence this conclusion, as is evident from table 5.[19]

[19] To find plausible values of the measurement errors, we compared the "true" income variance with the income variance after being influenced by m (see table 2). Whether the error is additive or multiplicative has a large impact on the contribution of the error to measured variance. For example, we find from the Monte Carlo experiments that the "true" income variance accounts for approximately 80%, 60% and 50% of the observed income variance for the poor in the first year when $m \sim N(0, 600)$, $m \sim N(0, 1000)$, $m \sim N(0, 1500)$. However, for incomes below 2000 Rupees, the multiplicative error does not change the variance of the true income for errors with a standard deviation of less than 20% of income. For larger errors, we find that $m \sim N(0, 0.3adjY)$ and $m \sim N(0, 0.4adjY)$ accounts for 6% and 20 %, respectively, of the observed variance of the income of the poor. For our purpose, however, the importance of the error lies in how it affects the transition matrices, not in its share of observed income variance.

Table 5. Monte Carlo simulations of the impact of a multiplicative measurement error on the probability of remaining poor in normal times and when entering a crisis, according to variability of the error.[20]

	Staying probabilities: $p_{11}(t,x)$		
	Entering crises	Normal	Difference entering-normal
Sample	0.67	0.48	0.19
m~ N(0, 0.05adjY)	0.65	0.48	0.17
m~ N(0, 0.10adjY)	0.64	0.48	0.16
m~ N(0, 0.15adjY)	0.63	0.48	0.15
m~ N(0, 0.20adjY)	0.61	0.47	0.14
m~ N(0, 0.25adjY)	0.59	0.47	0.12
m~ N(0, 0.40adjY)	0.54	0.45	0.10

Table 5 shows that the larger the multiplicative measurement error, the lower is the difference between the probability of remaining poor during normal times and the same probability when entering a shock. This indicates that the gap between these two sample estimates, which amounts to 19 percentage points, represents a lower bound of the discrepancy between the true transition probabilities in these two situations. This result is also sustained if the error is additive, see appendix 6, table A6.1.

A similar picture can be depicted for the poor households' chances of improving their position under the different circumstances. In a normal-to-normal transition we find that the poor have almost a 70% higher chance of moving one income class upwards, compared to the same movement in a normal-to-shock situation. Thus, there is a much higher probability of rising out of poverty when the households are not affected by shocks, which is not a surprising result. This is also a robust finding, indicated by the fact that this estimate of the normal-to-normal (normal-to-shock) transition probability is not contained in the 95% confidence interval of the normal-to-shock (normal-to-normal) distribution. In addition, this finding is also robust against measurement error in that the gap between the estimates represents a lower bound.[21]

[20] We use 1000 repetitions in measurement error simulations in this paper.
[21] See the impact of simulated measurement error on these estimates in appendix 6 table A6.2 for an additive error and appendix 7, table A7.1 for a multiplicative error.

That entering a shock severely worsens the situation in terms of income mobility for the poor is also indicated by the probability estimates of households in income class 2 that move one class down. If the subsequent year is normal, these households have a 31% chance of moving down, while the probability rises to 35% if they enter a year of shock instead. However, the difference between the two is statistically rather weak. Even if the normal-to-shock probability is not present in a 95% confidence interval of the corresponding normal-to-normal distribution, we cannot reject the possibility that this latter estimate is not contained in confidence intervals of the former, even if we narrow the interval considerably. The simulations of the measurement errors support the hypothesis that households in income class 2 did not have a different probability of moving one class down when entering a shock. The probability in normal times for this movement is very robust against small and medium errors, while the corresponding probability for those who enter a crisis increased relatively much for small errors.[22] Hence, measurement error may account for the observed differences in the respective probabilities.

4.1.2 Recovery in the aftermath of the disaster?

Now compare the mobility of the poor in the exit matrix with the normal matrix in figure 1. We find a seven percentage point higher probability of remaining poor when moving from a year of crisis to a normal year compared to transitions between normal years [23]. This amounts to a 15 % higher probability of remaining poor, which must be said to be considerable because the crisis in fact ended the previous year. This supports the hypothesis that the negative impact on the poorest spills over to the year after the shock. We also find that this result is very robust against likely measurement error.[24]

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[22] See appendix 6, table A6.3 and appendix 7, table A7.2.

[23] The existence of a spillover from the shocks is supported by our estimates (bootstraps) of the distribution of the probabilities. This is suggested because the estimate of the movement of the poorest group from a year of shock and into a normal year (55%) is not contained in the 99% confidence interval of the "normal to normal" year distribution of the same income group and movement. Similarly, but not as significant, the estimate of the probability of going from a normal to normal year (48%) is not contained in an 87% confidence interval for the shock to normal year distribution.

[24] The same result is found if the error is additive, see appendix 6, table A6.1.

Table 6: Monte Carlo simulations of the impact of a multiplicative measurement error on the probability of remaining poor in normal times and when exiting a crisis, according to variability of the error.

	Staying probabilities: $p_{11}(t,x)$		
	Normal	Exiting crisis	Difference Exiting-normal
Sample	0.48	0.55	0.07
m~ N(0, 0.05adjY)	0.48	0.54	0.06
m~ N(0, 0.10adjY)	0.48	0.54	0.06
m~ N(0, 0.15adjY)	0.48	0.54	0.06
m~ N(0, 0.20adjY)	0.47	0.54	0.07
m~ N(0, 0.25adjY)	0.47	0.54	0.07
m~ N(0, 0.40adjY)	0.45	0.51	0.06

We also find a similar result for those that start out in income class 2 and fall into poverty the subsequent year. There is more than a 20% higher probability for households in group 2 of falling into poverty if a shock occurred in the previous year compared to movement between normal years. However, neither the probability of remaining in income class 2, nor the probability of moving out of poverty and into income class 2 differs significantly between the two matrices.

Taken together, these findings indicate that the situation does not revert back to the normal mobility when moving from a year of shock to a normal year. Rather, this provides support for there being relatively large negative spillovers over time for poor households subjected to the crisis. This may explain the observation that there is a larger share of poor households in the year after the shock (30%) compared to the normal year share (24%).

The next step is thus to investigate the magnitude of this spillover on mobility in comparison to the impact of the actual disaster. From the exiting shocks matrix and the entering shocks matrix, we find that there is a significantly lower probability of remaining in poverty for a household moving out (55 %) compared to moving into a shock (67 %)[25]. Note also that households starting out in income group 2 have approximately the same probability of being impoverished the year after, irrespective of whether they enter or exit a shock.[26]

[25] Neither of the estimates are included in the other's 95% confidence interval.

[26] For plausible confidence intervals, we cannot reject the hypothesis that the probability is the same in the entering matrix and the exiting matrix for households in income class 2 of moving one class down.

Thus, households that are close to the poverty line are just as inclined to fall below the line whether they are subjected to a shock or to the spillover effect.

When it comes to opportunities to move out of poverty, however, we see that the probability of moving from the poorest group and into income class 2 is close to 60 % higher when exiting a shock compared to entering one. So when moving from a year of shock, the situation for poor people climbing from group one to two is very similar to the normal situation. The same can be said for those that remain in income group two; this transition probability is not significantly different irrespective of which matrix we study.

Before we turn to the more privileged households, we summarize our most important findings. When a shock occurs in a particular year, there will be a significant negative spillover in the subsequent year that reduces the households' probability of moving out of poverty, and increases the probability of falling into poverty for those just above the poverty line. While the shock has a much more severe effect on the poor, by increasing the probability of remaining poor by 40% compared to normal, we find that the spillover effect is also substantial. A movement from a crisis to a normal year increases the probability of remaining poor by approximately 15% compared to mobility for households not directly hurt by a shock.

4.2 Implications for the privileged

4.2.1 Entering shocks

The households in the upper income levels seem to have experienced the disasters very differently from the poor in terms of the mobility pattern. There is no significant difference in the probability of remaining in the highest income class (5) between normal to normal years and normal to shock years. One reason may be that the income class 5 has no upper bound. Thus, it might well be that these households are hurt by the disaster, but that most of them have such a high income that the reduction does not bring them down the income ladder more than normal. However, if the households are evenly distributed over the income range, we would expect that being hit by a disaster would increase the transitions from the upper to the lower classes. Note also that these differences are very robust against measurement errors, see appendices 6 and 7, tables A6.4 and A7.3.

The probability of moving from the highest class to income class 4 is larger for a transition from a normal to normal year (16%) than one from a normal to a shock year (11%). This difference is significant on the basis of a 95% confidence interval for the normal to normal distribution, and a 87% interval for the normal to shock distribution. It is also remarkable that there is the same probability of going up from class 4 to 5 in the two cases, which we take as support for a hypothesis that richer households do not experience very different mobility patterns when a shock occurs compared to normal times. Likewise, the probability of staying in income class 4 in the two different situations seems to be the same. The probability of remaining in class 4 in normal to normal years (14%) is included in narrow confidence intervals of the normal to shock distribution, which indicates that the two situations may not be significantly different with respect to transition probabilities. The probability of staying in income group 4 when entering a shock (17%) is not included in confidence intervals wider than the 85%, which indicates that there is some chance that the probabilities are different. Taken together, however, we reject the hypothesis that the two situations are different in terms of transition probabilities.

Looking at possible lagged effects, it seems as though going from a disaster into a normal year implies less opportunities for income generation for higher income groups than both the two other types of transitions. To see this, note that those in income group 5 had a probability of 49 % of remaining in this group when moving out of the disaster and into a normal year, compared to 54 % in a normal transition. This aspect is also reflected in the proportion of households in the two upper categories being larger in normal situations (34%) compared to the movement from a disaster (27%), and that the probability of falling from category 4 and 5 to 1 is twice as large in the latter situation (compared to normal to normal transition). Note also that the probabilities for households starting in group 4 (5) in a normal year of being in group 4 or 5 in a year of shock is 47% (67%), which is close to the normal to normal year transition probability. Summing up for the upper income groups, then, it seems that the only impact of natural disasters on income mobility is a small negative lagged effect.

5. Conclusion

The aim of this paper is to use some “natural experiments” to investigate the impact of natural disasters on the income mobility of rural households in Pakistan, and to propose some methodological procedures for evaluating the robustness of transition matrices. In addition to the expected result that the poor have a higher probability of remaining poor when entering a crisis compared to normal times, a main result is that there seems to be a substantial negative effect in the following year. Moreover, the poor have a 15% higher probability of remaining poor in the so called “recovery phase” after a disaster, compared to normal mobility. The more privileged households, to the contrary, seem not to be much affected by the crisis. We find no other impact than a slightly higher probability of moving out of the most favorable income group the year after the crisis (compared to normal movement). These results suggest that crisis relief for the poor should not only be provided during a natural disaster, but should also be maintained through the subsequent year in order to avoid increased impoverishment.

We propose a simple bootstrap method to facilitate statistical inference based on mobility matrices, and construct confidence intervals for the probability estimates. We illustrate the potential magnitude of measurement error on transition matrices and find that relatively small errors may induce a substantial downward bias of the probability of remaining poor. More encouragingly, however, simulating different types of measurement error gives a reasonable foundation for evaluating the impact of measurement error, in particular for comparison of the probabilities of remaining poor.

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Appendix 1: Adult equivalents

The scale is based on calorie requirements from the WHO, which is the same scale McCulloch and Baulch (2000) use for this IFPRI data set .

Age	Male weight	Female weight
0-1	0.33	0.33
1-2	0.46	0.46
2-3	0.54	0.54
3-5	0.62	0.62
5-7	0.74	0.70
7-10	0.84	0.72
10-12	0.88	0.78
12-14	0.96	0.84
14-16	1.06	0.86
16-18	1.14	0.86
18-30	1.04	0.80
30-60	1.00	0.82
60+	0.84	0.74

Appendix 2: Comparing the original data with our sample: Yearly household income for all households

	Original data			Our sample		
	N	Mean	Std. dev.	N	Mean	Std. dev.
Year 1	734	29368	34223	685	28911	33072
Year 2	734	34430	33928	685	34091	34047
Year 3	734 733*	34877 33577*	48447 33286*	685	33463	33162
Year 4	752	37246	50138	685	36230	46916
Year 5	730 729*	47019 43825*	99485 49538*	685	42969	47999

* Deleting one outlier

Appendix 3: Comparing the original data with our sample: Yearly household income for the poorest and the richest households.

To investigate whether our sample is skewed in the tails compared to the original data set, we divided the households in two categories reflecting the rich and the poor. Here, the poor households are those who have a household income of 10000 Rupees or less, while the rich are defined by an income level above 60000 Rupees. Note that these figures are not deflated. As we can see, there are no significant differences between the statistics.

		Original data A) Y<10000 B) Y>60000			Our sample A) Y<10000 B) Y>60000		
		N	Mean	Std. dev.	N	Mean	Std. dev.
Year 1	A	121	6900	2396	110	6771	2412
	B	66	101085	75522	58	100150	74898
Year 2	A	99	5994	2995	93	5958	3050
	B	106	100102	42911	95	101365	44366
Year 3	A	98	6805	2686	89	6780	2711
	B	96 95*	110088 100848*	101378 45872*	85	101515	47766
Year 4	A	73	5157	11218	65	5353	11685
	B	96	127734	95353	83	123562	90588
Year 5	A	61	5753	4354	56	5920	4418
	B	144 143*	133410 117732*	200739 70255*	130	116396	68474

* Deleting one outlier

Appendix 4: Quintile matrices.

Entering shocks quintile matrix

		Income quintile in year (t+1)					
		1	2	3	4	5	Total
Income quintile in year t	1	**62**	14	15	8	1	100
	2	35	**20**	26	15	5	100
	3	19	24	**19**	27	11	100
	4	9	18	20	**29**	24	100
	5	6	5	10	17	**61**	100
	Total	26	16	18	19	21	100

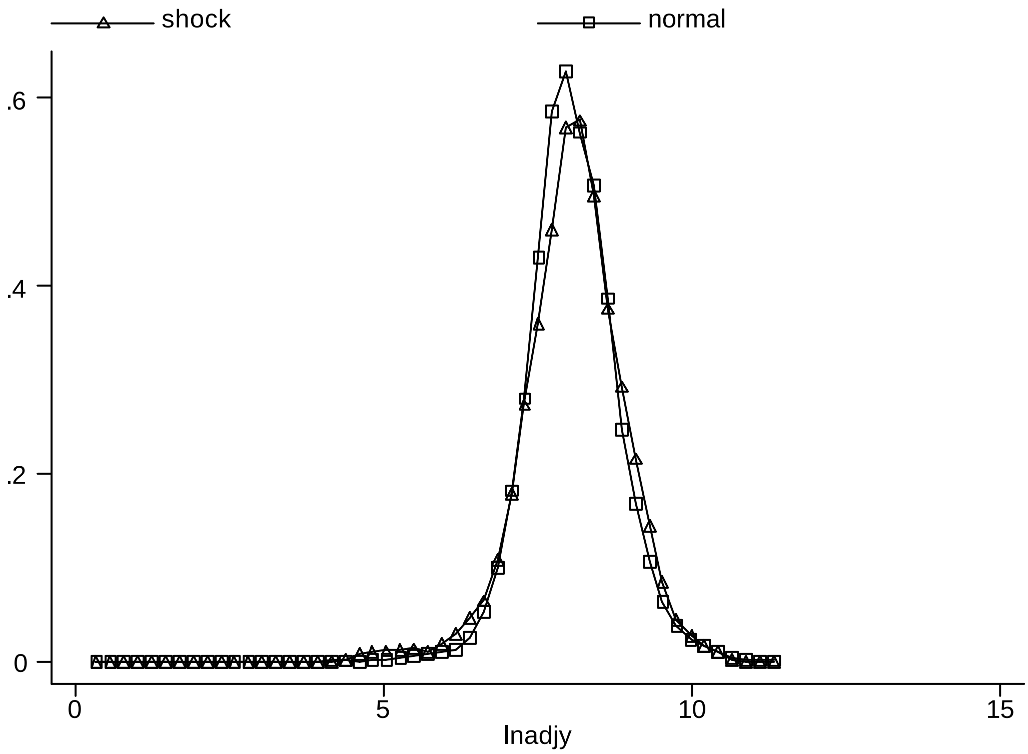
Normal quintile matrix

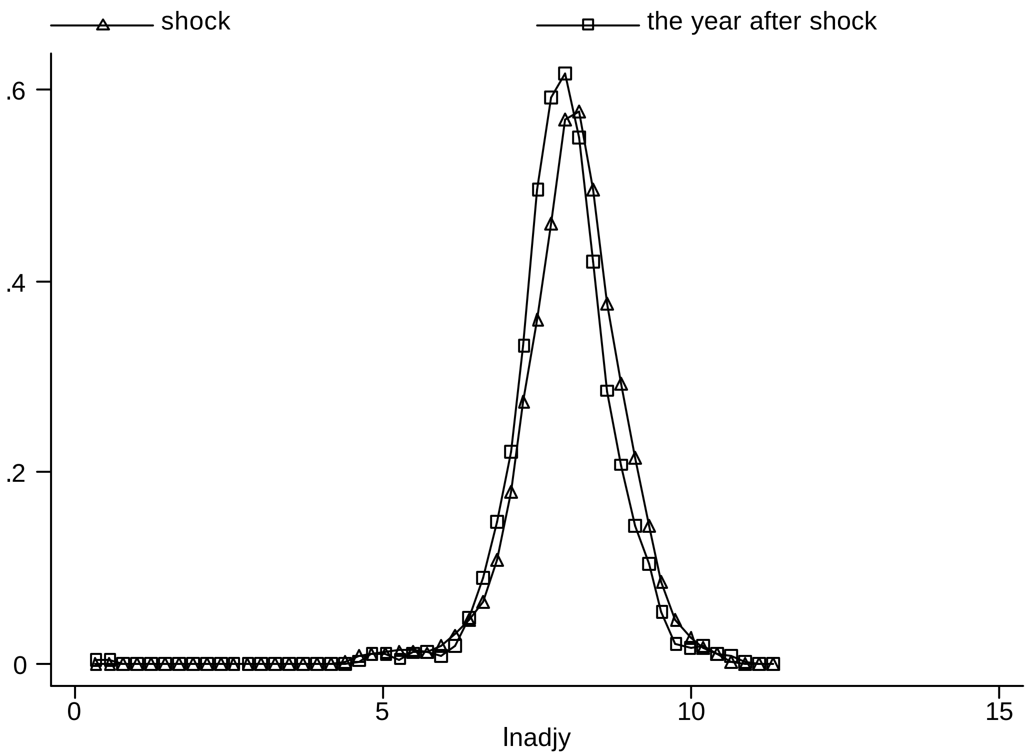
		Income quintile in year (t+1)					
		1	2	3	4	5	Total
Income quintile in year t	1	**41**	24	18	11	7	100
	2	23	**30**	24	16	7	100
	3	14	23	**26**	23	13	100
	4	8	15	23	**30**	24	100
	5	4	7	12	23	**54**	100
	Total	18	20	21	21	21	100

Exit shocks
quintile matrix

		Income quintile in year (t+1)					
		1	2	3	4	5	Total
Income quintile in year t	1	**48**	25	16	7	5	100
	2	29	**29**	23	14	6	100
	3	19	27	**24**	22	9	100
	4	20	21	20	**27**	12	100
	5	3	10	15	23	**48**	100
	Total	24	22	19	18	17	100

Appendix 5: Kernel density functions of log income for those households that were subjected to a covariant shock and for those that were not hurt.





Appendix 6: **Monte Carlo simulations of the impact on transition probabilities in the normal matrix, crisis matrix and exiting matrix of an additive measurement error, according to variability of the error.**

Table A6.1. The probability of remaining poor.

	Staying probabilities: P11				
	Entering crises	Normal	Exiting crisis	Difference entering-normal	Difference Exiting-normal
Sample	0.67	0.48	0.55	0.19	0.07
M N(0,200)	0.64	0.47	0.54	0.17	0.07
M N(0,400)	0.60	0.46	0.53	0.14	0.07
M N(0.600)	0.56	0.44	0.51	0.12	0.07
M N(0,800	0.53	0.42	0.49	0.11	0.07
M N(0,1000)	0.50	0.41	0.47	0.09	0.06
M N(0.1500)	0.46	0.39	0.45	0.07	0.06

Table A6.2. A poor household's probability of moving one class up.

	P12				
	Entering crises	Normal	Exiting crisis	Difference entering-normal	Difference Exiting-normal
Sample	0.16	0.27	0.25	-0.11	-0.02
M N(0, 200)	0.18	0.27	0.25	-0.09	-0.02
M N(0, 400)	0.20	0.27	0.25	-0.07	-0.02
M N(0, 600)	0.21	0.26	0.26	-0.05	0.00
M N(0, 800)	0.22	0.25	0.25	-0.03	0.00
M N(0, 1000)	0.21	0.23	0.24	-0.02	0.01
M N(0, 1500)	0.19	0.19	0.19	-0.00	0.00

Table A6.3. The probability for households in income class 2 of becoming poor.

	P21				
	Entering crises	Normal	Exiting crisis	Difference entering-normal	Difference Exiting-normal
Sample	0.35	0.31	0.38	0.04	0.07
M N(0, 200)	0.37	0.31	0.37	0.06	0.06
M N(0, 400)	0.37	0.31	0.37	0.06	0.06
M N(0, 600)	0.38	0.32	0.38	0.06	0.06
M N(0, 800)	0.38	0.32	0.38	0.06	0.06
M N(0, 1000)	0.38	0.33	0.39	0.05	0.06
M N(0, 1500)	0.38	0.34	0.39	0.04	0.05

Table A6.4. The probability for households in income class 5 of remaining in class 5.

	Staying probabilities: P55				
	Entering crises	Normal	Exiting crisis	Difference entering- normal	Difference Exiting-normal
Sample	0.56	0.54	0.49	0.02	-0.06
M N(0,200)	0.55	0.54	0.47	0.01	-0.07
M N(0,400)	0.54	0.54	0.46	0.00	-0.08
M N(0.600)	0.54	0.53	0.45	0.01	-0.08
M N(0,800	0.54	0.53	0.44	0.01	-0.09
M N(0,1000)	0.53	0.52	0.43	0.01	-0.09
M N(0.1500)	0.51	0.49	0.43	0.02	-0.06

Appendix 7: Monte Carlo simulations of the impact on the transition probabilities in the normal matrix, crisis matrix and exiting matrix of a multiplicative measurement error, according to variability of the error.

TableA7.1. A poor household's probability of moving one class up.

	P12				
	Entering crises	Normal	Exiting crisis	Difference entering-normal	Difference Exiting-normal
Sample	0.16	0.27	0.25	-0.11	-0.02
M N(0, 0.05adjY)	0.17	0.27	0.25	-0.10	-0.02
M N(0, 0.10adjY)	0.17	0.27	0.25	-0.10	-0.02
M N(0, 0.15adjY)	0.18	0.26	0.25	-0.08	-0.01
M N(0, 0.20adjY)	0.19	0.25	0.24	-0.06	-0.01
M N(0, 0.25adjY)	0.19	0.25	0.23	-0.06	-0.02
M N(0, 0.40adjY)	0.19	0.22	0.21	-0.03	-0.01

Table A7.2. The probability of a household in income class 2 of becoming poor in the next period.

	Staying probabilities: P21				
	Entering crises	Normal	Exiting crisis	Difference entering-normal	Difference Exiting-normal
Sample	0.35	0.31	0.38	0.04	0.07
M N(0, 0.05adjY)	0.38	0.31	0.37	0.07	0.06
M N(0, 0.10adjY)	0.37	0.31	0.37	0.06	0.06
M N(0, 0.15adjY)	0.37	0.31	0.37	0.06	0.06
M N(0, 0.20adjY)	0.37	0.31	0.38	0.06	0.07
M N(0, 0.25adjY)	0.37	0.32	0.38	0.05	0.06
M N(0, 0.40adjY)	0.40	0.36	0.41	0.04	0.05

Table A7.3. The probability of remaining in income class 5.

	Staying probabilities: P55				
	Entering crises	Normal	Exiting crisis	Difference entering-normal	Difference Exiting-normal
Sample	0.56	0.54	0.49	0.02	-0.05
M N(0, 0.05adjY)	0.55	0.54	0.47	0.01	-0.07
M N(0, 0.10adjY)	0.54	0.54	0.45	0.00	-0.09
M N(0, 0.15adjY)	0.53	0.53	0.44	0.00	-0.09
M N(0, 0.20adjY)	0.52	0.51	0.43	0.01	-0.08
M N(0, 0.25adjY)	0.51	0.50	0.42	0.01	-0.08
M N(0, 0.40adjY)	0.47	0.46	0.39	0.01	-0.07

Summary

We evaluate the impact of disasters on income mobility by drawing on "natural experiments". While the poor have a much higher probability of remaining poor when entering a crisis compared to normal times, there is also a negative effect in the year after. Richer households seem to be unaffected. A simple bootstrap method is proposed to facilitate statistical inference for mobility matrices. Also, we simulate measurement error to illustrate its magnitude on these matrices. Small errors induce a substantial downward bias of the probability of remaining poor, while comparisons across states seem more robust, which is promising for impact analysis.

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