Casting the Net Wide *and* Deep: Lessons Learned in a Mixed-Methods Study of Poverty Dynamics in Rural Bangladesh*

Peter Davis
Social Development Research Initiative, UK
Email: p.r.davis@sdri.org.uk

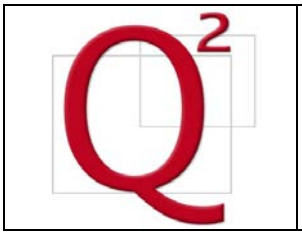
Bob Baulch
Tan Tao University, Vietnam
Email: bobbaulch@gmail.com

Abstract

In this paper we reflect on lessons learned in developing a mixed-methods approach to the study of poverty dynamics in a three phase qual-quant-qual study of poverty dynamics in rural Bangladesh. We argue that a sequential but integrated approach has a number of advantages over single-method approaches or non-integrated studies. In particular, mixed-methods research strengthens our ability to make more reliable causal inferences, both in individual life trajectories, and in collective trends. We also examine how integrating qualitative and quantitative methods raises important issues for poverty dynamics research, including the way that concepts are developed and deployed, how field research is designed and conducted, how causation is identified, and how findings are analysed and presented.

Keywords: poverty dynamics, chronic poverty, mixed methods, Bangladesh

*The authors thank the Chronic Poverty Research Centre (CPRC), HarvestPlus, the International Food Policy Research Institute (IFPRI), and the University of Waikato, for funding data collection. They also thank Zahidul Hassan and Md. Zobair of Data Analysis and Technical Assistance Ltd (DATA) for managing the fieldwork so efficiently, and Wahid Quabili, Biswas Akhter, Rafiqul Haque (Shawpon), Dilara Hasin, Safia Satter (Sonia), Md. Abdul Aziz and Anowara Begum (Nupur) for excellent research assistance. Agnes Quisumbing, Paul Shaffer and Andrew Shepherd provided useful comments on earlier versions of this paper.



Q-Squared Working Paper No. 58
Summer 2012

1. Introduction

In recent years, the use of mixed methods has become well established in poverty research in both developing and industrialised countries.[1] However, mixed methods have been used rarely to study poverty dynamics—that is the factors and processes associated with movement into and out of poverty (Baulch and Hoddinott, 2000). Furthermore, even though mixed-methods studies of static poverty are now relatively common, a frequent criticism of such studies is that the quantitative and qualitative data on which they are based are poorly integrated, often on an *ad hoc* basis after data collection has taken place (Kanbur, 2000).

In this paper, we reflect on the lessons learned from a unique mixed-methods study of poverty dynamics in rural Bangladesh. This study, which deliberately adopted an integrated and sequenced 'qual-quant-qual' approach, is one of few mixed-methods studies of poverty dynamics in either developing or industrialised countries.[2] We argue that mixed-methods work is well positioned to foster a critical, rather than a mechanical, approach to poverty dynamics studies, and thus contribute to both conceptual and substantive research outcomes. In particular, we argue that mixing methods enhances our understanding of poverty dynamics by revealing a number of hidden causes of socio-economic mobility (in rural Bangladesh, these were dowries, life-cycle issues, insecurity, and power-resource relations) and by strengthening our ability to make causal inferences.

In the next section, we reflect on lessons learned in formulating research aims, conceptualising poverty dynamics, and choosing key indicators. In Section 3, we describe our qual-quant-qual research design and field methods before moving on, in Section 4, to discuss lessons learned from nesting a medium-N qualitative sample with a large-N quantitative study. This leads to Section 5, in which we reflect on how an integrated but sequenced approach strengthened our ability to make causal inferences. In Section 6 we discuss how our the integrated approach was reflected in the data analysis

[1] See: Carvalho and White (1997); Hulme and Toye (2007); Kanbur and Shaffer (2007) and the Q-Squared Working Paper Series (http://www.trentu.ca/ids/qsquared.php).

[2] The few other mixed methods studies of poverty dynamics in developing countries include Adato et al. (2005) for South Africa, Barrett (2004) for Ethiopia and Kenya, Lawson et al (2008) for Uganda. Most of these studies are unpublished and/or incomplete.

and the presentation of research findings. We conclude by drawing out a number of methodological and practical lessons for other researchers interested in studying poverty dynamics.

2. Lessons Learned in Formulating Research Aims and Choosing Concepts and Indicators

The overall study was concerned with identifying and investigating the most important causes of decline or improvement in people's lives – including declines into, or escapes from, poverty – over the medium to long term. Previous research had suggested that there had been significant declines in headcount poverty rates in Bangladesh in recent years, including in rural areas (see Sen and Hulme 2006; World Bank 2008). However there had been few studies investigating how this is experienced within the life trajectories of the rural poor, and what socio-economic processes helped explain why some people moved out of poverty while others stayed poor, or became poorer. Because our study was necessarily longitudinal and exploratory, we felt it ideally lent itself to a mixed-methods approach in exploring these issues.

Poverty, as a state that a person, a household, or wider group can experience was a key concept. In quantitative research, poverty tends to be seen in monetary (expenditure or income) terms. This is not because quantitative researchers do not recognise the importance of non-monetary aspects of wellbeing at the conceptual level, but because welfare measures like expenditure or income are more easily quantified than many other contributors to wellbeing, and therefore suit specialised forms of statistical analysis.[3] In developing countries, expenditure tends to be used in preference to income as the welfare measure as it is usually easier to measure, and less subject to variation than income (Deaton 1997). In most developing countries per-capita household expenditure is determined from household surveys, and poverty lines are typically determined from national data calculated on the basis of an inflation-adjusted cost of a bundle of goods that can fulfil basic needs. Most national statistics offices produce a set of official poverty lines based on the cost of acquiring a minimum level of calorie requirements (the food or 'lower' poverty line) plus a modest allowance for non-food

[3] In recent years there are attempts to develop multi-dimensional indicators of poverty (Alkire and Foster 2008) but attempts to operationalise these in empirical research are still rare.

expenditures (to give the total or 'upper' poverty line) (Ravallion, 2010). The lower and upper poverty lines are then adjusted for regional differences in price levels, which are updated regularly, and used to calculate and monitor national poverty on a consistent (if uni-dimensional) basis. For example, the Bangladesh Bureau of Statistics (BBS) produces lower and upper poverty lines by division and across urban, rural and statistical metropolitan areas (SMA). Table 1 shows the Bangladesh Bureau of Statistics' (BBS) upper poverty lines, which we used for our quantitative analysis of poverty dynamics. All our survey sites were in rural areas with initial surveys in 1994, 1996 and 2000, with the most recent qual-quant-qual mixed methods round in 2006-7.

Table 1: BBS upper poverty lines by Division
(Taka per person per month)

Division	1994	1996	2000	2006-7
Dhaka-Rural	547.4	618.1	650.7	877.4
Khulna-Rural		550.6		773.8
Rajshahi-Rural	501.0		597.6	798.7
Chittagong-Rural			733.1	928.7

Household expenditure data are useful for measuring broad poverty trends (such as whether poverty headcount ratios have changed) across a large population. However they are not so useful for identifying causes of mobility or accurately tracking individual households. Based on data from this study, Davis and Baulch (2011) illustrated how household expenditure can be supplemented by measures of other household characteristics (in particular assets), in order to avoid reaching erroneous conclusions about the socioeconomic mobility.

In qualitative research, poverty is usually defined in more multidimensional terms with characteristics less amenable to measurement included in the analysis – such as social status, exclusion, power, or political participation. Concerns for these aspects of welfare are reflected in recent prominent conceptual approaches to poverty, such as in the social exclusion, participation, and capabilities literature (see Stewart et al 2007) but methods for empirical investigation of these

dimensions of poverty are not well established. As a result qualitative research on multidimensional poverty is often exploratory with definitions of poverty seen as much a research output than a starting point.

In our qualitative work, poverty was linked to a multidimensional view of a person's wellbeing, within the temporal context of their life stories, and in the social context of their families and communities. In the life history interviews we used the Bengali word *obosta*, which roughly translates as 'life condition', to prompt discussions about a range of influences on a person's wellbeing in initial within-case analyses. The lack of imperative to quantify allowed a range of monetary and non-monetary dimensions of a person's wellbeing to be explored.[4] However, it meant that poverty status was judged by assessing a number of non-measured characteristics which could vary from researcher to researcher within the team. We minimised this variation by allocating overall poverty levels on a five point scale[5] (see Table 2). We also asked focus groups to place households into categories according to wellbeing levels at the current time and ten years earlier and to discuss why. This allowed us to cross-check life-history information. We then assigned wellbeing levels, drawing from these discussions and the life-history interviews, in a further group discussion among the research team, which included the qualitative analyst. This was done at the end of the life history research in each village while the life history interviews were fresh in our memories before we moved to the next site.

[4] See Stewart et al (2007) for an excellent discussion of a range of conceptual approaches in poverty research.
[5] We do consider this technique to be precise enough to warrant more points.

Table 2: Qualitative wellbeing levels for individuals[6]

Level	English	Bengali	Guideline
1	**Very poor or destitute**	*khub gorib, na keye chole*	Suffering tangible harm to health because of poverty, generally due to insufficient food. Tend to be landless or near landless.
2	**Poor**	*gorib*	Very vulnerable but eating reasonably well. Vulnerable to moving into level 1 if a commonly occurring shock is experienced. If land is owned, it usually less than an acre for a medium-sized household.
3	**Medium**	*madhom*	A commonly occurring shock would not result in tangible harm or going without food. Have household assets, or generate household income, equivalent to between one and two acres of land for a medium-sized household.
4	**Rich**	*dhoni*	Hold household assets or generate household income equivalent to that generated by two to ten acres for a medium- sized household.
5	**Very rich**	*khub dhoni*	Hold household assets or generate household income equivalent to that generated by ten acres or more for a medium-sized household.

While qualitative and quantitative researchers often hold similar views on the nature of poverty, the types of indicators used influences way each group thinks about poverty. Quantitative researchers tend to think about poverty in relation to poverty lines and in money-metric terms; qualitative researchers are drawn to exploring poverty in less measurable multidimensional and contextual terms.[7] The integrated approach encouraged both groups of researchers to consider tacitly held assumptions associated with their particular approach to poverty, particularly when disagreements arose over the assessments of poverty status of particular individuals or households.

An approach to ontological and epistemological differences which was pragmatic in nature allowed us to explore a complex problems like the assessment of socioeconomic mobility most effectively in order to generate usable policy-relevant knowledge. We found that the most useful debates occurred

[6] These levels appear on the trajectory diagrams in Appendix 2.
[7] See Stewart et al (2007) for an excellent discussion of different approaches to measuring poverty in development contexts.

when the research team was solving concrete and practical research problems, rather than engaging in detached hypothetical or abstract discussions.

3. Lessons learned in the Field: Research Design and Fieldwork Methods

In 2006, IFPRI, DATA and the Chronic Poverty Research Centre (CPRC) began a study to resurvey all the households surveyed in three previous evaluation studies of three development intervention types.[8] The original evaluations surveyed a total of 1907 households and 102 villages located in 14 of Bangladesh's 64 districts and we combined these households to create in effect a single panel so that we could draw from the wealth of historical data available for the households. The districts and villages span the range of agro-ecological conditions found in rural Bangladesh and, while the sample cannot be described as nationally representative, it does broadly characterize the variability of livelihoods found in rural Bangladesh (see Appendix 2 for a map showing the location of the survey villages). As the focus of this study was on understanding what causes people's lives to either improve or decline, children who had left original households and set-up their own households were tracked as long as they had not migrated outside their home district. The 2006-7 research had three integrated and sequenced phases:

3.1 Phase 1: Focus-group Discussions

Phase 1 was a qualitative phase using focus groups to examine perceptions of changes (and why these had come about) from women and men in a sub-sample of twenty-nine sites across a total of eleven districts in our survey communities. In each site, four separate groups were formed comprising poor women, poor men, non-poor women and non-poor men. The focus groups were organised and facilitated by field researchers from DATA Bangladesh cooperating with village leaders and locally knowledgeable people. Poor households were defined as those that had suffered food shortages due to poverty at some time in the previous year, and were selected during initial discussions with key

[8] The previous studies were commissioned evaluations conducted by the International Food Policy Research Institute and associates in Bangladesh to evaluate the short-term impacts of microfinance (1994), new agricultural technologies (1996-97 and 2000-01) and the introduction of educational transfers (2000 and 2003). These are described in Zeller et al. (2001), Hallman, Lewis and Begum (2007) and Ahmed (2005), respectively.

informants. The focus groups had from 5 to 10 participants. A total of 116 single-sex focus group discussions, evenly divided between 'treatment' and 'control' villages, were conducted in July and August 2006.

Groups were first asked to list the main causes of decline in people's lives (using the word Bengali word '*obosta'* which roughly translates as 'life condition'). Participants were then asked to choose the three most important of the causes of decline. A similar process was conducted for improvements in wellbeing, and for causes of remaining in poverty. This allowed an initial open brainstorming discussion to take place followed by a consensus-finding exercise where the three most important causes of decline, improvement or stagnation were chosen by the group.

These group discussions were carried out partly as a piloting exercise to inform the design of following phases, although they also provided information for the World Bank's Bangladesh poverty assessment which utilised the initial findings of the study (see World Bank 2008). The findings from these discussions helped to formulate new aspects of the 2006-2007 quantitative household survey, in particular the modules of the survey where respondents were asked to report on particular shocks or positive events they had experienced over the previous ten years, and to report on household links to officials, politicians and leaders.

The unusually large number of focus group discussions was due to the decision taken early on in the project to conduct four in each village with, respectively, poor women, poor men, better-off women and better-off men. In addition, there was interest in including villages from each of the three original evaluation studies. As a consequence many more discussions were conducted than would have been necessary if their only purpose was as a pilot study for informing subsequent phases.

3.2 Phase 2: Household Survey

Phase 2 was a quantitative survey of the original households and new households that had split from the original households but remained in the same district. This household survey was conducted by teams from DATA between November 2006 and February 2007, the same agricultural season as the original surveys, and covered 2,152 households (of which 1,907 were core households that took

part in the original survey, and 245 were 'splits' from the original households).[9] The household survey questionnaire was designed to be comparable across sites and with the original questionnaires from the evaluation studies.

At the start of this phase, 'pre-testing' of the household and community questionnaires was conducted in similar villages that were not part of the sample. Analysts (both qualitative and quantitative) and the field team worked together in field-testing modules of the household survey and then as a group discussed each aspect of the survey to check and change any parts that had caused problems.[10] Once this was done, the household and community survey questionnaires were revised and a 9 day training course held for the survey interviewers and supervisors. The first five days of this training involved introducing and discussing the 18 core and 4 additional modules of the household survey questionnaire. Interviewers then spent a day practicing with these questionnaires in non-survey villages followed by two-and-a-half days of feedback and modification of the questionnaires in the light of the practice day, together with final instructions from the survey managers. Additional training was provided to the survey supervisors concerning the administration of the community questionnaires.

Following the training, seven survey teams were formed with five members per team (including survey supervisors) in the educational transfer and microfinance sites and seven members per team in the agricultural technology sites. In total, there were 34 enumerators and seven field supervisors, all of whom had undergraduate level qualifications in the social sciences or statistics. The seven field supervisors had all worked on previous rounds of the survey, and had strong survey facilitation skills.

[9] A community-level questionnaire was also administered to key informants at this stage to obtain basic information on each village, and changes since the last survey round. GPS coordinates for all sample households and village facilities were also collected.

[10] As many of the modules had been used in previous waves of the evaluation surveys, it was not necessary to pilot all of the modules of the questionnaires, but only those which had been redesigned or were new (for example, the shocks, perceptions of poverty and wellbeing and the links with influential people module).

The survey itself was conducted by seven teams of experienced enumerators between November 2006 and February 2007.[11] Median interview times were two-and-a-half hours for households in the educational transfer and microfinance sites, and five-and-half hours for the households in the agricultural technology sites (in which additional 24 hour food recall, agricultural production and aquaculture modules were administered, and blood haemoglobin levels were measured using the hemocue finger prick technique). GPS coordinates for survey households and facilities (health clinics, markets, schools) in the villages were also collected using handheld GPS units. In the course of the two or three days spent in each village by the survey teams, the survey supervisor also interviewed key informants (such village leaders, school teachers, health clinic staff or NGO workers) to complete the different sections of a community questionnaire. All the household questionnaires were checked i*n situ* by the field supervisors, and where responses were found to be incomplete or improbable, the household was revisited. Second visits were necessary for approximately 19% (427) of households, and were most frequent (because of the length of the additional modules) in the agricultural technology sites.

The overall attrition rate across the three interventions was 6.3 per cent (120 of the 1,907 core households) or 0.8 percent per year across the three interventions, with attrition being lowest (0.4 percent per year) in the agricultural technology and highest (2.0 percent) in the educational transfer sites.[12] An investigation of the pattern of attrition, using probit regressions, in these panels suggests that it is mostly random (Quisumbing, 2007; Baulch and Quisumbing, 2010). Field researchers were able to track around three-quarters (365/485) of the household splits so the total number of households in the panel increased over time. The overall quantitative resurvey data is felt to be of high quality,

[11] All of the interviews in the Education Transfer sites were completed by December 2006, but the interviews in the agricultural technology and microfinance sites took longer because of the collection daily food recall data as well as blood haemoglobin.

[12] This level of attrition is comparable to the 6 per cent attrition rate for the first two rounds of the Indonesia Family Life Survey (Thomas, Frankenberg and Smith, 2002). It is significantly better than the 16 per cent attrition between the first and second rounds, and 38 per cent attrition between core households in the first and third rounds, of the Kwazulu-Natal Income Dynamics Study (KIDS) in South Africa (Agüero, Carter and May, 2007). See Alderman et al. (2001) for a systematic analysis of patterns of attrition in KIDS and two other developing country panels. Other panel studies in Bangladesh that have tracked household splits include the Bangladesh Nutrition Survey (Rosenzweig, 2003) the BIDS village panel (Rahman and Hossain, 1995; Sen, 2003) and the Matlab Health and Demographic Survey (Razzaque and Streatfield, 2002).

largely because of the considerable experience and expertise of the survey company (DATA Ltd). [13] Nevertheless there were a few things which, in retrospect, could have been improved upon. These include: consistent spelling of village, union and upazila names; recording the ages of children consistently in the household roster and anthropometrics modules; and collecting GPS coordinates for all facilities (banks, clinics, local government offices, schools etc) used by the sampled households. For analysing poverty dynamics, and economic mobility more generally, it would also have been extremely useful to have tracked members of core panel households who had moved outside their original districts—but this was not possible due to financial constraints.

3.3 Phase 3: life history research

Phase 3 consisted of a qualitative study based on the life histories of 293 individuals, in 161 selected households, in eight of the districts of the original quantitative study.[14] The selected households were a sub-sample of the larger quantitative sample. The eight districts were selected to represent the range of environments in rural Bangladesh and to include sites from all three of the evaluation studies.

In each study site we selected two villages located in different unions. [15] For each site (two villages), we then randomly selected 20 households with 5 households being selected from each cell of the village's poverty transition matrix which was constructed using the expenditure data from the initial and most recent household survey. [16] As the numbers of households surveyed differed between villages and intervention, sampling weights were calculated based on each household's probability of selection in the life-history subsample.

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[13] The Phase II data is publically available via http://www.ifpri.org/dataset/chronic-poverty-and-long-term-impact-study-bangladesh

[14] Of these eight districts, six were in districts where Phase 1 focus groups had been carried out.

[15] The union is the lowest administrative level in local government in Bangladesh. These 'sites' were located in the same district except for the Mymensingh/ Kishoreganj 'site' which included villages fairly near each other but spanning the district boundary.

[16] The four categories were move up, move down, chronic poor, and chronic not poor, across the time period between the baseline and 2006-7 with reference to poverty line levels calculated for the survey year in the relevant division.

We held an initial training workshop at the start of the life-history phase. The qualitative team consisted of two male and two female field researchers who were postgraduate social-science graduates and experienced interviewers. A check-list for the life-history interviews was created, and the team, accompanied by the analyst, then spent one day pre-testing interviews with respondents who were not part of the sample. After pre-testing a further debriefing session was held where interviews were discussed and further refinements to the approach were made.

Interviews were then conducted in the first two subsample villages and a further 'debriefing' workshop followed. We decided then that women would only interview women and men would only interview men and refined the interview check-list and our approach to assigning wellbeing levels to respondents based on focus-groups as well as on interviews.

For the life history interviews we interviewed, whenever possible, one adult man and one woman separately in each household. Conducting two life history interviews per household provided a number of advantages:

1) It allowed immediate cross-checking of memories of key events. From the differences between the two accounts we got an idea of the accuracy of information since some kinds of events were recalled more accurately by both respondents while other types of information were more reliably recalled by one of the interviewees. One of the key problems with retrospective interviews is the quality of recall, so this helped us to judge recall accuracy of details, particularly of dates and family events.

2) It gave a gendered perspective on events, episodes, and processes. We found differences in emphasis between what affects women's wellbeing and what affects men's wellbeing, together with respondent's perceptions of these. Women tended to remember more about children and revealed more about domestic tension and family relationships, while men gave more accurate information on land (areas, purchases and sales), some forms of household business, and village discussions and events that were more commonly attended by men then by women – such as village *shalish* (informal arbitration) meetings.

3) The unit of analysis (or case) was the individual and provided a useful contrast with the household survey where the household was the case. This helped to correct some of the biases introduced by using households as cases and provided insights into the processes underlying household formation, growth, decline, and dissolution. Sometimes parts of the life histories described life within a different household - for example, when talking about life before marriage.

4) Conducting two interviews per household was not twice the effort of one interview per household because a large part of the cost and time involved travelling, finding households, and arranging times to interview. When the team could interview two individuals in one household at roughly the same time, this was quicker and cheaper than interviewing two individuals in different households in different locations. In Bangladesh, where it is preferable for men to interview men and women to interview women, a mixed-sex team worked well for interviewing two individuals in one household at one time before moving to the next household.

After each life history interview had been conducted, interviewers wrote up the interview in Bengali on the same day in a format that had been formulated in the initial workshop and refined in the field during discussions with the author. Interviewers also kept fieldwork diaries in which they noted their reflective impressions of the interviews and lessons learned about methods. These diaries were translated and became a part of the qualitative dataset.

All interviews and focus-group discussions were recorded with small unobtrusive digital voice recorders, with the permission of the research participants. We did not attempt to write full transcripts; but the digital recordings were used for checking back on interviews for the initial same-day write-up in Bengali, for later analysis, and for the final anonymised write-up in English.[17] Each life history was written as a chronological account of life events, identifying causal mechanisms and drawing from discussions that encouraged counterfactual thinking. The interviews did not intentionally focus on any

[17] Some annotated and anonymised examples of these life histories can be found at http://www.sdri.org.uk/bangladesh.asp. If funding allows, it is planned to add to these in the future.

particular development interventions; rather, the aim was to produce, as accurately as possible, the participant's perspective on his or her life trajectory, the causes behind improvement or decline in wellbeing, and how life could have been if the events that emerged—both positive and negative—had not occurred.

Alongside each narrative life history a diagram was drawn. Each diagram was drafted during the interview with participation from the interviewee and then later redrawn neatly on a large sheet of graph paper. On return visits these sheets became a focus for checking events and their effects. Diagrams were then scanned and redrawn on computer with labels anonymised and translated into English. On each diagram the level of wellbeing at different points in the life trajectory was indicated using a scale of one to five using the categories described in Table 2, based on life-conditions described by the respondent and the focus-group discussions. Appendix 2 contains examples of the final diagrams with wellbeing levels added.

Wellbeing levels were checked during the final village-level discussion with people who knew the households well, and then were written onto the life-history diagrams during a round-table discussion among the researchers who had carried out the life-history interviews and facilitated the village discussion groups. In these final discussions all information about households and members was used, and levels of wellbeing were agreed by consensus after discussion. These discussions were also digitally recorded – creating another data source about the households which could be coded and analysed in nVivo8.

During the life history (Phase 3) fieldwork in each village at least one (sometimes more) additional focus group discussion was carried out in order to map a history of the village. The 'development' of the village was mapped on a trajectory diagram similar to the individual life-history diagram, recording important village events, periods of opportunity, perceived causes (new crops, irrigation, new roads, electricity etc.) and community-level shocks (storms, floods, crop failure, fertiliser shortages etc.). These discussions were usually held in a school building or near a village leader's house. We tried to have a Union Council (*parishad*) member and a number of elderly people

in attendance. These focus-group discussions tended to be dominated by men so in most sites the female researchers also facilitated separate focus groups with women to mitigate this male bias.

We feel strongly that it is difficult to conduct qualitative research of this kind without research analyst/s spending significant time in the field. Even when an analyst is not a local language speaker (in our case he was) we would suggest an initial workshop, pre-testing exercises and set of first interviews with the analyst present. After the interviews have been completed with notes and diagrams translated, we found it was also useful for the analyst to revisit all households with interviewers. This allowed the most important points from initial interviews to be followed up with participants and allowed the original interview write-ups and diagrams to be checked (which is an added incentive for field researchers to accurately write up interviews). It also gave the analyst a feel for data quality and allowed participants to explain complex issues. These follow-up interviews included recording short videos which became part of the data set and were useful for memory jogging during later analysis and writing.

4. Lessons Learned from Combining Medium-*N* and Large-*N* Studies: Going for Breadth *and* Depth

One of the key differences between qualitative and quantitative fieldwork is the number of cases (*N*) selected for study. Quantitative studies, with their reliance on statistical techniques to analyse data, typically requires large sample sizes and the analysis of a limited set of pre-defined attributes or variables. Following Gerring (2005) we refer to these studies as large-*N* studies. The household survey in Phase 2 of our study, resurveyed 1907 core households in 102 villages located in 14 of Bangladesh's 64 districts, may be regarded as a large-*N* study allowing statistical analysis of variables and relationships between variables.[18] Large-*N* studies are particularly powerful because numerical data from a large number of 'cases' can be analysed. They also allow inferences to be made from samples which are more representative of an entire population.

[18] Cross-sectional household surveys conducted by national statistical agencies usually have much larger sample sizes than panel surveys. An extreme case would be the Socioeconomic Survey (Susenas) in Indonesia which surveys over 200,000 households every three years. Even in less populous countries, like Botswana, national household income and expenditure surveys sample around 6,000 households.

In contrast, qualitative research usually relies on smaller numbers of cases but with more scope for within-case exploration, and much greater flexibility in following interesting leads in the field when they arise. The smaller sample size in qualitative research allows researchers to carry out what Clifford Geertz referred to as 'thick description' (Geertz 1973). For poverty studies this encourages a multidimensional stance and attention to spatial and temporal context. The number of cases that can be examined is limited because qualitative fieldwork tends to take longer, generates more information per case, and aggregation and analysis of this information is more challenging because it is (initially at least) non-numerical. In contrast to household surveys, the variables to be measured and their attributes are not pre-determined, so the reduction of data to forms that allow comparison between cases (coding) tends to occur in much later in the research process in qualitative than in quantitative studies. Thus qualitative studies tend to allow the examination of a small number of cases, but the range of variables to be examined is left open, and analysis is usually both within, and between, cases.

In quantitative studies, even though a large number of variables can be recorded per case, it is impossible to introduce new variables once the fieldwork has started, although unexpected answers and a few free form response questions can be post-coded.[19] Thus qualitative research is invaluable for exploratory work, or for piloting before a quantitative survey, to make sure that all aspects of interest in a particular context are included in the quantitative survey. In our study, the findings from the focus-group discussions conducted during Phase I helped us refine the household survey questionnaire and reassured us that we were covering all important issues of interest that could arise.

Quantitative researchers tend to talk about 'data collection' while many qualitative researchers prefer to talk about *creating* or *making* data, rather than *collecting* data due to the special nature of qualitative field research. Richards (2005:37) explains that "[collecting data] carries the implication that data are lying around, like autumn leaves, ready to be swept into heaps". She argues that qualitative researchers need to be more aware of their own role in creating data. Field researchers in

[19] In quantitative studies new variables can be created as indices, aggregates or classifications derived from of existing variables. However it is usually not possible to capture new basic variables once the fieldwork has commenced.

qualitative studies therefore need a higher level of guidance and training because the interviews and recording involve more discretion, sensitivity and creativity than quantitative interviewing using questionnaires. In addition, the division of labour between field researchers and analysts is less likely to be as clearly maintained as is common in quantitative surveys.

In contrast, quantitative fieldwork can feel more like 'data collection' because most questions are predetermined, closed and standardised, so as to ensure consistency across the entire sample, making subsequent aggregation and analysis more straightforward. This highlights one of the differences in emphasis between quantitative and qualitative approaches: quantitative studies emphasise the need to prevent the introduction of bias by representative sampling and maintaining consistency between cases, while qualitative studies emphasise an openness and flexibility to new dimensions of enquiry as they arise in the field; they attempt to understand context and complexity in people's lives; and they explore the uniqueness of people's stories before they are aggregated into some kind of comparative form.

Our view is that both sides of the qual-quant divide benefit from combining these priorities. In qualitative studies it is often difficult to make general inferences about a population because cases are too few in number to be representative of the wider population and questions are not standardised. Quantitative researchers, on the other hand, need to recognise the limitations on grounded understanding and explanation imposed by predefinition of categories and numerical representation. The weaknesses on both sides can be ameliorated by their combination. In our study this process started by nesting qualitative subsamples within the larger quantitative sample survey of the population, so that information from each side could strengthen the findings of the other.

Overall we support Shaffer's (2006) call for more integration in mixed-methods studies. This includes exploiting mutual learning and synergy, which involves more than merely deploying quantitative and qualitative methods in parallel in the same project. However, we would supplement Shaffer's analysis by adding that planned sequencing of qualitative and quantitative investigations is often more productive than conducting them simultaneously. Our experience in Bangladesh suggests

that considerable value is added when qualitative research phases inform, and are informed by, a quantitative research phase.

5. Lessons Learned in Exploring Causation Using Mixed Methods

We have already pointed out that qualitative research is particularly useful for exploratory studies, while the power of quantitative research lies in its ability to confirm or refute propositions drawing from larger, and potentially more representative, samples of a population. Of course this distinction is a matter of degree, but due to its more propositional, rather than exploratory stance, quantitative research tends to allow hypotheses and research questions to be more clearly defined and more systematically verified or refuted. In our overall research project, our aims were both exploratory and propositional. We wanted to better understand the range of drivers (or causes) of improvement or decline in peoples' lives, but also the impact of development interventions on poverty.

In this study, we recognized the potential for qualitative and quantitative research to complement each other in evaluating the multitude of events and processes which cause changes in people's life trajectories, including the effect of development programs. Assessing causation is important in poverty dynamics studies, because we are interested not only in observing movements into or out of poverty but also in understanding why these movements take place—and, ultimately, in making informed decisions about what can be done, in policy terms, to support the causes of improvement while protecting people from the causes of decline.

However, identifying the causes of decline or improvement in people's lives is difficult—some would say impossible. Our position is a pragmatic one. We believe it is possible to identify causes by drawing from different approaches to causation across the social sciences, and that we can learn more about the effects of events or interventions on people's lives by using a mix of methods than by using one method alone. Table 3 outlines how the methods used in this study are linked to different approaches to causation and helps illustrate how these approaches can complement each other.

Table 3. Different approaches to exploring causation in poverty dynamics research[20]

	Experimental	Statistical	Process Tracing	Counterfactual
Methods with comparative advantage	quasi or natural experiments comparing intervention with control/ comparison groups	statistical analyses (regression, correlation) examining relationships between variables in household panel data	analysis of sequences of events that trace primary causes through intermediate causes, or causal combinations	counterfactual thought experiments of closest-possible worlds, in which participants are invited to identify causes of change
Predominant view of causation	probabilistic or deterministic	probabilistic	deterministic, but contingent on causal fields	deterministic
Focus on causes or effects?	effects: focus on treatment's effects in experiments	causes: focus on dependent variable in regressions	causes: including intermediate sequences or combinations of causes	effects: would the effect have occurred in a different, closest-possible world without the cause?

In the natural sciences, a common way of exploring relations between cause and effect is by controlled experiments. The nearest analogies to this in the social sciences are randomized control trials (RCTs), in which a treatment or intervention of some kind (such as a conditional cash transfer) is randomly assigned to particular cases and withheld from others (controls). Although this is a common approach in studying health or education interventions in developing countries, the contexts in which such experiments are possible, or ethical, are limited, particularly for the study of poverty dynamics.[21] Natural or quasi experiments, in which exogenous changes to a policy or institution creates intervention and comparison groups serendipitously, can also sometimes be used to study the effect of interventions (Meyer 1995). In poverty studies, particularly when they are longitudinal, it is rarely possible, or even desirable, to have complete control over which treatments are applied to which cases.

[20] We draw here from Henry Brady's four theories of causality: neo-Humean regularity theory, manipulation theory, counterfactual theory, and mechanisms and capacities (Brady 2002).
[21] See Deaton (2009) for an illuminating discussion of the limitations of randomized control trials in development contexts.

In these studies, matching techniques (including, but not limited to, covariate and propensity score matching) are often used as a way of making statistical comparisons between treatment and comparison groups (see Ravallion 2008, Khandaker et al. 2010).

While our three interventions had some features of a quasi experiment, it was generally difficult to maintain clearly defined intervention and control groups in this study. In the microfinance and agricultural technology programs, for example, the interventions were more about having access to services that were exploited to varying degrees, while in the educational transfer sites, the benefits were rolled out over time and were not under the control of the researchers. Over the same time period, microfinance became so ubiquitous in rural Bangladesh, that it was not possible to find households that had no access to microfinance services in the districts we worked in. Thus, though it may have been useful to distinguish between intervention and 'control' households in the initial evaluation studies, over the longer term, a purely experimental approach to causation became more and more problematic with time.

The statistical analysis of relationships among observed characteristics of cases (variables or attributes), forms a second observational approach to identifying causal relationships, most commonly used in the analysis of survey data. This technique builds on the 18th Century philosopher David Hume's idea that causation involves regularity in relations among empirically observed entities. In this case, it is not necessary—and Hume argued, not even possible—to identify the causal mechanisms that underlie correlations between variables (Marini and Singer 1988). This kind of approach observes covariation between 'causes' ($\boldsymbol{x}_{1,2,\ldots,n}$) and an 'effect' ($y$) and is usually expressed as a correlation. The logic which lies behind such statistical analyses uses correlations among variables as possible indicators of causation, without identifying precise causal mechanisms (see Abbott 2001, 132). Within econometrics and macroeconomics, a time series **x** may even be said to 'Granger-cause' another series **y** if current and lagged values of **x** improve the predictability of **y** (Granger, 1969).[22]

[22] Of course, as is pointed out in most econometric textbooks, the Granger causality approach is no more than a statistical generalisation of the *post hoc ergo propter hoc* principle, and it is easy to think of situations when this principle is violated. For example, Christmas card sales regularly precede Christmas, but can hardly be said to 'cause' it!

Experimental and statistical approaches to causation (the two columns to the left in Table 3) underpin most quantitative impact evaluations. Qualitative studies, such as our life history study, are better suited to identifying cause-and-effect relations through process tracing or counterfactual thinking with research participants much more involved in the process (the two columns to the right in Table 3). This allows first-person perspectives on causation to be included which can complement third-person correlation-based analyses.

In the life histories we used process tracing to examine possible causal mechanisms in sequences of life events by drawing on the participants perspectives.[23] The life history interviews also allowed us to encourage counterfactual thinking with participants regarding the effects of reported events or their own actions on their lives, including what they thought could have happened without the events or actions. We also put the counterfactual ideas into practice when we invited participants to discuss and rank the main crises and opportunities according to the effect they had had on their present circumstances. When participants identified events or episodes that had made a significant difference to their present circumstances, we then invited them to consider how things could have been if the particular event had not taken place. So, for example, a statement like, "If I hadn't got the job, I wouldn't have been able to afford medical care for my mother, and she would have died," illustrates a counterfactual thought experiment drawing on a participant's perspective of his or her own circumstances. Sometimes the research participant's interpretation of causes is framed within a worldview which contrasts with that of researchers. For example many of our research participants attributed the ups and downs in their lives to supernatural phenomena. These explanations were recorded and included in the analysis because such perceptions are important in explaining people's actions and understandings – even while discretion is needed in attributing overall causes. For example such findings help explain why seeking out traditional healers is still is still widespread in treating illnesses – sometimes with damaging consequences.

[23] The term *process tracing* is used by political scientists (see, for example, George and Bennett 2005) to describe this kind of activity in political analysis; it refers to peering "into the box of causation" (Gerring 2008: 1). Process and mechanisms are seen as the means by which a cause (X_1) is seen to produce the effect (Y).

This kind of analysis also attempts to uncover the particular circumstances in which observed causal regularities are likely to recur. For example, a statistical correlation may be observed between low income and poor health. However process tracing may identify plausible mechanisms linking low income to ill health, for example, via malnutrition, poor housing, or poor access to clean water. It may also identify the circumstances in which the particular cause-and-effect relationship will occur, such as where cheap and effective health provision is not available. Certainly some intervening variables and contextual conditions can also be measured and covariations analyzed statistically; however, plausible mediating links are best identified through an examination of individual cases making use of first-person experiences.[24]

An analysis of linking mechanisms may also uncover instances in which a causal relationship suggested by a correlation is reversed—for example, in some cases, poor health may cause low income due to physical weakness, inability to retain a job, or increased time spent on healthcare impeding income earning (Deaton 2003).

To sum-up, we believe that within-case analyses of qualitative research (including participant insights), combined with the cross-case analyses of statistical regularities from quantitative research, provides the best chance of reliably uncovering causation in poverty dynamics research. Without case-based research, quantitative researchers rely on anecdotal 'plausibility stories' (see Abbott 2001:132) to explain statistically observed regularities between variables. This becomes more problematic in large-*N* quantitative studies when there is usually a clear division of labour between field researchers and analysts. Similarly, qualitative researchers often construct sophisticated theories of causal links from in-depth investigations of small numbers of cases, which may or may not be representative of the wider population. Combining within-case and cross-case analyses is therefore our preferred method for examining cause-and-effect relationships in poverty dynamics research.

[24] The statistical technique of multilevel analysis which has been used regularly by medical researchers for many years and is now being used more frequently in the social sciences, provides another way to 'tease-out' possible causal links between hierarchies of (possibly nested) variables (Hox, 2002).

6. Lessons Learned in the Analysis and Presentation of Findings

A number of lessons also emerged from the analysis and presentation of findings from this study. Initial analysis of the qualitative data focused on assigning case attributes and coding, particularly the reasons given for decline or improvement in people's lives. After initial coding more analytical coding and the construction of descriptive tables took place exploring more abstract themes (e.g. vulnerability, graduation from poverty, the importance of assets, intergenerational transfers, social exclusion, social stigma, reputation, status etc.). The qualitative material was also analysed for patterns across various categories of research participants.

The initial quantitative analysis focused on understanding the correlates of household movement in and out of poverty, in particular gender and the role of assets in protecting households from shocks. Exploratory graphic and tabular data analysis was followed by a logit analysis of attrition, reduced form OLS regressions, the estimation of contour and lowess plots, covariate matching and instrumented fixed effect regressions.

Integrated analysis of the qualitative and quantitative data then took place. Where the two sets of data disagreed, for example in differences in assessed wellbeing levels at different points in time, each case was been examined to try to determine the reasons for disagreement. As the poverty transition matrices had been based on per capita household expenditure while the life history diagrams were more subjective and holistic assessments of wellbeing, some disagreements between quantitative and qualitative poverty dynamics were bound to occur. We found that expenditure-based transition matrices showed many more poverty transitions (particularly out of poverty) than the life-history-based trajectory diagrams. As the life histories sub-sample was nested within the larger household panel survey, it was possible to compare quantitative and qualitative assessments of poverty transition on a case-by-case basis for this sub-sample. The analysis of these 'disagreements' was undertaken jointly by the co-authors (one of whom is sociologist and the other a development economist/econometrician), and provided many opportunities for mutual learning. Five main explanations of the disagreements between the quantitative and qualitative poverty assessments were developed– with the consideration of assets in addition to expenditures and proximity to the poverty line explaining three-fifth of the disagreements. Throughout the analysis process we considered

instances of disagreement between qualitative and quantitative interpretations of the data as an opportunity for further learning.[25]

We found it was very important for the main qualitative and quantitative analysts to stay in touch and have regular face-to-face interactions. This allowed discussions to occur which allowed questions to be raised from one side of the study which could then be interrogated using data from the other side. Because the qualitative sample was nested in the quantitative sample, this quantitative data could be analysed alongside findings from the qualitative fieldwork. Often hypotheses raised based on the learning from this smaller sample were tested on the larger quantitative data. This has been useful in exploring the effects of phenomena such as dowry, health shocks, household economies of scale and household composition.

During the write-up and analysis phase of the study, we also discovered that quantitative and qualitative researchers tend to have different approaches to the issues of anonymity and the nature of informed consent. For the quantitative research team, obtaining the household's permission to conduct an interview and then anonymising the names of household members and villages from the data files was generally felt to be sufficient to preserve respondent confidentiality. The quantitative team therefore saw no special problem in making the unit record data of the household survey publically available, first in CD form and then via the internet, as long as the names of respondents and villages were removed from the relevant data files.[26] In contrast, the life-history data posed more problems as release of the photographs, recordings, transcripts and trajectory diagrams would make it possible for respondents to be identified. In explaining the qualitative research, respondents had been assured that their names and village names would not be disclosed to anyone outside the research team and that their anonymity would be protected. Due to the nature of the 'thick' descriptions in life history interviews, and the use of actual names in interview recordings, 'raw' data from the qualitative work could therefore not be publically released. Individual life histories could therefore only be made

[25] See Davis and Baulch (2011) for an account of the learning that took place though the examination of these disagreements.
[26] See http://www.ifpri.org/dataset/chronic-poverty-and-long-term-impact-study-bangladesh

public after they had been completely anonymised which was a relatively expensive and time consuming process.[27]

The short video that was prepared of the life history interviews raised even starker problems, since it was almost impossible to disguise life histories respondents' identities in such a medium. Despite several requests it was decided not to distribute copies of this video or place it on the study website.

7. Concluding remarks

At the outset of the Bangladesh study, our intention was to integrate qualitative and quantitative methods to make their combination worth more than the sum of the parts, recognising that each side of the artificial, and increasingly redundant, qual-quant divide could learn from the other, and enhance the validity, reliability and policy impact of our findings. In poverty research, 'putting together' studies, which have little integration in terms of both field work and analysis, are much more common than 'methodological integration' of which there are only a few examples (Shaffer, 2006).[28] While there are a few examples of mixed-methods studies in Bangladesh (e.g. Kabeer 2004; Hallman et al. 2007 for our agricultural technology sample) full 'methodological integration' is surprisingly rare given the large number of poverty studies undertaken.

Qualitiative studies are often criticised for being interesting but anecdotal while quantitative studies are often criticised for being insufficiently grounded or oblivious to first-person experiences. One of the principal advantages of our approach of nesting a qualitative life-histories sub-sample within the larger quantitative household panel survey is that the issue of generalisability can be directly addressed. Similarly, the understanding which the focus groups and life history interviews provided about the community context and individual motivations for taking certain actions provided a substantial grounding for the quantitative research. This was especially valuable in studying the drivers of poverty dynamics, in which a number of hidden causes of socio-economic mobility (e.g.

[27] See http://www.sdri.org.uk/bangladesh.asp for a selection of annotated and anonymised life histories from the Phase 3 of the study.
[28] See da Silva (2006) for an annotated bibliography of recent 'q-squared' analyses of poverty.

dowries, life cycle patterns, insecurity and power relations) that are often overlooked in quantitative analysis alone.

The pairing of qualitative and quantitative data has also allowed us to go much further in probing causation than either the qualitative or quantitative data would in isolation. Adopting a deliberately mixed methods approach, with an eclectic approach to causation, both in conceptual and research methods terms, strengthened our ability to make causal statements about the poverty consequences of common sequences of (positive and negative) events. Such statements are useful both in understanding the drivers of poverty dynamics and in the design of anti-poverty interventions and social protection measures.

To sum-up, the use of an integrated and sequenced 'qual-quant-qual' approach in this study of poverty dynamics achieved much more than if qualitative and quantitative research had been undertaken separately and then 'put together'. The research findings are based on a more robust and representative body of evidence than is usual, and which challenged the researchers to extend their skills beyond their familiar comfort zones. In the analysis it was particularly valuable to be able to interrogate the life histories sub-sample of the larger household panel in order to formulate new ideas and test them based on all the evidence available. This kind of working-together required an openness and a shared commitment to learning from all evidence, of whatever kind, and the ability to overcome entrenched disciplinary positions. Our hope is that sequenced and integrated mixed-methods research will eventually become mainstream in studies of poverty dynamics, so that public policy can be based on a deep as well as a wide body of evidence.

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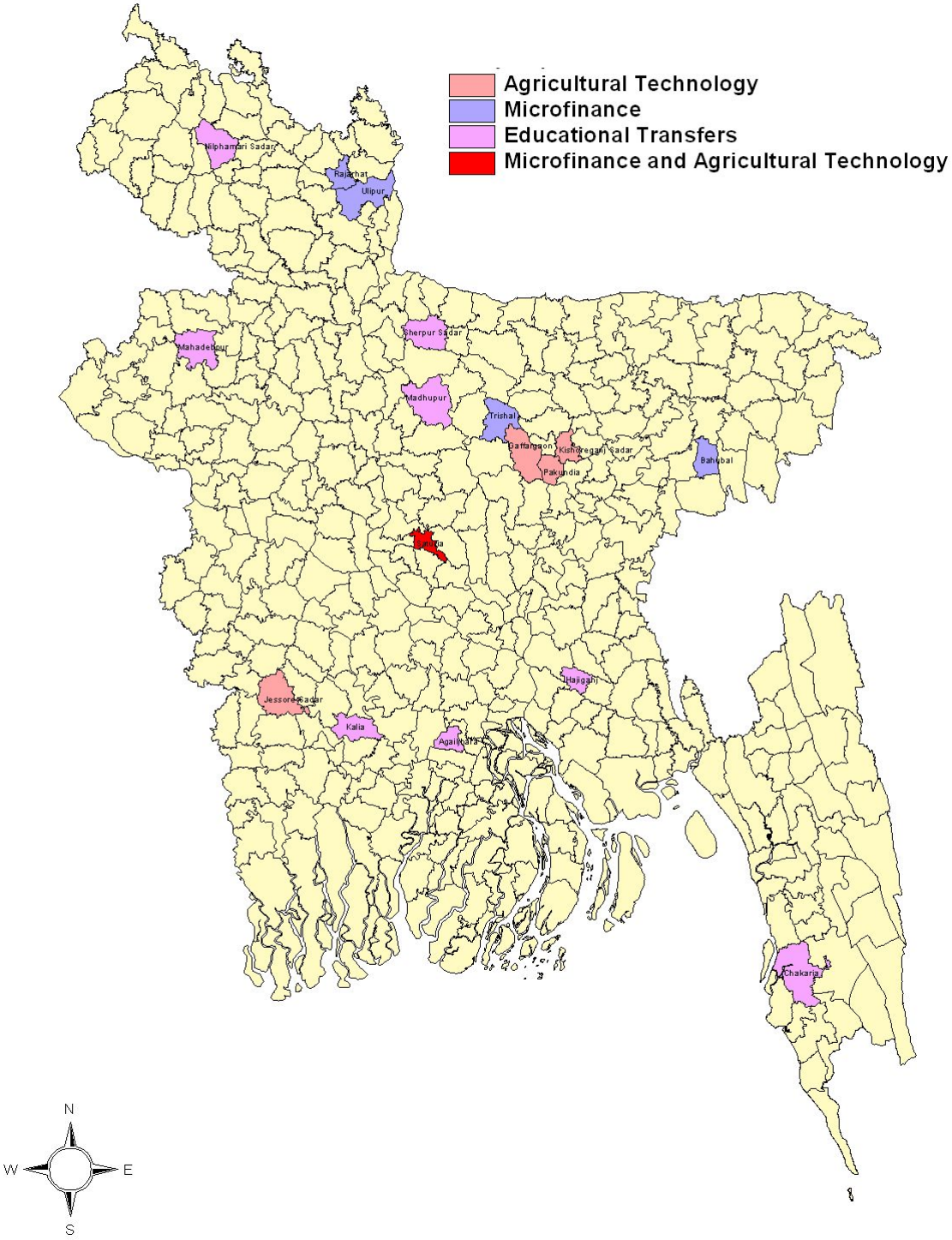
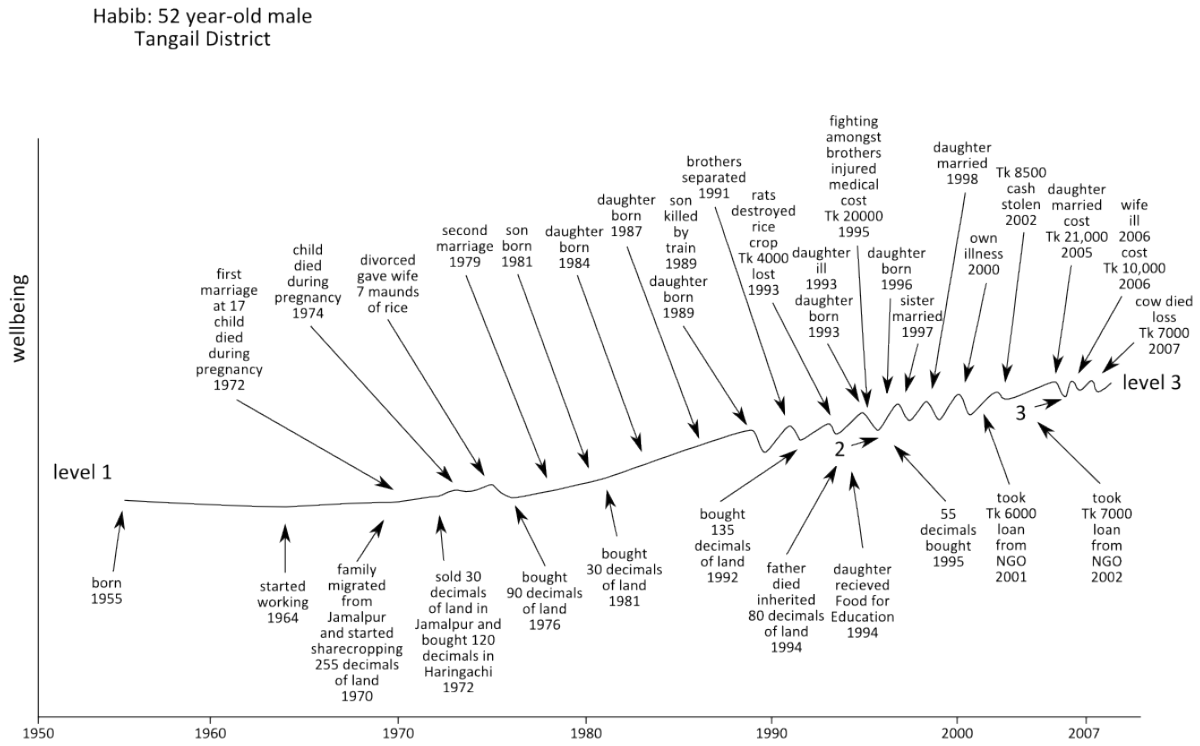
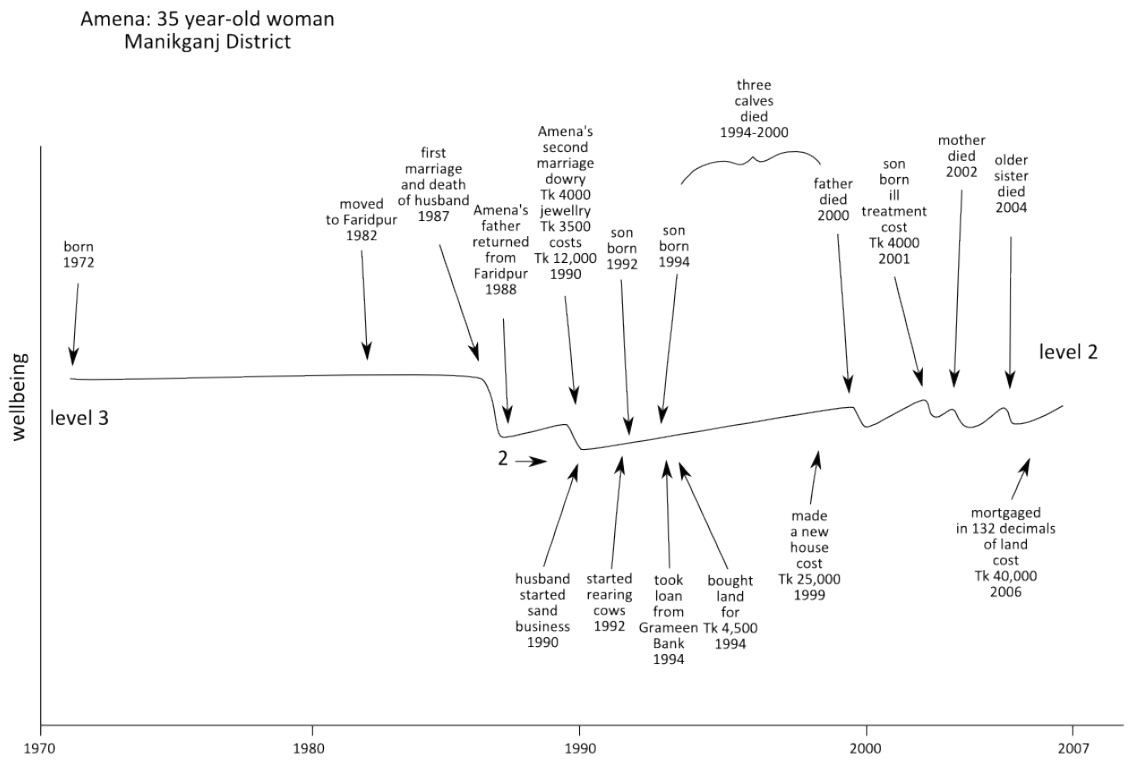
Appendix 1: Map of the thanas/ upazilas surveyed by intervention

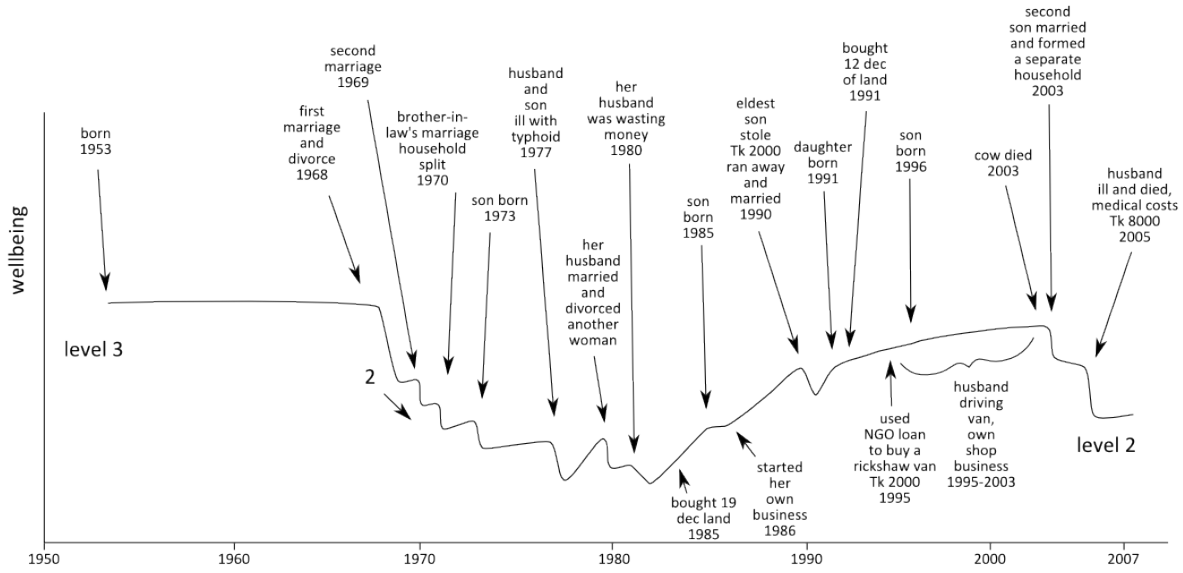


Appendix 2: Examples of life history diagrams





Amena: 54 year-old woman
Nilphamari District



Malek: 46 year-old male
Cox's Bazar District