QUAL-QUANT

QUALITATIVE AND QUANTITATIVE POVERTY APPRAISAL: COMPLEMENTARITIES, TENSIONS AND THE WAY FORWARD

Contributions
to
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CONTENTS

CONFERENCE PROGRAM AND PARTICIPANTS i

Q-SQUARED? A COMMENTARY ON QUALITATIVE AND QUANTITATIVE POVERTY APPRAISAL Ravi Kanbur 1

QUALITATIVE AND QUANTITATIVE POVERTY APPRAISAL: THE STATE OF PLAY AND SOME QUESTIONS Ravi Kanbur 17

QUALITATIVE APPROACHES: SELF CRITICISM AND WHAT CAN BE GAINED FROM QUANTITATIVE APPROACHES Robert Chambers 22

THE BEST OF BOTH WORLDS? Robert Chambers 26

SELF-CRITICISM AND OBSERVATIONS ON THE WAY TO FINISHING *VOICES OF THE POOR: FROM MANY LANDS* Patti Petesch 30

BRIDGING QUANTITATIVE-QUALITATIVE DIFFERENCES IN POVERTY APPRAISAL: SELF-CRITICAL THOUGHTS ON QUALITATIVE APPROACHES Norman Uphoff 33

CAN QUALITATIVE METHODS HELP QUANTITATIVE POVERTY MEASUREMENT? Martin Ravallion 38

QUALITATIVE AND QUANTITATIVE APPROACHES TO POVERTY ANALYSIS: TWO PICTURES OF THE SAME MOUNTAIN? Francois Bourguignon 44

STRENGTHENING QUANTITATIVE METHODS THROUGH INCORPORATING QUALITATIVE INFORMATION David E. Sahn 47

'APT ILLUSTRATION' OR 'ANECDOTAL INFORMATION?' CAN QUALITATIVE DATA BE REPRESENTATIVE OR ROBUST? Caroline Moser 52

INTEGRATING QUALITATIVE AND QUANTITATIVE APPROACHES: LESSONS FROM THE PASTORAL RISK MANAGEMENT PROJECT Chris Barrett 56

TOWARDS A BETTER COMBINATION OF THE QUANTITATIVE AND THE QUALITATIVE: SOME DESIGN ISSUES FROM PAKISTAN'S PARTICIPATORY POVERTY ASSESSMENT DavidBooth 60

POTTERS AND SLUMS: TWO QUALITATIVE/QUANTITATIVE PROJECTS IN INDIA Vijayendra Rao 65

THE QUAL-QUANT DEBATE WITHIN ITS EPISTEMOLOGICAL CONTEXT: SOME PRACTICAL IMPLICATIONS Luc Christiaensen 70

INTEGRATING THE QUAL AND THE QUAN: WHEN AND WHY? Jesko Hentschel 75

DIFFICULTIES IN COMBINING INCOME/CONSUMPTION AND PARTICIPATORY APPROACHES TO POVERTY: ISSUES AND EXAMPLES Paul Shaffer 80

QUALITATIVE AND QUANTITATIVE POVERTY APPRAISAL WORKSHOP: SOME REFLECTIONS AND RESPONSES **Rosemary McGee** **85**

WHAT IS REQUIRED TO REDUCE TENSIONS AND INCREASE COMPLEMENTARITY? **Rosemary McGee** **89**

DATA AS SOCIAL PRODUCT: PROBLEMS IN QUALITATIVE AND QUANTITATIVE WORK **Ron Herring** **91**

THE EMPLOYMENT PROBLEM IN SOUTH AFRICA: FROM COINTEGRATION TO MR. ISAACS **Gary S. Fields** **95**

POVERTY RESEARCH: EXTRACTIVE OR EMPOWERING? **Alex Wilks** **99**

TENSIONS, COMPLEMENTARITIES AND POSSIBLE CONVERGENCE BETWEEN THE QUALITATIVE AND QUANTITATIVE APPROACHES TO POVERTY ASSESSMENT **Erik Thorbecke** **103**

CONFERENCE PROGRAM AND PARTICIPANTS

Qualitative and Quantitative Poverty Appraisal:
Complementarities, Tensions and the Way Forward

Sponsored by
The MacArthur Foundation
Poverty, Inequality and Development Initiative
Cornell University
March 15-16, 2001
401 Warren Hall

March 15
9.00—9.30
Chair: Alaka Basu
Opening: Qualitative and Quantitative Poverty Appraisal: The State of Play and Some Questions
Ravi Kanbur
9.30—11.00
Session I: Qualitative Approaches: Self-Criticism and what can be gained from Quantitative Approaches
Robert Chambers, Patti Petesch and Norman Uphoff

11.00—11.30
Break

11.30—1.00
Chair: Elizabeth Peters
Session II: Quantitative Approaches: Self Criticism and what can be gained from Qualitative Approaches
Martin Ravallion, Francois Bourguignon, David Sahn

1.00—2.30
Lunch

2.30—4.00
Chair: David Lewis
Session IIIA: Best Practices
Caroline Moser, Christopher Barrett, David Booth, Vijayendra Rao

4.00—4.30
Break

4.30—6.00
Chair: Muna Ndulo
Session IIIB: Best Practices
Luc Christiaensen, Jesko Hentschel, Paul Shaffer

7.30
Dinner

March 16
9.30—11.30
Chair: Ravi Kanbur
Session IV: The Future: What Is Required to Reduce Tensions and Increase Complementarity?
Rosemary McGee, Ronald Herring, Gary Fields, Alex Wilks, Erik Thorbecke

Lunch and end of workshop

Qualitative and Quantitative Poverty Appraisal:
Complementarities, Tensions and the Way Forward

Sponsored by
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Cornell University

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401 Warren Hall

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Q-SQUARED?

A Commentary on Qualitative and Quantitative Poverty Appraisal

Ravi Kanbur
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SUMMARY

This compilation brings together the proceedings of a workshop on "Qualitative and Quantitative Poverty Appraisal: Complementarities, Tensions and the Way Forward." Contributors were asked to submit short summaries of their positions, with detailed references to the literature as necessary. The compilation represents a remarkable statement of the state of the art and the debate on "Qual-Quant", at a time when the complementarities between the qualitative and the quantitative traditions in poverty analysis are being recognized, but the tensions are ever present, and analysts and policy makers are looking for a way forward in using the two approaches to design effective poverty reduction strategies.

The workshop spent a fair amount of time characterizing the two traditions along different dimensions, with (at least) the following five dimensions being identified:

1. Type of Information on Population: Non-Numerical to Numerical.
2. Type of Population Coverage: Specific to General.
3. Type of Population Involvement: Active to Passive.
4. Type of Inference Methodology: Inductive to Deductive.
5. Type of Disciplinary Framework: Broad Social Sciences to Neo-Classical Economics

Different contributors collapsed these dimensions in different ways, and used different sets of terminology. But there seems agreement that points more to the "left" in the above spectra are more "qualitative" in nature, while those more to the "right" are "quantitative". Some participants pronounced themselves to be in the "extreme center". What is important, it was generally agreed, are the strengths and weaknesses of locating at different points along these dimensions, for the purpose at hand.

It was recognized that there are indeed strengths and weaknesses at each end of a given spectrum. Numerical information can be more easily aggregated, but it can miss out on nuance and texture. General coverage aids representativeness, but can lose context. Statistical inference can help in discussion of causality, but misses out on the power of inductive approaches. And so on. The key, then, is how to make the best of complementarities while minimizing tradeoffs.

There was general support in the workshop for “small movements” from either extreme of any of the five dimensions. The support was strongest, almost universal, for the first two dimensions. For example, those in the qualitative tradition agreed that some numerical information could and should be collected in participatory poverty appraisal (PPA). There was also agreement (less strong) that the credibility of qualitative studies with policy makers and others would be greater if site selection could be put into a sampling frame – preferably, the same sampling frame as for a nationally representative household survey. The PPA exercise, for example, could be done on the site using its characteristic methodology as before, but the results could now be situated in a broader context. By the same token, those in the quantitative tradition agreed that the introduction of more subjective and open ended questions in a standard household survey could in fact provide a better handle on defining and measuring poverty even in standard income/consumption terms. Qualitative work could also help in (i) suggesting causal connections to be econometrically tested, (ii) understanding statistical outliers, (iii) finding appropriate "exclusion restrictions" in econometrics and (iv) appreciating the extent of measurement error. Moreover, initial participatory exercises could suggest questions for inclusion in the standardized surveys.

However, it was also recognized that there were limits to how much of such “simultaneous mixing” could be done without losing the essence and the strengths of either end of the spectrum in question. In any event, such mixing was more possible for some dimensions than others – the workshop did not have as many useful suggestions for mixing along dimensions 4 and 5 above, for example. In view of this, the issue arose of “sequential mixing”, where each end of the spectrum is called upon to do its best, and the integration takes place between the results of the two approaches so devised. It was recognized that this was difficult, not least because of disciplinary divides in dimension 5.

Moreover, such mixing would require institutional arrangements to take account of the current dominance of the quantitative approach in policy-making circles. Relatedly, those in the qualitative tradition were more concerned about the ethics of doing research on poor populations with no direct benefit to the poor themselves (and considerable benefit to the researchers). They emphasized the importance of communicating research findings back to the poor, in a language they could understand. These issues were not on the agenda of those in the quantitative tradition, whose rationale is more that if research influences policy directly or indirectly and thus helps the poor in turn, then it is worthwhile.

It was generally agreed that this first Qual-Quant workshop was hugely successful in what it did. But we have probably got as far as we can at this level of generality. An interesting dialogue has been opened up among some of the leading practitioners from both traditions. The next step should be to get more specific, focusing in greater detail on particular case studies of simultaneous or sequential mixing, to understand in greater depth the strengths and weaknesses of different points along the five dimensions identified above, and complementarities and tensions in mixing them.

I. INTRODUCTION

Poverty analysts in the "Qualitative" and in the "Quantitative" traditions have been highly active in the policy debates of the past decade. While quantitative approaches have been dominant, especially in policy-making circles, the use of qualitative approaches has been increasing. Many bilateral and multilateral agencies now routinely commission studies in this tradition. There have also been increasing attempts at integrating the two approaches. But systematic attempts have been few, and successes have been even fewer. While there is a general acceptance, at least at the level of rhetoric, of the obvious complementarities between the two approaches, the tensions are more than apparent. The situation has undoubtedly improved compared to a decade ago, but practitioners in the two traditions still seem to inhabit unconnected worlds, with their own conferences, their own academic journals, and separate departments of (the same) aid agencies who sponsor their work. The main point is that practitioners do not seem to talk to each other as much as they ought to, given the common objective of helping to develop sound poverty reduction strategies.

This is the context of the workshop, "Qualitative and Quantitative Poverty Appraisal: Complementarities, Tensions and the Way Forward," held at Cornell University on March 15-16, 2001.[1] A central tenet of the workshop was self-criticism. Rather than defending their approaches, the two traditions were asked to be self-critical and ask what the other approaches could bring to the task of understanding and reducing poverty. A second tenet was the search for best practice in combining the two approaches, through discussion of actual examples. These tenets contributed to the quest for mutual understanding rather than defense of turf, which it is all too easy to resort to.

The contributions in this collection represent a remarkable perspective on the state of the art, and the debate, on poverty appraisal. They were all presented and discussed at the Cornell workshop. Contributions were invited not in the form of conventional "30-page" papers, but brief statements, "2-pagers" as they came to be known, in which participants could set out the key issues and their central points, backed up by references to more conventional papers. These "2-pagers" are reproduced here with only minor revisions by the authors.

In this Commentary I want to highlight what appear to me to be the major points emerging from the written contributions and from the workshop discussions. I cannot of course do full justice to the full range of issues covered and discussed, and my emphases are likely to be as idiosyncratic as anyone else's, but I hope that the overview does underline the areas of agreement, the lessons learnt, and the remaining areas for debate and discourse.

[1] The organizers thank the Poverty, Inequality and Development Initiative of Cornell University and the MacArthur Foundation for their support.

II. WHAT EXACTLY ARE THE TWO APPROACHES?

There was considerable discussion of definitions. What exactly do we mean by "Quantitative" and "Qualitative"? In his contribution to this collection, Kanbur follows Carvalho and White in defining terms as follows:

> "The quantitative approach to poverty measurement and analysis is defined here as one that typically uses random sample surveys and structured interviews to collect data – mainly, quantifiable data – and analyzes it using statistical techniques. By contrast, the qualitative approach is defined as one that typically uses purposive sampling and semi-structured or interactive interviews to collect data – mainly, data relating to people's judgments, attitudes, preferences, priorities, and/or perceptions about a subject – and analyzes it through sociological or anthropological research techniques."

But this characterization, which would be recognizable to many, was questioned by a number of people as being somewhat misleading. As Jesko Hentschel notes in his contribution:

> "Often, the terms 'qualitative' and 'quantitative' are used to describe both the methods of data collection (i.e. closed ended household survey, participatory ranking exercise, direct observation) as well as the type of data collected (percentage of children enrolled in schools, parents reasons why they don't send their kids to school). But what is generally referred to as 'quantitative' methods (large-scale household survey) often produce 'qualitative' data (reasons of parents not to send their children to school) and vice versa."

Hentschel might also have added that even such "qualitative" data as "reasons why parents don't send their children to school" could be turned into numerical form by coding the different reasons, and this is often done.

David Booth and Jesko Hentschel offer a useful distinction between types <u>data</u> on the one hand, and types of data collection <u>methods</u> on the other. On the latter, they propose the terminology of "contextual" and "non-contextual," meaning by the former methods "that attempt to capture a social phenomenon within its social, economic and cultural context," while in the latter, "the sampling, the interview schedule, the training of enumerators and other aspects of best practice survey technique are designed precisely to collect information that is untainted by the particularities of the context in which it is described" (Booth et al., 1998). Booth and Hentschel prefer to reserve the term "quantitative" and "qualitative" for the types of data collected, although a more accurate terminology might be "numerical" and "non-numerical", and this would also avoid confusion with the more general usage of the terms qualitative and quantitative.

There were several variations on this theme of types of data and collection methods in the contributions. Ron Herring cautioned that contextuality is present everywhere, even in seemingly "untainted" sample survey methods:

> "I have seen researchers use data from a rural credit survey in Pakistan, for example, that I know to be bogus: there were many areas of the country into which the questioners were afraid to go for reasons of personal safety but felt compelled to fill out the forms anyway. There were also questioners in this survey who abandoned attempts to find the sampled households and made up answers on the basis of conversations at the local tea shop (which I dubbed the Chai-stall error) ….. Variables more important than instrument and method may therefore be interest and trust – the relationship between interviewer and subject …."

While Booth talks of participatory methods as a sub-class of contextual methods, Robert Chambers offers a different classification in his contribution, thinking of participatory methods as the other end of the spectrum from "established" methods. A second spectrum, which he labels from qualitative to quantitative, then captures variations in methods ranging from contextual to structured uniform interview questionnaires. Social anthropology, involving participant observation is then, according to him, an established qualitative approach. On the other hand, participatory poverty research, at the other end of the spectrum from established approaches, can produce numerical (what he calls quantitative) or non-numerical (or what he calls qualitative) data. Chris Barrett also offered his own classification, "ranging from the specific to the general on one axis and from subject-driven to researcher-driven on the other axis. Qualitative autobiography would occupy one corner (specific/subject-driven) of that box, a quantitative census the opposite corner (general/researcher-directed), with considerable opportunity for creative combination of qualitative and quantitative in between."

While the contours of clarity are slowly emerging from these attempts at typology and classification of types of data, there is clearly some way to go, and there is still room for confusion. I would like to propose the following line of argument, starting from the Booth-Hentschel approach, but with significant modifications. The words "qualitative" and "quantitative" have probably lost precision in this context since they are used generally and generically. They serve a useful purpose in conjuring up a picture of what one has in mind and, perhaps, this is what will always happen no matter how precisely we specify the meaning . However, I think it is important to keep the distinction between types of data and types of collection methods. On types of data, I prefer to use "numerical" and "non-numerical" as opposed to quantitative and qualitative, given my observations on the latter two words above, and given that the new suggested words reflect more accurately what is being discussed.

On types of methods, the contextual/non-contextual discussion of Booth and Hentschel gets at the right issues but, following Herring, seems to suggest that context does not matter for collecting household survey information, for example. The distinction is surely more to do with how wide-spread is the data collection effort, how representative is the population covered, and how uniform is the instrument for studying the phenomenon under consideration – Barrett's terminology of "specific" versus "general" captures this spectrum well.

We have furthermore the Chambers spectrum from "established" to "participatory". The issues here are well captured by the terminology of Barrett's "subject-driven to researcher-driven" spectrum, since what I think Chambers has in mind, for example in contrasting the participant observation methods of established social anthropology with the participatory poverty appraisal tradition, is the extent to which the researcher versus the poor set the agenda. But perhaps the terminology could be improved (the word "subject", for example, itself suggests a process where the poor have no control of the process, and the word "participatory" has its own historical baggage). One suggestion I have for labeling the spectrum is from "passive involvement" to "active involvement" of the population on whom the information is being collected. It should be clear that the active-passive labeling is to do with the design of the information collection effort – as Herring's caution makes abundantly clear, even when passive involvement is in the design (as in the assumption that households simply answer household survey questionnaires truthfully, or that they can be contacted at all) the reality on the ground may be quite different.

Consider now alternative methods of analysis and inference, which also characterize the different approaches normally classified under "Qual" or under "Quant". Erik Thorbecke characterizes the former as inductive and the latter as deductive. Gary Fields in his contribution highlights the power of the deductive method, and Francois Bourguignon's characterization of the qual-quant difference as "two pictures of the same mountain" nevertheless relies on a deductivist logic. But inductive approaches also form a substantial part of the social sciences. As Luc Christiaensen elaborates in his contribution:

> " The school of thought associated with quantitative poverty assessments is logical positivism. In this view, there exists a single external reality and it is the analyst's task to capture this as closely as possible. To do so, the analyst seeks to increase the likelihood of unbiased, objective answers mainly by relying on statistical principles in its study design….Qualitative research methods on the other hand are associated with the interpretivist and constructivist tradition. These views start from the recognition of a multitude of realities, and believe that objectivity and value-free science are simply impossible… To fully understand the topic of interest within its context, the inquiry methods used seek to involve many stakeholders and to obtain multiple perspectives on the subject of research and the meaning of the concepts…."

Finally, there is also a disciplinary dimension associated with, but not perfectly correlated with, the inductive-deductive divide. It is not so much a continuum as an "economics versus the rest" divide. Alex Wilks puts the issue squarely on the table as follows: "Economists are normally prepared to generalize far more than people trained in, for example, an area studies approach. To people with training in other social sciences … the use of econometric analyses to try to 'prove' causal relationships is extremely mystifying and almost totally impenetrable." He quotes Heilbroner and Milberg approvingly: "Economics must come to regard itself as a discipline much more closely

allied with the imprecise knowledge of political, psychological, and anthropological insights than with the precise scientific knowledge of the physical sciences." Now, the other social sciences have themselves been moving in the deductive direction, but the fact remains that the economic method is decidedly more deductive than the other social sciences.

I propose, therefore, that the following five dimensions can capture most of the key features of information collection and analysis we see around us:

1. Type of Information on Population: Non-Numerical to Numerical.
2. Type of Population Coverage: Specific to General.
3. Type of Population Involvement: Active to Passive.
4. Type of Inference Methodology: Inductive to Deductive.
5. Type of Disciplinary Framework: Broad Social Sciences to Neo-classical Economics.

I believe most concrete studies can be located somewhere along these five dimensions. They are also useful because they serve to pin down the picture we have in our mind's eye when we think of "Qualitative" or "Quantitative" in a general sense. Analyses which are based on non-numerical information, which are specific and targeted in their population coverage, which in their design require active involvement from the population covered, which use inductive methods of inference and which operate in the broad framework of social sciences other than economics, we tend to label as "Qualitative". Those which are based on numerical information, which are general in their population coverage, which require only passive involvement of the population covered, which use deductive (usually statistical) methods of inference and which rely on the neo-classical economic framework, we tend to label as "Quantitative."

The five dimensions allow plenty of mixing and matching, and we see this in reality. As already discussed, many studies thought of as "Quantitative" because they are based on general household surveys and because they make heavy use of statistical inference techniques, in fact collect and analyze information of the non-numerical ("Qualitative") type. Studies in anthropology ("Qualitative") often allow only passive response from the subjects of the study.[2] Participatory poverty appraisal methods can indeed generate numerical data ("Quantitative"), etc. What the five dimensions allow us to do is to decompose the characteristics of each specific study or analysis to compare with others. They also allow us to have generic discussions of the costs and benefits of locating at different points along each spectrum, depending on the task at hand.

Now, five dimensions may be too many to carry around with us, and the contributions to this workshop have implicitly been focusing or aggregating in one way or another. Thus Booth and Hentschel's proposal can be seen as collapsing these five dimensions into just two. Their first dimension is what they refer to as type of data (although they label the

[2] In correspondence, Biju Rao argues that good anthropological research does indeed involve subjects in an "active" way, albeit not in the sense of "action research" which directly improves the participants wellbeing.

spectrum as qualitative to quantitative). This corresponds exactly to our Dimension 1. Their second dimension, from contextual to non-contextual, corresponds most strongly to our Dimension 2. But there are elements of Dimension 3 in there as well. This is what explains on the one hand Booth arguing that participatory methods are a sub class of contextual methods, and on the other Chambers wishing to put participatory work on a totally different dimension. Booth and Hentschel do not really have a discussion of our Dimensions 4 and 5.

Barrett's classification focuses on our Dimensions 2 and 3, and he argues that at any point along these dimensions one can have numerical and non-numerical information collected and analyzed. He does not have much to say on inductive versus deductive methods or on economics versus other disciplines, except that judicious use must be made of a mix along these dimensions. Chambers focuses on our Dimension 3, which in his terminology is "Participatory to Establishment". His other Dimension, "Qualitative to Quantitative," is a combination of our Dimensions 1, 2 and 4. On disciplines, he explicitly notes that non-economic social sciences as much as economics can use passive rather than active involvement of the population.

For my part, after the workshop I feel that these five dimensions are really needed to give a full account of the differences that were discussed at the workshop, and will serve us well as a check list in situating one analysis in comparison to another.[3]

III. Strengths and Weaknesses

Participants from the two traditions were asked to be self-critical in identifying the weaknesses of their tradition and the strengths of the other. Francois Bourguignon, from the quantitative end, noted in the workshop that "quantitativists seem sometimes to lack a talent for self criticism." However, in his own contribution he stressed the pitfalls of simplistic econometric approaches in uncovering causality. Martin Ravallion laid out the well known problems of standard techniques of poverty measurement in the quantitative tradition:

"The first such problem is the *identification problem* of how to weight aspects of individual welfare not revealed by market behavior … The second problem is the *referencing problem* of determining the reference level of welfare above which one is deemed not to be poor – the poverty line in welfare space, which must anchor the money-metric poverty line … Addressing the identification and referencing problems requires information that is not found in conventional objective socio-economic survey data for representative samples of households. Sample surveys restricted to the standard 'objective' data have been found to be of somewhat limited use for measurement and policy."

[3] Paul Shaffer has commented, in correspondence, that these five dimensions are derivative of a typology in the philosophy of social science which distinguishes between (i) empiricism/positivism, (ii) hermeneutics/constructivism and (iii) critical theory/critical hermeneutics.

David Sahn raised another aspect of standard money-metric based analyses of poverty when he noted:

> "We know that health and educational attainment, to say nothing of measures of social exclusion, insecurity, and other measures of deprivation, are often only weakly explained by incomes or expenditure. The correlations between money metric and other measures of poverty are often quite low. Likewise, the use of multivariate analysis to see how well, for example, incomes predict nutritional or health outcomes, has generally uncovered very weak relationships."

The strengths of quantitative approaches are, perhaps paradoxically, best laid out by Robert Chambers in his contribution, and they were generally accepted by all participants. These include:

> "*time series comparisons to identify trends in whatever dimensions are measured;
> *cross-section comparisons between different individuals, households, groups and communities, and across regions, countries and continents;
> *correlations which identify associations which raise questions of causality and covariant changes;
> *estimates of prevalences and distributions within population areas;
> *triangulation and linkages with qualitative data;
> *the credibility of numbers in influencing policy-makers;
> *the utility to policy-makers of being able to put numbers on trends and other comparisons."

Norman Uphoff touched on related themes in his comments on the Voices of the Poor Study, which was introduced and discussed by Patti Petesch in her contribution. After praising the study for communicating "to persons who have heretofore considered poverty essentially in abstract, faceless quantitative terms, what it feels like to be among "the poor", he goes no to ask three questions:

> "(1) How can we know how **representative** are the persons who speak as members of 'the set of poor people'? (2) How can the different aspects of being poor be **summarized**, to gain some idea of the severity of magnitude of personal poverty? (3) And how can we know **trends** among the poor, whether their numbers are increasing or decreasing, and whether their poverty is getting 'deeper' or more bearable over time?"

Interestingly, while Robert Chambers argued that quantitative approaches could suggest causalities to be further investigated, many in the quantitative tradition saw the development and formulation of hypotheses as strengths of the qualitative approaches. This is particularly apparent in the case studies that were described during the workshop. Chris Barrett discussed how initial participatory appraisal preceded questionnaire design, and "underscored issues the importance of which we had not previously appreciated … 'Ethnography' precedes 'sampling' in the dictionary and ought to in the field as well."

Biju Rao reinforces this argument when he notes in his contribution that 'Qualitative fieldwork, unlike the quantitative survey method, allows issues to be probed in the field the moment they are observed. This permits 'surprises' to be easily incorporated into the data gathering process," and he goes on to give specific examples in his work where this happened. Caroline Moser's account of work in Colombia shows how the in depth investigation of a qualitative approach was needed to uncover the reality that:

> "although political violence was a serious government priority in Colombia, it was not the major concern of the urban poor, for whom economic and social violence were more important … data such as this highlights the fact that political peace accords, while of critical importance, on their own will not reduce the high levels of violence, with its associated impact on both vulnerability and poverty."

The above accounts seem to confirm to some extent the argument of Carvalho and White, quoted in Kanbur's contribution, that the strengths of qualitative approaches lie in "(i) richer definition of poverty, (ii) more insight into causal processes, (iii) more accuracy and depth of information on certain questions."

Thus there really does seem to be a fair amount of agreement on the strengths and weaknesses of being located at different points along the spectrum for the first two of our dimensions – Type of Information (non-numerical to numerical), and Type of Population Coverage (specific to general) – and a recognition that there are tradeoffs involved as we move from one end to the other. There was less agreement, and in fact less engagement, on the other three dimensions. The active-passive spectrum, so important to work in the participatory tradition, was somewhat eclipsed by discussions about the first two dimensions. Thus Martin Ravallion started his discussion with the qualifier that "qualitative methods have objectives that I do not touch on here, such as helping empower those who participate." And even some who were ostensibly in the qualitative tradition described the evolution of their relationships with the population studied in terms of a discovery of new facts rather than actually helping the population studied.

Turning to the inductive-deductive spectrum, those in the quantitative tradition could not, it seemed, articulate the independent strengths of the deductive mode of reasoning, and even when they did it was as an input to some final deductive purpose (e.g., qualitative studies to suggest hypotheses to be then tested by "rigorous" quantitative methodologies). But then neither, it seemed, did those in the qualitative tradition have a well articulated theory of the benefits of deductivism versus inductivism. There was a definite tilt towards the deductive, even among those in the qualitative tradition. Context was important, and contextuality was highlighted as a key feature of information collected, but again this was very often seen as just a better investigating method, as a way of revealing the unique "truth", rather than a way of revealing multiple and possibly conflicting perspectives on reality.

Finally, I do not think the workshop adequately engaged the issue of disciplinary differences in analyzing and understanding the world, with a view to locating their strengths and weaknesses. This may be partly because those in the quantitative tradition

were all economists, while those in the qualitative tradition had a mix of disciplinary training. One consequence of this is that discussions tend to focus on the weaknesses of the economic method (which are well known, and well recognized by the best economists) as opposed to the strengths of other disciplines such as sociology, anthropology or political science.

IV. BEST OF BOTH WORLDS?

In his second contribution, Robert Chambers poses the question of whether it is possible to combine qualitative and quantitative to gain from the strengths of each and avoid the weaknesses of either. At the end of his journey "From Cointegration to Mr. Issacs", Gary Fields concludes:

> "So why is unemployment in South Africa so tragically high? Above market-clearing wages? The labor hassle factor? Quantitative research reveals the importance of the first, qualitative research the importance of the second. Both are true. There can be no question that both types of research are needed."

Biju Rao summarized some of the gains of combining the two approaches by coining the phrase, "it helps to have had tea with a statistical outlier." This sentiment of "complementarity" was much expressed, and also well illustrated in the contributions on "best practice". But the nagging feeling of irreducible tradeoffs, and loss of information and effectiveness in "forced marriages" of approaches, was ever present. Thus David Booth starts off by saying that "This note takes as read that drawing together information about poverty that has been gathered in different ways is a good thing, with benefits on both sides of the qual/quant divide in terms of the richness and robustness of findings and analysis." Yet he ends his note in less sanguine mood: "My concern is that, under pressure to produce bold conclusions and strong policy messages, PPA fieldwork will become more like survey work … The danger is that this will result in a loss of precisely the features for which PRA has been recognized as a robust method, capable of generating valid, reliable and illuminating results".

Let us start by enumerating areas in which at least a small move in "the other direction" was considered to be feasible without altering the very nature of the method, and desirable as well. Starting from the standard quantitative end, it was agreed that there could be enormous benefit to "expanding the traditional types of questions asked in existing sample survey data used for poverty measurement, drawing on more subjective questions and participatory methods favored in QUAL work, but imposing a structure, such as rigorous sampling methods and pre-determined qualitative scales." (Martin Ravallion). At the same time, Robert Chambers comes from the other direction to say: "Two common assumptions are wrong: that participatory means only qualitative; and that quantitative data can only be generated by questionnaire surveys." He quotes many examples of where the participatory study generated numerical information on a large number of individuals.

Another area on which there was agreement was that studies in the qualitative tradition would gain a lot and lose not too much if the selection of population studied could be put into a sampling frame, preferably a sampling frame for a standard nationally representative household survey. Notice that this does not mean that the study has to administer a standard income/expenditure questionnaire. Once the population to be studied has been identified (eg a cluster of villages representative of an area, or a group of households representative in some other way), the power of the participatory techniques can be brought to bear. But the key is that the results can be put in a frame which allows their representativeness to be discussed and assessed. As Norman Uphoff notes in his contribution:

> " Qualitative studies do not need to Ignore or Reject **sampling techniques**. These can give readers some idea of how common or frequent are the characteristics or experiences being reported … This is not to say that no information is valid unless it is accompanied by a detailed sampling procedure and a large enough sample to be able to establish statistical 'significance'. It is a plea for putting qualitative data into enough of a quantitative framework that they can be meaningfully interpreted."

In his contribution to the workshop, Paul Shaffer gives a specific example of how this was done:

> "The Myanmar PPA used a four stage process in an attempt to establish the typicality of village-based results. This entailed: 1) specifying the population characteristics in question to generalize; 2) identifying indicators of those characteristics; 3) selecting broad regions or areas which are predominantly characterized by these characteristics; 4) selecting a number of villages which are typical of these broad areas."

Within the villages so selected, conventional PPA techniques like well being rankings etc could be carried out. The sampling design is clearly important in convincing policy makers and others of the typicality of the results. But it should be noted that there may be practical difficulties in using the sampling frame of a nationally representative survey, for operational reasons to do with the way clusters are constructed in surveys that are only expected to be representative at the national level.

However, Shaffer's contribution also sets out the limits to how much integration can in fact be effected, stemming from the core features of different approaches. For example, in standard income/consumption based quantitative analysis the underlying conception of illbeing is non-fulfillment of preferences as measured by low levels of income or consumption, whereas in the participatory approach it is "multidimensional and generated in dialogue". He then gives the following striking illustration:

> "In Guinea … household survey data suggest that women are not more likely than men to be consumption poor or to suffer greater consumption poverty … Sensitivity analysis using different adult equivalence scales and different poverty

> lines (stochastic dominance tests) affirms this result … PPA data from the village of Kamatiguia however, suggest that women as a group are worse off than men as a group … In group discussions, a substantial majority of men and women maintained that women were 'worse off' than men, and a large majority held that in a second life they would prefer to be born male rather than female … PPA results cannot serve to refute [income/consumption] results because differences relate to different conceptions of ill-being (and not, say, sampling or non-sampling error)."

One instinctive response to this type of outcome is to introduce more standardized questions in the PPA exercise, perhaps even in the form of a standardized questionnaire. But this is precisely the trend that David Booth is uncomfortable with. As he says, "this will imply a corresponding restrictions on exploratory, responsive and reflexive enquiries, which are rightly considered the *forte* of contextual methods."

Thus, focusing on the first three dimensions of difference identified in Section 2, there appears to be agreement on large gains to be had from limited movements from either direction, but also agreement that too large a degree of movement could eradicate what is valuable at each end of the spectrum. In light of this, another way of proceeding is what was referred to by Martin Ravallion as "sequential" as opposed to "simultaneous" mixing of approaches. The purest form of this is for each approach to do its best, untainted by the other, and then to use the results to triangulate and to inform the next stage of design of each, rather than forcing a combination which might not be appropriate. In her contribution Patti Petesch notes how "the quantitative poverty analysis available in Bank country assistance strategies (CAS) and poverty assessments strongly reinforce and add credibility to poor people's reports about the extent of and trends in poverty where they live." Paul Shaffer, on the other hand, points out potential inconsistencies in his work on Guinea, but this can be the start of a discussion rather than the end.

In his contribution, Kanbur poses the following question: "Accepting the current trend towards combining the two approaches, should an integration be forced at the design stage, or should each technique be allowed 'to do its best', leaving integration for a later stage? In light of the discussions at the workshop the answer seems to be "Both". There is substantial support for small movements from either side "in the other direction". But there are grave concerns about large movements, ending up with an undifferentiated single instrument or approach. In this case, however, it is the responsibility of analysts to reach out and understand the other ends of the spectrum and to learn from them. This is particularly difficult because of disciplinary divides. According to Martin Ravallion:

> "The main barriers to mixing Qual and Quan methods appear to lie in the resistance of practitioners and reviewers to stepping outside the traditional boundaries of practice. Economists have traditionally eschewed subjective questions; oddly, while economists generally think that people are the best judges of their own welfare, they resist asking people how they feel. Psychologists have often obtained individual data on subjective welfare by carefully designed questions in survey instruments or experimental settings that are poorly designed

> for other purposes. And anthropologists and non-quantitative sociologists have often turned their backs on any sort of 'survey'."

Getting a little of the best of both worlds seems relatively easy. Getting more than this, it seems, may be quite a bit more difficult.

V. ETHICS AND THE POLICY PROCESS

In general, those in the quantitative tradition seem less concerned with ethical issues of data collection and analysis. Perhaps because of their close interactions with the populations they study, those in the qualitative tradition are much more aware of these issues, and worry more about them, largely in self-critical mode. For example, Rosemary McGee argues as follows, in response to the question from Kanbur, "Will combining qualitative techniques with quantitative techniques make the former more 'extractive'?:

> "Qualitative approaches are not the same as participatory approaches. In any case, participatory approaches do not necessarily empower, and are not by definition non-extractive. Quantitative approaches are not necessarily, or uniformly, extractive … More reflection is needed by all on what is in it for the respondents (whether 'it' is a questionnaire or an in-depth ethnographic case-study) … Self-critical awareness and context are central to the question of what is ethical and what is not; there is no general rule. There should be some non-negotiables: a minimum one which most participatory practitioners agree on but some still fail to honor is the right of participants to have accessible, appropriate feedback on the findings and uses to which participatory research is put. I do not know of any codes of practice for quantitative research; but with increased interest in combining methods a matter which needs to be pushed up the agenda is the question of how to avoid the default mode of adopting the lowest common denominator in ethical terms."

More generally, Alex Wilks raised the issue of Researcher/Community interaction:

> "…to what extent does poverty analysis empower the people that it studies? For me too much research appears to be framed to try and reach bold conclusions about the state of poverty and what should be done about it. It would be better if it was geared towards contributing to public debate, presenting data and results for different audiences to chew over … Highly technical analyses which rely on detailed algebra, complex charts or intimidating jargon are less likely to lead to good discussions with people on the ground, leaving them solely as the passive objects of study and of high-level speculation about their situation."

Wilks's comments raise the question of who exactly the audience is for poverty research, and this was discussed a little at the workshop. In the qualitative tradition the feeling is strongest that research should be directly beneficial to the poor, and should empower them to push policy makers in one direction or another. While the notion of helping the poor indirectly through influencing policy makers directly is also present in the

qualitative tradition, it is much more strongly present in the quantitative tradition. For each audience, a different type of language is needed. Policy makers, for example, are equally put off by highly technical and abstract analysis, but at the same time the language which appeals to them is not the language which might communicate to the poor themselves. It can be argued that a well crafted policy memorandum could, by convincing policy makers to alter a policy, have a major and immediate impact on the lives of the poor.[4] Similarly, a well crafted technical argument which changes the shape of elite academic thinking could in turn affect discourse and hence policy. Those in the quantitative tradition are most comfortable with this sort of rationale. But, at the same time, it is also recognized, by both traditions, that a compelling story is often worth a thousand regressions in convincing policy makers. And getting policy makers to actually go to the field and meet and spend time interacting with poor people is argued by Rosemary McGee to be an effective pro-poor strategy.

Whatever the mix of qualitative and quantitative that is "ideal" for influencing policy makers, it was strongly felt by those in the qualitative tradition that "when brought together, the two rarely have similar status." The resource devoted to information collection and analyses in the qualitative tradition are far less than for analyses based on conventional household surveys. One suggestion was to institutionalize the collection of information in the qualitative tradition by including it in the responsibility of statistical services, and to augment the resources of these services appropriately. In similar vein, there is need to discuss further how to institutionalize the closer interaction between these two traditions in donor agencies. At the moment they seem to be in separate channels in many agencies. A practical step, in the spirit of "small movements in the opposite direction" would be for individual units to increase their recruitment of analysts in the "other" tradition. But, as discussed earlier, there are limits to how far this can and should go. There seems to be no substitute for holding the specific country strategy papers of each agency, and the strategy documents of the countries themselves, accountable for making sure that they are indeed making the best of both worlds.

VI. CONCLUSION: THE WAY FORWARD

Jesko Hentschel notes: "The desirability and usefulness to combine qualitative and quantitative methods to analyze social realities is pretty much accepted in the literature today; voices of segregation – still quite powerful in the 1980s – have subsided notably." This workshop confirms the Hentschel proposition and, hopefully, goes some way to cementing the new relationship.

But questions remain, not least on how the complementarities are to be reaped and trade offs minimized. We have probably got as far as we can at this level of generality – in characterizing the key features of the two traditions, in enumerating strengths and weaknesses, and in beginning a discussion of the benefits and the pitfalls of integration and how best to attempt it. It seems to me that the next step in furthering understanding

[4] In correspondence, Alex Wilks comments that given diverse conceptions of the nature of truth and of the nature of power one should not be misled by notions of "speaking truth to power," and that there may be many long term disadvantages of experts presuming to do this.

is to get much more specific. One way of doing this, and building on the excellent experience in this workshop with discussions around "best practices", is for a group of interested qualitative and quantitative analysts to dissect some case studies in detail, to learn specific lessons on process, design, framework, output and policy impact. For example, the group could initially look at four case studies – two which start from the quantitative end and move towards the qualitative end, and two which go the other way. The group could then look at four more case studies, this time of "sequential" mixing, where the two traditions do their best within their own frameworks, but try to learn and adapt from the lessons of the other. Such an exercise would be a worthy follower to this first Qual-Quant, or "Q-Squared", workshop.

QUALITATIVE AND QUANTITATIVE POVERTY APPRAISAL: THE STATE OF PLAY AND SOME QUESTIONS

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I. INTRODUCTION

Over the past decade and a half, there has been increasing use of qualitative methods in poverty appraisal. A leading illustration of this is the extent to which The World Bank's Poverty Assessments have as a component a Participatory Poverty Assessment. However, with the growing prominence of qualitative techniques, questions have inevitably arisen about their relationship to quantitative techniques, primarily based on nationally representative household sample surveys. The often invoked complementarities between the two approaches have not managed to overcome or hide the tensions between them.

Clearly, both sets of approaches have a considerable contribution to make in furthering our understanding of poverty, and in helping to formulate poverty reduction strategies, policies and interventions. Each approach may capture and reflect reality in a way that might bridge the gap on many disagreements on poverty reduction strategies (see Kanbur, 2000). But progress requires a better articulation of the benefits and costs of each approach, and how each could be further strengthened by utilizing the results and insights of the other. The object of this workshop is to bring together leading practitioners to discuss these costs and benefits, and to propose elements of a road map of how the two approaches can best be combined. This note briefly reviews the state of play, and poses some possible questions for discussion.

A number of very interesting recent papers have taken up the "Qual-Quant" issue. Examples include Carvalho and White (1997), Booth et. al (1998), and McGee (2000). As these contributions show, some consensus is developing on the relative strengths and weaknesses of the different approaches. In what follows the discussion in Carvalho and White (1998) is used as a basis for posing further questions.

II. "QUALITATIVE" AND "QUANTITATIVE"

The terms "qualitative" and "quantitative" are sometimes apt to cause confusion. Some who use household survey techniques bristle at the suggestion that "qualitative" variables cannot be captured using these techniques, and point to the wealth of quantitative analysis of information on perceptional variables gathered from some "standard" household surveys. Others who use participatory techniques show that some quantification can always be done – it is just a different type of quantification. Carvalho and White (1997) define terms as follows:

"The quantitative approach to poverty measurement and analysis is defined here as one that typically uses random sample surveys and structured interviews to collect data – mainly, quantifiable data – and analyzes it using statistical techniques. By contrast, the qualitative approach is defined as one that typically uses purposive sampling and semi-structured or interactive interviews to collect data – mainly, data relating to people's judgments, attitudes, preferences, priorities, and/or perceptions about a subject – and analyzes it through sociological or anthropological research techniques."

Booth, et. al. (1998) prefer to talk of methods which are "contextual" or "non-contextual", meaning by the former methods "that attempt to capture a social phenomenon within its social, economic and cultural context," while "the sampling, the interview schedule, the training of enumerators and other aspects of best practice survey technique are designed precisely to collect information that is untainted by the particularities of the context in which it is collected."

Sticking to the qualitative and quantitative terminology, Carvalho and White (1997) provide the following listing of the strengths and weaknesses of each:

A. Quantitative

Strengths: (i) makes aggregation possible, (ii) provides results whose reliability is measurable, (iii) allows simulation of different policy options. Weaknesses: (i) sampling and non-sampling errors, (ii) misses what is not easily quantifiable, (iii) fails to capture intra-household issues.

B. Qualitative

Strengths: (i) richer definition of poverty, (ii) more insight into causal processes, (iii) more accuracy and depth of information on certain questions. Weaknesses: (i) lack of generalizability, (ii) difficulties in verifying information.

Carvalho and White (1997) and McGee (2000) use the above to characterize quantitative approaches as having **breadth**, and qualitative approaches as having **depth**. The key is to combine the breadth of one and the depth of the other.

III. AN ANALOGY

Before discussing ways of combining qualitative and quantitative techniques, an analogy might help. In many political campaigns, candidates and commentators make use of opinion polls and focus groups. Opinion polls are analogous to household surveys in poverty analysis. They are designed to be representative, and simple standardized questions are asked repeatedly over time to get a sense of the trends in political support. Focus groups are small, and chosen purposively to reflect particular socio-economic and demographic groups. There is some structure to a focus group discussion, but typically it is kept to a minimum, to allow feelings and characterizations to emerge from the participants themselves. Smart political candidates use both, and triangulate between

them. Sometimes, an issue which emerges from a focus group (like a candidate's "likeability" or "trustworthiness") is then tested more broadly with polls. At other times, a surprising or sudden change in poll trends (fall off in support from a particular group) is made the subject of focus group analysis. (For a look into the world of political polling, see www.zogby.com). This interplay of quantitative and qualitative data is seen not only in the politics, but in the commercial arena as well. Combining qualitative techniques based on focus groups with more standard market surveys is now standard operating procedure in the business world (see, for example, http://www.yankelovich.com/ as one example among literally hundreds of companies that specialize in qualitative techniques in marketing).

IV. QUAL-QUANT

Carvalho and White (1997) discuss three ways of combining the best of qualitative and quantitative approaches:

> "* **integrating** the quantitative and qualitative methodologies;
> * **examining, explaining, confirming, refuting, and/or enriching** information from one approach with that from the other; and
> * **merging the findings** from the two approaches into one set of policy recommendations."

Under integration are such interactions as using quantitative data to focus in on particular sub-groups of interest for qualitative study, and the use of qualitative work to guide the design of the quantitative survey questionnaire. Under examining etc. are a range of examples, including Jodha's (1988) famous example of a case where quantitative indicators showed a worsening but more qualitative indicators showed an improvement, or examples from Togo and Cameroon, where "the qualitative work revealed a food security problem more serious than one would expect on the basis of the available quantitative data." Under merging a number of examples are given where policy specifics were able to be better designed because survey based and participatory research complemented each other.

McGee's (2000) recent evaluation of qualitative and quantitative assessments of poverty in Uganda illustrates these principles for a particular country, focusing on how the Uganda National Household Survey (UNHS) and the Participatory Poverty Assessment (PPA), can be better combined. Among the recommendations are:

- Using PPA to further examine downturn in some welfare indicators between 1995/6 and 1996/7 as derived from the UNHS.
- Using PPA insights to refine UNHS questionnaire.
- Given the findings of the PPA, include questions on risk and vulnerability in the UNHS.
- Matching sample design for PPA and UNHS – with some households in common.
- Increasing standardization of poverty trends assessment in PPA.

- Attempt to repeat PPA for a second round at some first round sites.

V. SOME QUESTIONS

From the above, it seems as though what has happened in market research and political polling is set to happen in poverty appraisal as well. Carvalho and White (1997) reviewed 11 World Bank Poverty Assessments. They note that all 11 incorporated at least two of their ways of combining quantitative and qualitative approaches. From McGee's (2000) account, the process has started in Uganda and well set to advance further. This state of play leads to several questions, some ordinary, some fundamental, and some procedural. Workshop participants might consider these questions in developing their comments.

***Ordinary*:**

1. Does the definition of "qualitative" and "quantitative" above do justice to the differences between the approaches?
2. Is the characterization given above of the technical strengths and weaknesses of the two approaches adequate?

***Fundamental*:**

3. Are the differences just to do with data collection methodologies? For example, McGee (2000) highlights the fact that increasing alcohol expenditure (all else held constant) would be rated positively in current quantitative assessments, but probably negatively in qualitative assessments given the effects of (usually, male) alcoholism on (male and, especially, female) welfare.
4. Will combining qualitative techniques with quantitative techniques in the way suggested above simply make the former more "extractive"? Is a key aspect of qualitative techniques a philosophy of "empowering" the poor and bringing them into the decision making process, and will the current trends towards combination threaten this feature? How can this feature be spread more widely than current participatory methods allow?
5. What are the implications of Qual-Quant for International Development Targetry, which is by definition quantitative in nature?

***Procedural*:**

6. Accepting desirability of the current trend towards combining the two approaches, should an integration be forced at the design stage, or should each technique be allowed to "do its best", leaving the integration for a later stage? What are the losses from forcing household surveys to be more qualitative and participatory assessments to be more quantitative?
7. What is the best institutional framework for ensuring the close interaction between the two approaches? Can we learn from the use of qualitative and quantitative techniques in the commercial arena?

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QUALITATIVE APPROACHES: SELF CRITICISM AND WHAT CAN BE GAINED FROM QUANTITATIVE APPROACHES

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My brief is the strengths of quantitative approaches and self-criticism of qualitative approaches in poverty appraisal. I hope this will be balanced by contrasting points from those on the quantitative side.

I. GAINS FROM QUANTITATIVE APPROACHES

Quantitative approaches usually means research using standard sampling techniques, questionnaires and statistical analysis. The well known potential benefits include:

- time series comparisons to identify trends in whatever dimensions are measured,
- cross-section comparisons between different individuals, households, groups and communities, and across regions, countries and continents
- correlations which identify associations which raise questions of causality and covariant changes
- estimates of prevalences and distributions within populations and areas
- triangulation and linkages with qualitative data (Booth to this workshop)
- the credibility of numbers in influencing policy-makers
- the utility to policy-makers of being able to put numbers on trends and other comparisons

II. SELF-CRITICISM OF QUALITATIVE APPROACHES

Benefits and gains from qualitative data from poverty appraisals and research have been documented in comparative analyses drawing on a range of experience (e.g. Norton and Stephens 1995; Booth et al. 1998; Brocklesby and Holland 1998; Holland with Blackburn 1998; Robb 1999; Brock 2000). There is a literature on appropriate forms of rigour with qualitative approaches and data (e.g. Lincoln and Guba 1985). Self-critical epistemological awareness (Brock and McGee in preparation) is at the core of rigour. This section lists some of the main things that can go wrong. It points to weaknesses, dilemmas, dangers, tensions and trade-offs. They apply also to much other research. They fall into two main groups: methodological and analytical; and ethical.

A. Methodological

Design and Fieldwork

- *Selection and training of facilitators*. Not everyone has it in them to be a good participatory facilitator. Selection is liable to be constrained. Consultants pop up all over the place to claim to be "PRA trainers" who are not competent, and who do not concern themselves with behaviour and attitudes. Training is too short. Training is not experiential in the field.
- *Selection of sites*. Only a few sites are selected, limiting representativeness. Purposive selection brings in biases which may not be recognised. One is the choice of atypical communities where an NGO is working or there is a special project.
- *Unrepresentative participation*. The views and interests of a dominant group in a community, or of dominant individuals in focus groups, are over represented. Marginalised groups are left out, especially the "bottom poor".
- *Agenda framing.* The realities expressed are over-influenced by the agenda of the facilitators.

Analysis

However "good" the data, analysis is vulnerable to distortion, inaccuracy and unrepresentativeness, especially with

- *Large amounts of data.*
- *Data which are not comparable or difficult to compare*
- *Deadlines, and lack of time and resources*
- *Imprecise analysis* in which items are sorted into categories with unclear boundaries, where emergent boundaries change during analysis, or where different analysts make different decisions
- *Analysts with strong preconceptions and mental templates* (diagrammatic models into which they habitually fit complex realities)
- *Pressure and incentives for early and striking policy messages*

These can then lead to:

- incomplete coverage of the data
- distortions through misleading or inconsistent classification
- falling back on conventional or personal categories of classification instead of allowing the data to generate emergent categories
- selective searching for data which fit the analyst's preconceptions
- circularities, where the method (through language, choice of site, process etc) itself contributes to an emergent category
- successive simplification and editing, excluding qualifications and exceptions

- unsubstantiated or not fully substantiated assertions based on the authority of the process rather than the actual data
- omission of qualifications, caveats and limitations of the data and statements
- oversimplification of complex realities
- overattention to striking quotations
- overgeneralising

B. Ethical

a. *With Participants*

- taking the time of poor people, especially those suffering poverty of time and energy, without recompense. This is more often with females than males
- raising expectations of benefits which are not realised
- leaving participants exposed to later penalties (violence, persecution, etc.)

b. W*ith Policy Influence*

The ethics of the soundbite when evidence is selected and simplified to make assertions which variously

- fit the analyst's preconceptions
- are not be based on adequately representative or adequately analysed qualitative data
- but which are striking and have potential for policy influence.

Resolving the conflict here between academic and policy-oriented values is *not* simple.

C. Tensions and Trade-offs

Running through these issues are tensions with trade-offs. Some of the more obvious are:

- Depth in fewer places versus breadth in more
- Scale and representativeness versus analysability
- Standardisation, narrowness and analysability versus open-endedness, depth, and difficulty in analysis
- Volume of data versus willingness to suspend habitual categories and preset ideas [under pressure the analyst is driven to use familiar embedded categories]
- Delegation of data analysis versus personal judgement and "ahhas!"
- Resources available for follow-up with fewer communities or groups versus more communities or groups with fewer or no resources for follow-up

Fortunately, experience on how to minimise shortcomings and improve quality is accumulating fast.

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THE BEST OF BOTH WORLDS?

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Two common assumptions are wrong: that participatory means only qualitative; and that quantitative data can only be generated by questionnaire surveys. Since the early 1990s examples have multiplied which show these to be false. Three types stand out:

1. *Large-scale Surveys*. Participatory visual analysis with PRA-related methods was used in over 130 villages in Nepal by Action Aid in 1992 in a utilisation survey, generating tables with a population which summed to 35,414. Pile sorting and other participatory methods were used in 20 districts in Malawi, Zambia and Zimbabwe by SCF for SADC on how poor farmers coped with the 1992 drought (Eldridge refs).
2. *Aggregating from Focus Groups.* Focus group responses have been aggregated (e.g. Narayan et al., 2000, 124-132; Moser and McIlwaine 2001; Cornwell et. al, 2001). This has also occurred in some Participatory Poverty Assessments (e.g. Bangladesh (UNDP 1996), Kenya and Tanzania)
3. *Individual or Group Analyses which Count, Estimate and/or Value.* Examples are social and census maps, historical matrices (e.g. Freudenberger 1995; PRAXIS 2001, 98 and 102), seasonal food calendars (e.g. Mukherjee and Jena 2001, 51), and matrix scoring of crop varieties (e.g. Drinkwater 1993; Manoharan et al., 1993).

These are all participatory, putting them in the NE quadrant of the diagram. Most of them have used visuals and/or tangibles to generate the numbers. The NE quadrant may have been relatively neglected partly because questionnaires are professionally and institutionally embedded and because much of the innovation has been by younger professionals in the NGO sector in the South who neither have nor aspire to academic clout.

The questions are whether, to what extent, and in what contexts, we can have the best of both worlds with these approaches and methods: the numbers needed for representativeness and credibility, and the insights needed for relevance and realism. Recent work suggests that we can (e.g. Eldridge references; Moser and McIlwaine 2001; Cornwell et. al, 2001). Scope and credibility are enhanced by the coverage and accuracy of participatory methods with group-visual synergy. With this, representation is cumulative and cross-checked through the overlapping knowledge of participants (Chambers 1997, 160). Sensitive information difficult to access through questionnaires may be shared, as with the castewise breakdown of number of families with addiction to alcohol (PRAXIS 2001, 33).
Critical questions need to be asked about methodology, epistemology and ethics; about whether and in what conditions there are trade-offs rather than the best of both worlds; about comparative costs and benefits; about feasibility in terms of the training, behaviour

and attitudes of facilitators. Evidence to help us answer these questions is accumulating. A workshop is proposed to review the evidence and explore ways forward. Is this a good idea? What other questions should it address?

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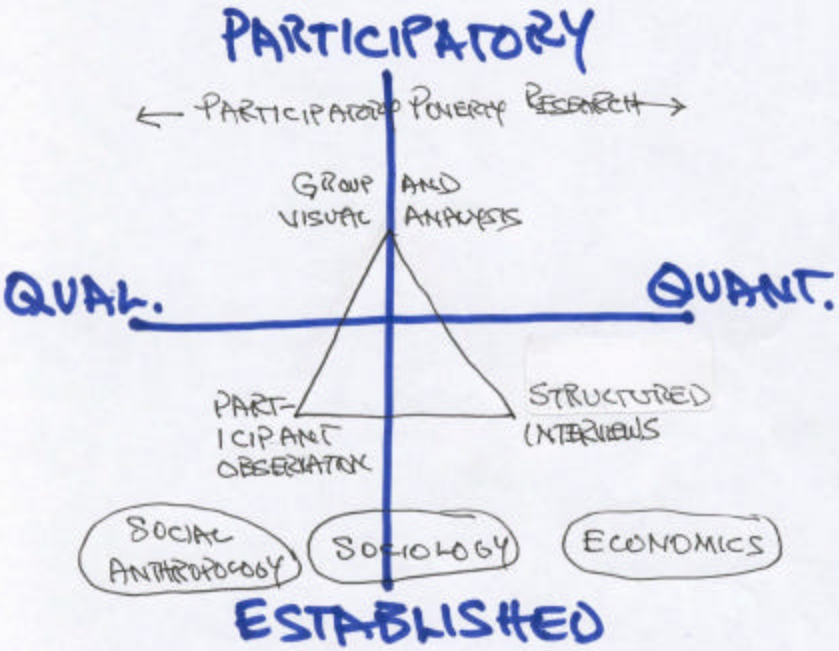
DIMENSIONS OF INTERACTION AND OUTCOME



= PRIMARY MODE OF INTERACTION

NEW
OLD
= CHARACTERISTICS OF APPROACH AND METHODS

QUAL ——— QUANT = DIMENSIONS OF OUTCOMES

= PRINCIPAL LOCATION OF PROFESSION ON QUAL-QUANT AXIS

[To accompany Wconnell6.doc]

RAZ
18 April 07

SELF-CRITICISM AND OBSERVATIONS ON THE WAY TO FINISHING *VOICES OF THE POOR: FROM MANY LANDS*

Patti Petesch
Consultant to The World Bank

I would like to make two confessions and one observation based on my work on Voices of the Poor (VOP), especially drawing on my current experiences with preparing volume three of the study, *From Many Lands*. This volume consists of a set of country case studies based on the 1999 fieldwork in poor urban and rural communities in 23 countries.

In my noble effort to be true to poor people's "voices" gathered for VOP, I initially disregarded other poverty studies. In fact these other studies, both quantitative and qualitative, are adding important depth to *From Many Lands*. The quantitative poverty analysis available in Bank country assistance strategies (CAS) and poverty assessments strongly reinforce and add credibility to poor people's reports about the extent of and trends in poverty where they live.

For example, the Ghana chapter in *From Many Lands* opens in a very destitute farming village called Tabe Ere. It is in the northern Savannah region, and people are dying of hunger there. I became alarmed that I'd have to find another opening, however, when I read the Ghana CAS and the very well done poverty report by the government's statistical agency. GDP averaged 4.2 percent between 1990 and 1998, and climbed to 5.5 in 1999; and poverty fell from 52 percent of the population in 1991-92 to just under 40 percent in 1998-99. For many other countries in our study, such positive trends cannot be found.

A closer examination of the quantitative reports, however, indicates that many poor people are being left behind, and poverty remains quite severe in most of the country. The declines in poverty were heavily concentrated in Accra and the Forest region. Elsewhere in Ghana, poverty rates have fallen only very modestly and are the highest in rural Savannah (70 percent), which is where Tabe Ere sits. Here is a case where geographic poverty data really provides a quite useful context – and I didn't have to change the opening about Nyooura, a poor farmer in Tabe Ere who has two wives and eight children, and was once doing fine until his cattle were stolen, and now he worries intensely about feeding his family.

At the other extreme, we have relatively wealthy Argentina in our sample, and we begin the chapter in La Matanza, where there is extensive unemployment (due to the closure of numerous factories) and pervasive problems of crime. Here again, weaving in some of the quantitative data has been helpful in understanding how such extensive social problems can exist in such a rich country. Argentina enjoys the highest per capita income in Latin America and some of the region's highest social sector spending. However, the

overall data on Argentine incomes and social spending hides big equity gaps and big distortions in who benefits from public spending.

Argentina suffered a long period of hyperinflation and recession, which lasted well after the nation's transition to democracy in 1983. In these years, poverty in the larger Buenos Aires area skyrocketed from 8 percent of the population in 1980 to 41 percent at the end of the decade. In the 1990s, the Argentine economy underwent a massive transformation – but poverty still remains very high at 29 percent in 1998. Problems of unemployment, crime, domestic abuse, and unresponsive institutions are described with as much urgency by poor people in "rich" Argentina as anywhere else in the study. And the benefit incidence of public spending also help to reinforce poor people's reports that they have very few places to turn for help. For instance, poverty oriented safety net programs are abundant in Argentina, but 75 percent of the poor do not receive any public assistance.

If statistics such as these help the voices of poor Argentines or poor Ghanains to be taken more seriously, then by all means I am eager to include them!

We haven't found any shortcuts in drafting the country case studies in *From Many Lands.* Rigorous analysis of qualitative data often requires an iterative drafting process of constantly returning to the data to identify and then cross-check key messages and the most helpful supporting evidence. Our experience with using local researchers and Ph.D. ghost writers has been very mixed. In addition, reporting on VOP findings within the Bank has also sometimes been uneven, with some findings overly simplified. Moving from the very large qualitative data sets that are generated in the field to a synthetic document requires extensive training in qualitative data analysis and report writing.

The qual-quant divide at the bank is shrinking, but it is still large. Strategies to reduce poverty need to be more grounded in poor people's realities. Qualitative approaches are gaining a wider presence at the Bank. For instance, you can read about the VOP findings in most of the Bank's strategy documents for countries where the study took place. And a few countries like Vietnam are blazing new trails in integrating qualitative work directly into national policymaking processes. Still, the divide between qualitative and quantitative work is often very large. In many countries, qualitative monitoring and evaluation processes are largely missing from efforts to measure development outcomes and impacts (e.g. in the context of PRSP, the Bank's new flagship debt relief program for heavily indebted countries). In addition, while more and more Bank activities are recognizing the potential of participatory models, action on the ground is often very thin as a recent evaluation of the Bank's participation work reveals. Poor people can be valuable resources for understanding project or policy impacts, or in programs that require directing services to special groups. They really do know who is poor and why; and they also have some pretty good ideas about what would help them become less poor.

Inclosing, I'd like to make the point that many of today'strategies to reduce poverty seem quite removed from poor people's realities, and this is the central challenge both for qualitative and quantitative work. I'll return to Argentina to make my point. Obviously,

liberalization has not been good for poor men and women in La Matazna, no matter what data sources you look at. I also want to suggest that poor people's wellbeing seems quite distant from many of today's macro debates about the policies and institutions needed for pro-poor growth. Forty-five percent of Argentina's labor force is in the informal economy, and you can read the chapter to get a sense of how precarious their jobs are and the wider impacts this has on people living in La Matanza. Today's leading strategies for poverty reduction in Argentina, however, largely overlook this basic reality about poor people's livelihoods and lack of security. More inclusive poverty research and policymaking processes are needed to change this.

BRIDGING QUANTITATIVE-QUALITATIVE DIFFERENCES IN POVERTY APPRAISAL: SELF-CRITICAL THOUGHTS ON QUALITATIVE APPROACHES

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Cornell University

> Our discussion will be adequate if it has as much clearness as the subject-matter admits of, for precision is not to be sought for alike in all discussions… It is the mark of an educated man to look for precision in each class of things just so far as the nature of the subject admits.
>
> Aristotle, *The Ethics*

This exercise of joint "stock-taking" for quantitative and qualitative methods of poverty assessment is welcome. I am pleased to be categorized as using and favoring "qualitative approaches" if this puts me in the very good company of colleagues like Robert Chambers and Deepa Narayan, although I have never considered myself a partisan or advocate for either qualitative or quantitative approaches to the exclusion of the other. I advise my students to use both kinds of methods, being usually more critical than laudatory of both approaches because each has its own blind spots and ambiguities. Decimal points are no guarantee of precision, any more than words give us assurance of validity.

In this workshop, we all have assigned roles, so in mine I will offer some self-criticisms of qualitative approaches to poverty appraisal. To the best of my knowledge, such approaches have been used most extensively and most systematically in the two volumes entitled *Voices of the Poor* produced for The World Bank under the leadership of Deepa Narayan with guidance from Robert Chambers among others. The research on which these volumes were based was undertaken to provide some substantial qualitative underpinnings for the *2000 World Development Report* which assessed poverty on a global scale.

Since I was a reviewer of these volumes in their final drafts, I do not want to dissociate myself from them. Indeed, I was pleased to be part of this effort to paint a picture of the "meaning" of poverty in human terms. I hope that these volumes communicated to persons who have heretofore considered poverty essentially in abstract, faceless quantitative terms what it feels like to be among "the poor." However, as I read the drafts, as a social scientist who has spent more time working with the poor than in analyzing data characterizing them, I felt some unease, which I will share with this group.

I start with three concerns: (1) How can we know how **representative** are the persons who speak as members of "the set of poor people"? (2) How can the different aspects of being poor be **summarized**, to gain some idea of the severity or magnitude of personal poverty? (3) And how can we know **trends** among the poor, whether their numbers are increasing or decreasing, and whether their poverty is getting "deeper" or more bearable over time?

[Note: I will leave aside the question of how we can know **how many** poor there are. Assessing gross or net numbers of the poor is clearly a quantitative matter, and one that lends itself to endless and not very satisfying debates that are remote from the experience and reality of the poor. It anyway depends upon qualitative definitions and criteria.]

These three questions point out **intersections** where qualitative investigations of poverty run head-on into the need for at least some quantification. No matter how good are the qualitative characterizations of the conditions of life for certain real persons who are considered to be poor, these three questions must be addressed by any researcher or evaluator who is concerned with the well-being and future of poor persons. Fortunately, there are some methods available that do not compromise or negate the value of good qualitative work.

(1) Qualitative Studies do not need to Ignore or Reject **sampling techniques**. These can give readers some idea of how common or frequent are the characteristics or experiences being reported. For an individual to report that he or she has been victimized by the police, for example, being defenseless because of his or her poverty, does not tell us whether this is a universal experience, a frequent occurrence, an uncommon thing, or a freak and completely idiosyncratic event. This is not to say that no information is valid unless it is accompanied by a detailed sampling procedure and a large enough sample to be able to establish statistical "significance." It is a plea for putting qualitative data into enough of a quantitative framework that they can be meaningfully interpreted.

This word "significance" is one of the greatest misfortunes to befall us, but that is another discussion. There are good reasons to chafe under the tyranny that statistical "hawks" have foisted upon the social sciences. However, sampling can be used more modestly, helping readers make informative inferences about the representativeness of information from individual or a few cases, and this seems to me of great importance and urgency.

(2) Recognizing that Poverty is a Multi-dimensional Phenomenon begs for some efforts to *knit the pieces of poverty together*, if it is to be spoken of and acted upon as a Single Problem. For this, researchers can construct **indicators** from a variety of measures, thereby combining different facets of the phenomenon into some summary measure. Such efforts will not be free of argument, but they can be very informative, and the debates over their construction are themselves usually worthwhile.

The alternative is to leave the reality of poverty *fragmented*, which gives short shrift to the question of poverty's sources and causes, which are usually illuminated by examining factors that coincide or correlate. Efforts to compile measures of poverty that are

multifaceted move investigations beyond description and more clearly onto the terrain of explanation. The ingredients for indicators need not be (all) quantified information. There are respectable techniques for converting qualitative data into quantitative forms that permit some reasonable combination and summation. This process is worth engaging in for the insights that it can give into the etiology and distribution of poverty.

(3) Further, most Researchers are Interested not so much in Poverty Synchronically as in what is happening *diachronically***.** Is poverty increasing or decreasing? For which categories of the population? To address these questions, one needs – and can fairly easily have – **time series data**, whether these are of quantitative or qualitative origin. Characterizing or evaluating poverty at one point in time may be interesting, but it seems more prying than principled. Even *synchronically*, the analysis and evaluation of poverty is more meaningful when it is **comparative**, using data to contrast or find similarities between different groups within a population or in comparable populations. Leaving poverty analysis in purely qualitative, descriptive terms may *enrich* our understanding of poverty, but it does not *advance* this understanding very much.

The debate between partisans of quantitative vs. qualitative analysis has always struck me as poorly conceived. My own disposition has been to argue against whichever was being the more strident and hegemonic at the time. I view this debate in the same light as the one that anthropologists have engaged in, as to whether *emic* or *etic* views of socio-cultural reality are more valid. The *emic* view proposes that reality is best understood in terms of the subjective meanings that people attach to their roles, relationships, material possessions, and the like. The *etic* view holds that reality is best viewed through the "optics" of concepts with cross-cultural relevance, like marriage or property or gender (Headland et al. 1990).

As a social scientist trained in comparative analysis I have generally favored *etic* approaches, but the more I become acquainted with field situations, the more I appreciate that marriage, property, gender and most other constructs that we use in social science are not the same across cultures or indeed often within cultures. Most anthropologists who have thought deeply about this subject find merit in both "takes" on reality, even if they may prefer to work more in one mode or another. There is no ontological basis for proponents of either approach to deny the validity of the other. Indeed, if one presses the matter, it is not clear that the **etic** view is any less subjective than the *emic* view.

The debate between qualitative and quantitative analysis often focuses on the whether the greater "precision" and "objectivity" claimed for the latter gives sufficient justification for preferring it over, even to the exclusion of, the former. I look at that debate and doubt whether the claims in favor of quantitative analysis can indeed be sustained. Numbers are often quite imprecise, and even very subjective. If you take one estimate (e.g., cultivated land area) and divide it by another approximate figure (e.g., number of rural households), you can come up with a very exact number of cultivated acres per household, even with three or more decimal places. But this number is a fiction, at least in its precise form. It is no less an approximation than is any semantic description, and indeed some descriptions are more accurate in a literal sense than are numbers. Remember that the gross national

product (GNP) – which today is regarded as something quite real and even exactly measurable, so that we can it grew by 0.4% last quarter in the U.S. rather than by 0.6% – is a **fabrication** based on multiple assumptions and conventions. That these are now very familiar and widely accepted does not diminish the dubious ontological status of GNP.

Measures or indicators of poverty have been, are being and will continue to be constructed with as much validity as those of GNP, labor force participation, unemployment rates, or other efforts to encapsulate and evaluate economic performance and dynamics. We should bear in mind that these are more informative in a comparative sense – synchronically across comparable populations if the measures are reasonably uniformly applied, and more usefully diachronically, comparing changes (or no change) over time.

Recognizing that any measure or indicator is no better than the constructs on which it is based and on the operationalization of concepts, we would do well to accept and integrate data that are more qualitative in nature rather than limit our quantitative analysis just to facets of reality that are easily or reliably measured. With disciplined efforts and collegial criticism and revision, we should be able to develop measures and indicators of poverty that are **richer** than those most widely used now. These will incorporate qualitative information that is converted into quantitative form, to make statistical pictures of poverty more multifaceted. An excellent example of this is N. S. Jodha's effort some years ago (1988) to resolve the conflict between his quantitative and qualitative measures of poverty in certain Indian villages by asking people what things distinguish persons who are poor or not poor in their locality. His results were very powerful and revealing.

The strategy of accepting simple measures of poverty as proxies because they are considered more reliable for being numerical is becoming less and less tenable. Our analytical capabilities for dealing with large amounts and great varieties of data are making it more and more feasible to calculate multiple poverty indexes and to consider a range of measures, rather than settle for just one – recognizing that each will have certain limitations.

These capabilities, however, can be very seductive, becoming a kind of "black hole" that absorbs both data and researchers that come near them. When we evaluate methodologies for poverty assessment, we need always to ask: for *what* purpose, or better, for *whose* purposes, will they be used? For academics for whom precision and elegance bring professional and personal rewards? For bureaucrats or policy-makers who need to make decisions about resource allocation? For the poor themselves, so that they understand their situation better and can act more effectively on their own behalf?

Just listing these three very different "constituencies" for poverty appraisal measures shows how unavoidably relative and subjective is our task. It is useful to look at the deficiencies of both qualitative and quantitative measures, but not to be able to conclude that one is better or more valid than the other. The challenge is how to **combine** the *strengths* of both approaches in ways that offset and minimize the respective *weaknesses*

of each. We live in a *both-and* world where multiplicity invariably trumps polarities, and where creative syntheses are ultimately more valuable than even the best analyses.

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CAN QUALITATIVE METHODS HELP QUANTITATIVE POVERTY MEASUREMENT?

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The World Bank

There are good and bad examples within both "qualitative" and "quantitative" approaches to poverty analysis and easy debating points can be had by selecting the other side's bad examples. While some people allude to deep philosophical differences between QUAL and QUAN, the differences seem to shrink amongst the best examples of each. An influential volume on QUAL research methods has argued that "..the differences between qualitative and quantitative traditions are only stylistic and are methodologically and substantively unimportant. All good research can be understood – indeed, is best understood – to derive from the same underlying logic of inference" (King, Keohane and Verba, 1994).

This note comments on whether and how poverty analysis might be improved using qualitative methods, by which I mean methods that try to draw on participant perceptions of themselves or those around them in a relatively unstructured and open-ended way. QUAL data might come as text, or in the more structured form of categorical data. The methods used vary widely, but are generally subjective and context-specific. There are many examples, one of which is the Participatory Poverty Assessment (PPA) that has become a popular tool at The World Bank.

There is nothing new to the idea of mixing QUAL and QUAN methods. Indeed, the distinction in practice between the two is often fuzzy; much (though certainly not all) of the QUAL data collected in practice is readily recognized by QUAN specialists as qualitative (nominal or ordinal) data, for which there is a large literature on appropriate methods of statistical analysis. However, it is known that there are some important lessons from QUAL methods for doing QUAN better. There has been much recent interest in "mixed methods" in the social and behavioral sciences.[2] QUAN economists often use QUAL methods to good effect in formulating and testing hypotheses as a preliminary step to QUAN specifications and sometimes as a final step too, when QUAN results are compared to perceptions/experiences "on the ground."

The note only comments on how mixing might help in the specific context of poverty analysis. In doing so I will take a somewhat narrow perspective on qualitative poverty

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[2] See, for example, Tashakkori and Teddlie (1998). For discussion of how economics can help mediate cross-disciplinary work see Rao (1997).

appraisal. In particular, qualitative methods have objectives that I do not touch on here, such as helping empower those who participate.[3]

I. PROBLEMS OF POVERTY MEASUREMENT

Some problems are specific to the data one is using, such as prior mistakes in survey design, compliance/response biases and data processing errors. There are also conceptual problems that persist in the best possible conventional data sources. Two such problems stand out. The first is the *identification problem* of how to weight aspects of individual welfare that are not revealed by market behavior. A classic example is the question of how family characteristics (such as size and composition) affect individual welfare at given total household consumption. Another example is the effect of command over non-market goods (including some publicly supplied goods). Other examples include measuring the individual welfare effect of relative deprivation, insecurity, and social exclusion. The second problem is the *referencing problem* of determining the reference level of welfare above which one is deemed not to be poor – the poverty line in welfare space, which must anchor the money-metric poverty line. (For further discussion see Ravallion, 1998). The measures one gets in practice and the policy conclusions drawn have often been found to depend critically on how these various problems were dealt with.

Addressing the identification and referencing problems requires information that is not found in conventional objective socio-economic survey data for representative samples of households. Sample surveys restricted to the standard "objective" data have been found to be of somewhat limited use for measurement and policy. The identification and referencing problems remain to be addressed from other data sources or by often implicit and ad hoc judgements made by analysts, with tenuous links to people's actual perceptions of their own welfare. I suspect – and can provide some evidence (Ravallion and Lokshin, 2001a) – that many of those people who perceive themselves to be "poor" are not in fact classified as such in conventional poverty statistics, and that many of the poorest (say) quintile in terms of their consumption relative to the poverty line would not place themselves on either of (say) the two lowest rungs of a 10-rung subjective welfare ladder. Discrepancies between objective assessments of whether poverty incidence is falling or not and perceptions on the ground have been noted. How one treats such discrepancies remains an open question; for example, one might argue that the perceptions on the ground are simply ill-informed. But even so, it would seem unwise to ignore those perceptions.

II. IS 'QUAL' THE SOLUTION?

While careful use of QUAL methods can help, these methods are not a panacea. All the standard problems of inference in descriptive or causal QUAN studies remain in QUAL studies (King et al., 1994). Yet in practice, it is not uncommon to find strong causal

[3] I also leave aside the important question as to whether there might be a trade-off between the quality of QUAL work and its prescriptive, empowerment, role; such a trade off is known in QUAN analysis when it tries to serve an activist policy agenda.

statements in PPAs, for example, that have unknown validity and are subject to obvious sources of bias. There appears to be ample scope for doing better QUAL work by taking lessons from the QUAN literature, such as past work on experimental and quasi-experimental methods for inferring causal effects.

It has been claimed that QUAL methods are better able to identify the causal processes determining poverty than QUAN methods (Carvalho and White, 1998). This is surprising, given that the notion of "cause" and "effect" is explicitly rejected by the constructivist/ naturalist founders of QUAL, while it is a cornerstone in the positivist roots of QUAN. QUAL proponents in practice are clearly more pragmatic, and often attempt to identify causality. However, it is equally true for QUAN and QUAL that if one makes causal statements that ignore well-established criteria for determining causality then the advance in knowledge made may well be illusory.

In turning to QUAL methods, we should not of course ignore the potential biases. The problems for understanding poverty this way are well known to experienced QUAL researchers, such as the problem of local capture by the non-poor of focal groups organized by well-healed outsiders – precisely the type of problem that rigorous sampling methods avoid. A somewhat deeper concern is that research in psychology indicates that latent personality traits of little obvious welfare relevance influence expressed perceptions of well-being. For example, the fact that a person is inhibited, rebellious or unconfident – all three are known correlates of self-rated welfare (De Neve and Cooper, 1999) – would not normally constitute a case for favorable tax/spending treatment (say) for the purpose of reducing inequality.

III. MIXING 'QUAL' AND 'QUAN'

There are two ways of mixing that might help in doing better poverty analysis: *sequential* and *simultaneous*. The former entails integrating methods sequentially for largely independent samples but within one well-defined social context. A common example is using open-ended questions asked in unstructured surveys of non-random samples as a preliminary step in formulating the questions to be addressed by a structured sample survey. Sequential mixing imposes few restrictions on QUAL methods, but may still require coordination with QUAN sampling designs (e.g., doing focal groups in the same randomly sampled communities).

Simultaneous mixing is probably more controversial. In this context, it entails expanding the traditional types of questions asked in existing sample survey data used for poverty measurement, drawing on the more subjective questions and participatory methods favored in QUAL work, but imposing a structure, such as the rigorous sampling methods and pre-determined qualitative scales. The analysis should clearly take account of the processes that generate the qualitative data, including the biases that can arise from the existence of latent social/psychological factors that are likely to be correlated with the causal (including policy-relevant) variables of interest. This can be difficult. On how it can be done using longitudinal qualitative data on perceived well-being (collected within

a multi-purpose survey) see Ravallion and Lokshin (2001b). But this type of QUAL data is still rare.

There are costs to mixing, which is bound to entail changes in how both QUAL and QUAN work is done. For example, the choice of geographic areas in PPAs is sometimes made on the basis of the local NGO connections of the facilitators. The PPA can then be heavily influenced by the issues of concern to the local NGOs, which also vary according to NGO placement. Integration with QUAN studies will impose constraints on all this, by dictating where the work is done, what topics are covered and how questions are asked. Pilot QUAL studies, in which the agenda is left more open, might help decide some of these issues. Prior analysis and policy dialogue will also help.

There could well be benefits too, as some recent examples have illustrated.[4] QUAN analysis is often hampered by failure to properly adapt the tools used to the specific context; sequential integration with QUAL work can help. Self- and social perceptions can provide important clues about welfare, directly addressing both the identification and referencing problems discussed above. Qualitative perceptions of the adequacy of consumption and services can be used to derive social subjective poverty lines (levels of spending below which people typically say that they are poor).[5] Mixed methods allow assessments of policy impacts and the monitoring of outcomes that span a broader range of welfare dimensions, allowing us to look at how much these different dimensions move together, and how both are linked causally to observed policy changes and other events affecting people lives.

To realize the potential benefits, it is highly desirable to have that data for the same households/communities as the rest of the data one needs for poverty analysis. Living-standards surveys that collect both types of data might throw new light on many aspects of welfare and its causal determinants. However, so far data integration has been weak. For example, The World Bank's LSMS surveys have generally avoided subjective questions in the household and community-level rosters. There are still many unexploited opportunities for mixing. For example, PPAs can be done in the same randomly chosen geographic clusters that are used in (two-stage) sampling for household surveys, thus allowing integration with the community and household level data. The same PPA teams can be used, but they would be constrained to coincide with the timing and placement of the QUAN survey. On top of the gains from mixing, the PPA would then have known representativeness, thus facilitating inferences about the population.

The main barriers to mixing QUAL and QUAN methods appear to lie in the resistance of practitioners and reviewers to stepping outside the traditional boundaries of practice. Economists have traditionally eschewed subjective questions; oddly, while economists generally think that people are the best judges of their own welfare, they resist asking people directly how they feel. Psychologists have often obtained individual data on subjective welfare by carefully designed questions in survey instruments or experimental

[4] See for example Rao (1998), Hentschel (1999), Kozel and Parker (2000), and Rawlings (2000). Tashakkori and Teddlie (1998) summarize other examples.

[5] On how this can be done rigorously see Pradhan and Ravallion (2000).

settings that are poorly designed for other purposes. And anthropologists and non-quantitative sociologists have often turned their backs on any sort of "survey." The challenge remains of how to best provide the information needs for sound and useful poverty analysis, drawing pragmatically from existing tools, and inventing new ones when needed.

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QUALITATIVE AND QUANTITATIVE APPROACHES TO POVERTY ANALYSIS: TWO PICTURES OF THE SAME MOUNTAIN?

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Looking at a mountain from the North or from the East may lead two painters to draw very different pictures. People watching their paintings side by side might believe they represent two different mountains. Yet, they will not be surprised to discover that the name of the mountain written below each frame is the same. What is more, they will even be able to figure out immediately the true shape of the mountain in a 3-D setting. The distinction between qualitative and quantitative approaches to poverty analysis, or the analysis of any other social or economic phenomenon, is of the same nature : two different types of perspectives, the reunion of which is the only way to get some idea of the reality. The only thing is that this reunion, as well as the drawing of each perspective may be more complicated than taking the picture of an object under different angles.

This note illustrates this intimate complementarity between the two approaches considering various definitions of quantitative and qualitative methodologies and various points of view for analysts and policy makers.

To make the argument shorter, and maybe tighter, consider the following general representation of welfare determination at the household level:

$$Q = F(A, IFS; MI, NMI; T, P)$$

The function F( ) represents the way in which a vector of private *endowments*, A, are transformed into a vector of *outcomes*, Q, which will themselves determine the *welfare*, W(Q), of the household. This transformation process itself depends on various sets of parameters: public or community *infrastructure* (IFS), *market institutions* (MI) and associated parameters (e.g. prices), *non-market institutions* (NMI), *the technological constraints* under which households in the community are operating (T) and the *preferences* of the household among various techniques of production and among various outcomes. The vector of outcomes, Q, may include standard economic flows like income or consumption expenditures, possibly disaggregated into various groups – food intake for instance, – and immaterial components like health condition, education of the children, participation to community activity, etc. Likewise, the vector of assets may include material components like land, agricultural equipment, and less material ones like education, know-how, initial health status, or social capital. F( ) is a vector function, or better a feasibility set. During a given period of time, the arguments of the function F( ) may be considered as exogenous. Over a longer period, however, dynamic relationships establish links between Q, A, T and possibly P. Uncertainty may be introduced, too, in

which case Q becomes a set of stochastic variables and their distribution must enter the welfare evaluation, W(Q). The transformation process may also be disaggregated into individuals within the household, in which case some kind of 'sharing rule' must be introduced among the arguments of F( ). In this general framework, poverty may be defined by the inability of individuals or households to reach some targets, Z(Q), defined in terms of the vector Q.

With the preceding representation, the distinction between quantitative and qualitative approaches to poverty analysis may appear in different ways. First, it is sometimes seen as the opposition between the left-hand side, that is a focus on *measurement* of poverty, and the right-hand side, or the *understanding* of the causes of poverty and mitigation strategies. This does not seem a very relevant distinction, though. Many studies both on the quantitative and the qualitative side tend to mix measurement and analytical issues. This may be more frequent on the qualitative side, as many authors think that there is no dichotomy between identifying poverty and understanding its causes. On the quantitative side, however, most studies addressing policy issues somehow rely on some implicit or explicit function F( ). Second, the qual/quant distinction may be between those components of the outcome vector and of the arguments of the F( ) function that are truly quantitative and readily observable in standard data sources – i.e. household surveys – and those which are qualitative and/or not directly observable except through participation surveys. It may also have to do with the difference between outcomes per se and the way they should enter welfare evaluation, W(Q), or the definition of the poverty line vector Z(Q) and distance from it. An example of that distinction is the difference between consumption or income/poverty and Sen's capability view, or the attempt at constructing multi-dimensional measures of poverty – possibly using qualitative variables. Third, the qual/quant difference may be in the nature of the arguments of the function F( ) which are taken into account in the analysis of poverty. It is true that the quantitative approach tends to stick to quantifiable arguments like assets, prices, or access to public infrastructure. More qualitative approaches insist in addition upon the role of non-market institutions as possible causes of poverty – e.g. ethnic or gender discrimination, non-democratic decision making, marriage laws, etc. – , and in some cases sources of relief – social capital, community action, for instance. However, quantitative approaches also insist on the role of qualitative factors like market imperfections or some non-market institutions, even though empirical quantification is generally uneasy.

Altogether, these differences point more to a complementarity than an opposition of the two approaches. To be sure, what is to be understood ideally is the very nature of the F( ) relationship, in all its complexity. People on the quantitative side know very well they are missing very much when restricting themselves to quantitative variables and quantifiable relationships in the model F( ). On the other side, a more complete and qualitative look at the poverty issue may fail to reveal what the actual model F( ) is. This is because too much a detailed a description of the living conditions and behavior of the poor and the community they live in may actually hide the logics of poverty trap mechanisms by introducing some confusion between causes and outcomes. Progresses are being made from both ends, and more are hopefully to come. However, the inherent

complexity of the phenomena and mechanisms under study make them hard to obtain and slow to come.

Given this incomplete knowledge of the mechanisms at work, the question arises of whether an approach must be preferred to the other. In effect, it is not hard to see that the nature of the issue at stake should determine whether a predominantly quantitative or a predominantly qualitative approach is to be preferred and, in both cases, the degree of complexity of the representation of poverty that must be used. If the problem is to know how to allocate development aid across countries, it is likely that a single criterion like poverty or income consumption with some internationally comparable poverty limit will be sufficient – although even such a 'simple' criterion is far from being fraught from ambiguity. This is because differences in poverty with respect to the other components of Q are likely to be strongly, but not perfectly correlated with income poverty at the country level. From the point of view of that particular policy question, a crude quantitative approach may probably useful. However, this certainly does not validate such an approach in another context. At the other end of the spectrum, if the problem at hand is the likely impact on poverty of promoting broader participation in community decision-making, then a fundamentally more complex analysis and typically qualitative information gathering is required. In most cases, however, a mixture of quantitative and qualitative approaches will be used. For instance, one may try to figure out quantitatively what may be the impact of some means-tested cash transfer program upon the consumption of the poor, the health and the schooling of their children. At the same time, however, qualitative knowledge is indispensable to determine the type of means-test that may be used and how they may be administered. It would clearly be unwise to evaluate such a program ex-ante without considering simultaneously both types of perspective.

It is probably not far from a truism to recognize that a quantitative approach to poverty is necessary any time a poverty measurement issue is at stake. The point is that most policy decisions in the field of poverty implicitly or explicitly call for some type of measurement. Indeed, the fundamental question will always be of the type : "will instrument A do *better* than instrument B"? One way or another, this "better" often introduces the need for a "norm" and for measurement according to that norm. Thus, the issue is less that of the need for quantification than the quality of the quantitative instruments available. Disappointment with quantitative approaches to poverty may be due to the excessive simplicity of available instruments. This cannot be a reason to abandon quantification altogether and to switch to pure qualitative analysis. On the contrary, this should be a motivation to improve existing instruments thanks to a better understanding of poverty definition and poverty determinants. Again, the only real issue in poverty analysis is to improve our knowledge of the function F( ). But of course, this may precisely be where the mountain alluded to at the beginning of this note stands.

STRENGTHENING QUANTITATIVE METHODS THROUGH INCORPORATING QUALITATIVE INFORMATION

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The myriad problems associated with quantitative poverty assessments are now well recognized. Many of these deficiencies can be the subject of technical improvements in how surveys are carried out. Sampling errors that plague surveys can be significantly reduced through the development of better sampling frames. More rapid and improved data entry programs will go a long way to addressing widespread non-sampling errors. The use of tests of stochastic dominance, which make poverty comparisons based on welfare distributions, rather than poverty lines, can improve the robustness of inter-group and inter-temporal poverty comparisons.[1] While certain analytical problems, such as the inability to identify equivalence scales for comparing welfare, will remain, sensitivity analysis and employment of analytical techniques to narrow the range of plausible size elasticities will further improve the quality of poverty measurement.

Beyond these technical improvements, a number of other improvements, drawing upon what are traditionally identified as the strengths of qualitative methods, can be incorporated into quantitative poverty measurement. For example, quantitative methods have been appropriately criticized for not taking into sufficient account information on context, or, similarly, being too shallow or narrow in the questions posed. In part, these deficiencies are remediable at reasonable cost, even in large-scale household surveys, through incorporating techniques and approaches used more frequently in qualitative methods.

An important area where traditional quantitative surveys have been deficient, but where improvements have been made and can continue to be made, is in the area of gender relations, and specifically, intra-household inequalities in command over market and non-market goods. Simple improvements, ranging from not just relying on interviews with male household head to incorporating more complex and nuanced questioning on gender roles and decision-making in hybrid types of questionnaires, will go a long way towards remedying the previous neglect of these issues in quantitative poverty measurement. There may be some disadvantage in terms of sample size, or costs of conducting the survey, but these are outweighed by the improvements.

[1] The fact that poverty measures and poverty rankings can be sensitive to the choice of a poverty line has lead researchers to use stochastic dominance analysis to make robust welfare statements [e.g., Atkinson (1987), Foster and Shorrocks (1988a,b)].

Another source of weakness in traditional quantitative survey methods has been in the valuation of non-market goods, especially public goods provided by government, but also household public goods such as the health environment of the domicile. By their very nature, the value of public goods to either households or individuals is impossible to measure directly in monetary terms. Here, once again, drawing upon methods that are typically associated with quantitative assessment may be useful. For example, one of the most important roles of government is to provide public goods in the form of security and the judiciary. It is difficult or impossible to assess in money terms who benefits from the existence of the police and courts. However, the integration of questions into household surveys on issues such as a person's sense of security and safety, and notions of social justice – again, questions normally associated with smaller qualitative surveys – is another example of the useful bridging of methods. Furthermore, methods that have not been extensively used in quantitative poverty assessments, such as contingent valuation, warrant more careful experimentation and exploration.

It is also the case that the deficiencies in quantitative methods of poverty assessment alluded to above can be ameliorated to some extent by defining poverty in terms other than money metrics, specifically income and expenditures. The seminal work of Sen has drawn into question whether a focus on commodities and utilities is the appropriate metric of poverty, and whether they instead should be viewed as a means to desired activities or states. He makes a strong argument that there are compelling reasons to define poverty in terms of the lack of basic capabilities to avoid hunger, malnutrition and poor health, and to be adequately clothed, to partake in the life of the community, to feel safe and secure, and so forth. These functionings have the advantage of being direct measures of well-being. That is, low incomes are only "*instrumentally significant,"* while deprivation of capabilities, such as poor health are "*intrinsically important."* Consequently, health, nutritional status, education, as well as capabilities associated with qualitative methods such as control over household resources and the ability to participate in decision making, are argued to be a more direct measure of capability deprivation, or poverty, than income or expenditures.[2]

When we move to measuring poverty in terms of capabilities, we thus begin to bridge the quantitative and qualitative methods divide, and simultaneously address some of the inherent weaknesses of quantitative methods. For example, measurement of functionings or capabilities are generally associated with individuals, not households. Thus, with these measures we begin to gain insight into intra-household issues that are often neglected with traditional quantitative poverty assessments. For example, we capture discrimination against females as manifested in less education, worse health, and a more limited sense of participating in the life of the community and in the decision making of the household. Moving toward capabilities and functionings also implicitly incorporates the value of many forms of public goods. For example, certain household public goods, such as the health environment of the domicile, will be reflected in the health status of

[2] Sen points out, there is a great deal of variability between low capabilities, such as poor health, and low incomes. Thus, this instrumental relationship is conditional and contingent, differing by community, families and individuals.

individuals. Likewise, a person's sense of security and his or her degree of social exclusion, etc., are in part reflective of public goods provided by the state.

Employing non-money metric measures of well-being, even in the context of quantitative survey methods, has a series of other benefits. These include avoiding the difficulty of identifying appropriate price deflators and purchasing power parity indexes for cross-country or intertemporal comparisons. Similarly, the problems with comparability of questionnaires that plague money metric measures, such as recall period and how many commodities are listed on the questionnaire, are less severe when it comes to other more discrete measures of poverty.

However, as we extend the dimensions across which we measure poverty, empirical methods become more complex. We know that health and educational attainment, to say nothing of measures of social exclusion, insecurity, and other measures of deprivation, are often only weakly explained by incomes or expenditures. The correlations between money metric and other measures of poverty are often quite low. Likewise, the use of multivariate analysis to see how well, for example, incomes predict nutritional or health outcomes, has generally uncovered very weak relationships.

To help understand and reconcile this weak relationship, we are engaged in some research showing that it is theoretically and empirically attractive to make multidimensional poverty comparisons, and that we can do so statistically, and in ways that are robust to the specification of the poverty lines and to the choice of poverty indices (Duclos, Sahn and Younger 2000).[3] More specifically, we can consider cases in which a dimension (e.g., incomes) contributes to well-being, but does so differently for different sub-groups of the population, the other dimension which affects well-being. One rationale for this is that each sub-group can reasonably be expected to have a different level of well-being for the same amount of income.[4] One particularly relevant application of this approach is when one of the dimensions of poverty is discrete, perhaps based on indicators that are more closely associated with qualitative poverty measurement. For example, we might consider insecurity or social exclusion, to be an indicator of well-being. We then define people who feel secure, etc., to be better off than people at the same income level who do not. In this case, the sub-groups are identified by the value of the discrete variable, or qualitative notion of well-being, and we compare their levels of a continuous variable such as income.

Thus, we achieve a more general comparison of the correlation between two or more welfare variables by ranking the sample according to one indicator and comparing the cumulative distributions of that indicator and another, ranked on the first, and vice-versa. And then we employ valid statistical tests for dominance along two or more dimensions

[3] This research builds upon recent work that has established statistical methods for non-parametric comparisons of different welfare distributions (Davidson and Duclos, 1997 and 1998).

[4] Atkinson and Bourguignon (1982,1987) first used this approach in the social welfare literature to avoid the arbitrary and difficult choice of an equivalence scale for households' income based on their size and composition. Atkinson (1991) and Jenkins and Lambert (1993) developed the analysis for comparisons of poverty.

simultaneously, essentially considering the joint cumulative density of two or more welfare variables at the same time.[5] Instead of a single poverty measure, we can now think of a poverty surface defined by functions of the cumulative densities of each of the two measures of welfare. Of course, while this approach still relies on sample surveys, it partially bridges the qualitative and quantitative in as far as it provides an analytical approach for incorporating contextual and qualitative questions, generally associated with qualitative assessment methods, with money metric indicators closely associated with quantitative methods.

[5] Duclos and Makdissi (1999) begin to explore this sort of multidimensional analysis using household income deflated by different equivalence scales along the different axes.

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‘APT ILLUSTRATION’ OR ‘ANECDOTAL INFORMATION?’ CAN QUALITATIVE DATA BE REPRESENTATIVE OR ROBUST?

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I. INTRODUCTION

While the challenges inherent in identifying the complementarities between quantitative and qualitative data are currently a source of discussion, particularly since the publication of the recent WDR, this is by no means a new debate. For instance, some fifty years ago pioneering ‘applied’ social anthropologists, Max Gluckman and Clyde Mitchell, used the concept of ‘apt illustration’ and its associated methodological tool, situational analysis, to justify to colonial administrators the importance of qualitative research in explaining the complex political, economic and social phenomena associated with rapid rural-urban migration in Southern Africa. Between 1990-1995 as a World Bank staff member I undertook a study of the ‘impact’ of structural adjustment reforms in four poor urban communities in four different regions of the world. At the time the results were dismissed by most as neither robust nor representative, which at best provided ‘anecdotal information’.

Since I have been asked in this meeting to talk about ‘best practice’, in the spirit of ‘self-criticism’ associated with this event, I would like to draw on the research methodology in this and a subsequent poverty-focused research project to highlight a number of issues that I consider important in reconciling contradictions between these two approaches. In so doing I would like to contribute to two widely discussed questions:

- Can qualitative data be robust or representative?
- Is it useful to quantify qualitative data?

II. CAN QUALITATIVE DATA BE ROBUST OR REPRESENTATIVE?

While the distinction between quantitative and qualitative **data**, and survey-based and **contextual** methods (Booth et al. 1998; Hentschel 1999) is an important starting point, in itself it does not address the issue of the relative robustness and representiveness. The research project mentioned above illustrates this dilemma. In this project we used the same three identical research tools in each of the four communities studied (with inbuilt monitoring mechanisms) – to ensure uniformity in methodology and data analysis across the different research communities. In each research area we undertook three types of data collection using the two methods identified above (see Moser et al. 1996):

- Random sample survey method to collect statistically quantifiable data from around 240 households
- Sub-sample survey using both structured and open-ended questions to collect qualitative data relating to such issues as intra-household divisions of labour, domestic violence, attitudes to child labour etc.
- Community survey based on contextual methods such as participant observation, triangulation and interviews to collect qualitative community level data

The research highlighted similarities and differences in individual, household and community level vulnerability in the context of economic crisis in countries as diverse as Zambia, Philippines, Ecuador and Hungary, and identified the manner in which a household's management of its complex asset portfolio influenced its capacity to cope (Moser 1998). While community case studies such as these are qualitative in terms of the scale of the study, yet they provide quantitative information – hence one of the confusions.

In one of the four countries, Ecuador, conclusions reached in the study (such as the importance of housing as an asset) were further tested in the LSMS–illustrating the way contextual research can highlight issues whose robustness can then be tested at a representative level (World Bank 1996). However, the problem is that research results such as these are not representative at the national level. Is it possible to develop more inclusive measures of robustness so that complex social issues do note remain confined to anecdotal boxes, and can play a complementary role in poverty assessments?

III. IS IT USEFUL TO QUANTIFY QUALITATIVE DATA?

The debate between qualitative and quantitative methodologies has been further heightened by the increased popularity and usage of participatory appraisal methodologies–long promoted by Robert Chambers and his IDS team–and which could be said to fit on the far left end of the qual-quat continuum.

Given the challenges (as well as the time and cost) associated with 'micro-level' survey-based research projects such as described above, between 1998-2000 I opted to explore contextual methods through participatory urban appraisal (PUA) methodologies, through a research project on violence in urban poor communities in Colombia and Guatemala (first piloted in Jamaica) (see Moser and Holland 1997; Moser and McIlwaine 2000; 2000a). The contextual background to the Colombia project fundamentally influenced both the research methodology adopted as well as data analysis and therefore is useful to highlights as follows. Colombia, through a participatory country assistance strategy (CAS) process, had itself identified violence as its most important development problem with important implications for levels of vulnerability and poverty. The subsequent Sector Study undertaken by the World Bank (World Bank 1999) (in collaboration with local researchers) revealed two further issues of importance:

- The overriding preoccupation of politicians and researchers with political violence (as against economic or social violence);

- A lack of information of the poor's perception of the problem of violence and its relationship to poverty

This lack of information from the poor themselves consequently provided the rationale for undertaking the Colombian PUA of violence and poverty. Of the extensive number of methodological issues addressed in this study (Moser and McIlwaine 1999) two are of particular relevance to this brief paper. First, is it possible for a PUA to be representative rather than purely anecdotal? To reflect patterns of violence at the national level the nine communities were selected in cities and towns across the country chosen to reflect differences both in geographical, political and economic zones as well as in types and levels of violence. Fieldwork was undertaken collaboratively with 20 local researchers who, for safety reasons, had previous experience in the chosen communities. A total of 1,414 people participated in focus groups that were triangulated to reflect age, gender as well as position/status within the community.

The second issue concerned the importance of quantifying the results of an extensive numbers of focus listings relating to community perceptions of different types of violence related problems – given the current predominant preoccupation with political violence. Not only did this show diversity in the types of violence identified – one community in Bogotá for instance listed 60 different types – but it also showed that although political violence was a serious government priority in Colombia, it was not the major concern of the urban poor, for whom economic and social violence were more important. In relative terms local urban poor communities on average perceived economic violence (54%) to be a more serious than social violence (28%) with political violence of least importance (14%) in their daily lives. Data such this highlights the fact that political peace accords, while of critical importance, on their own will not reduce the high levels of violence, with its associated impact on both vulnerability and poverty.

Similarly, to summarize very briefly, the quantification of data from an extensive number of institutional Venn Diagrams across the nine communities provided evidence that one in five social institutions in Colombia is perceived by local poor urban communities to generate perverse as against productive social capital (see Rubio 1997). At the same time women's organizations were the most trusted institutions in local communities (88%) (Moser and McIlwaine, 2001b). Again there are important implications from the quantification of this PUA for those concerned with local-level initiatives to strengthen peace and 'rebuild' cohesion and trust.

Numerous important methodological questions remain unanswered particularly the issues of the validity of perception data *per se* in poverty studies*;* nevertheless pushing the methodological envelope of participatory appraisal techniques is intended to show how such new qualitative data has an important contribution to poverty assessments that goes beyond the quotations of individuals – which while emotionally powerful – also makes them highly vulnerable to the criticism of being neither robust nor representative.

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INTEGRATING QUALITATIVE AND QUANTITATIVE APPROACHES: LESSONS FROM THE PASTORAL RISK MANAGEMENT PROJECT

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My brief at this workshop, as I understand it, is to share with you how my collaborators and I have combined qualitative and quantitative approaches in field-based research in rural Africa, with particular reference to a multi-year, USAID-funded Pastoral Risk Management (PARIMA) project in southern Ethiopia and northern Kenya.

I. BRIEF BACKGROUND ON PARIMA:

- Interdisciplinary project with 4 PI/co-PIs (anthropology, economics, range science, sociology) and a variety of local and external collaborators across several disciplines. Began in 1997 and presently funded through 2003.
- Study region is a ~800 km transect stretching from the Boran plateau in southern Ethiopia to Lake Baringo in north central Kenya.
- Objective: understanding the etiology of vulnerability among pastoralists in arid and semi-arid lands (ASAL) of east Africa and what sorts of policies, projects and technologies might best reduce that vulnerability.
- Pastoralists extraordinarily poor, with poverty mainfest in powerlessness (in national and often local politics), meagre opportunities (the harsh climate limits agricultural options while poor infrastructure and low incomes limit non-farm ones) and tremendous vulnerability (to drought, flood, banditry, political violence, human and livestock disease, grain price shocks). PARIMA has concentrated primarily on the latter two general sources.
- Complex system, the stochastic poverty traps within which are best uncovered and understood through multiple methods (McPeak and Barrett 2001).

II. QUALITATIVE-QUANTITATIVE METHODS INTEGRATION

Sometimes by design, sometimes by accident, we proceeded by integrating qualitative and quantitative approaches from the outset in something resembling a feedback loop. This has had the desirable effect of producing a reasonably homeostatic research mechanism, in that it has proved self-regulating in pushing us toward our objective. The feedback loop depends fundamentally on multi-faceted integration of qualitative and quantitative methods to ask and answer several generic questions:

1) What does it mean to be poor in this setting? How does this vary across individuals, households, and communities and over time? [i.e., are we asking the right questions of the right people at the right time?]

2) Derivative from 1), are we measuring the correct variables and in the right manner?
3) Is our inference of the qualitative and quantitative data on those variables consistent (a) across research methods (a test of robustness) and (b) with local expressions of understanding of the problem(s) (a test of relevance).

III. PROCEDURALLY, THIS QUALITATIVE-QUANTITATIVE INTEGRATION HAS TAKEN PLACE AS FOLLOWS

- Participatory appraisal in study region preceded questionnaire design and survey site selection. This underscored issues the importance of which we had not previously appreciated. It helped us design a locally acceptable survey strategy (e.g., safeguarding project field staff, of direct interest). "Ethnography" precedes "sampling" in the dictionary and ought to in the field, as well.

- Began analysis on the basis of ethnographic and PRA type data to draw out patterns and explanations from a relatively small sample of non-representative data. Presented these findings, our assessments of the extant literatures, and a draft Phase II, survey-led research design to a workshop of largely external stakeholders (donors, local and national governments, NGOs, local researchers) for feedback and revision. Qualitative work now followed up with quantitative analysis of data generated by repeated quarterly surveys among almost individuals in 300 households across 11 sites.

- Follow up quantitative survey analysis with qualitative exploration of anomalous results and puzzles. Example: livestock "overcounting" uncovered the importance of complex property rights in animals defined by the social origins of the livestock … a potential (partial) explanation of the pastoralist marketing puzzle.

- Questionnaires integrate open-ended questions designed to explore matters tough to frame precisely through traditional survey instruments (e.g., historical perceptions, indigenous climate forecasting methods).

- Survey work is intended to be integrated with project's outreach activities which, through separate funding, include community-level participatory identification of priority needs and, soon, pilot activities to be accompanied by qualitative action research.

IV. SPECIFIC EXAMPLE OF PARTICIPATORY RISK MAPPING (SMITH ET AL. 2000, 2001)

We developed an open-ended technique for getting people to identify and rank threats that concern them. We did this first in purposively/opportunistically selected focus groups, wherein the field leader (an anthropologist) took notes on explanations of these assessments and on the context. We record the simple ordinal data in numeric form, out of which we subsequently constructed a simple index. Having georeferenced all the points by GPS, the geographer on our team could construct contour maps of risk

assessment, polygonal maps of ethnic territories, link these to extant biophysical data (e.g., rainfall), permitting quite useful and original inference. We could then also analyze the data using limited dependent variable econometric methods, using the ethnographic the ethnographic notes from the original focus group interviews to provide causal explanations of the observed statistical correlations. This process uncovered important structural patterns of heterogeneous risk assessment between men and women, rich and poor, Ethiopia and Kenya, pastoralists near and far from towns, etc. Since rangelands policies have historically treated pastoralists as a homogeneous population, this analysis has revealed unintended distributional and targeting effects of past policies (e.g., borehole development, pasture improvement). We have now built that participatory risk assessment instrument into individual-level questionnaires fielded every three months so as to track more micro-level and temporal variation in risk assessment and to match individuals' ex ante risk assessment to their ex post experiences. That data collection is at a midpoint and preliminary analysis of it will begin soon.

V. LESSONS LEARNED THUS FAR

Bad practice is bad practice, whatever the method. There is no unconditional superiority of one class of methods over another. The germane questions appear to be:

- "when and how is good practice within a given class of methods still wanting?"
- "can another class of methods, well applied, fill in the blanks?" and
- "how can one method validate the findings of another?"

The breadth versus depth dichotomy (even unidimensional continuum) appears unhelpful, incorrect, or both. From our experience, it seems that a more apt representation might be Euclidean, ranging from the specific to the general on one axis and from subject-driven to researcher-directed on the other axis. Qualitative autobiography would occupy one corner (specific/subject-driven) of that box, a quantitative census the opposite corner (general/researcher-directed), with considerable opportunity for creative combination of qualitative and quantitative methods in between.

Mixed methods approaches can work, each enhancing the efficacy of the other.

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TOWARDS A BETTER COMBINATION OF THE QUANTITATIVE AND THE QUALITATIVE: SOME DESIGN ISSUES FROM PAKISTAN'S PARTICIPATORY POVERTY ASSESSMENT

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I. PRELIMINARIES

This note takes as read that drawing together information about poverty that has been gathered in different ways is a good thing, with benefits on both sides of the qual/quant divide in terms of the richness and robustness of findings and analysis. The focus is on the development of better practices for achieving integration of survey-based and contextual methods, in the context of a nationally-led, policy oriented process such as the PPA which is about to begin in Pakistan.

It may be helpful to distinguish between:

- Iteration (between surveys and contextual investigations).
- Linkage (of contextual investigations to survey frames and results).
- Triangulation (between different data sources, both between and within the two main methodological traditions).
- Convergence (where contextual methods take on properties normally associated with surveys).

The following notes are mainly about linkage and triangulation, taken in reverse order, with a final comment about convergence.

Some further distinctions are important if we are to avoid an unduly confused discussion. As the wording used above implies, I agree with those who prefer to speak about quantitative and qualitative *data*, but survey-based and "contextual" *methods*. In addition, it needs to be recognised that contextual (so-called qualitative) methods are of different sorts. Some of the claims made on behalf of contextual methods are only valid for the sub-class of *participatory* methods; and some of the reservations expressed by survey analysts about contextual methods mainly apply to the sub-class of rapid-appraisal approaches, to which modern participatory methods belong.

II. PARTICIPATORY METHODS AND TRIANGULATION IN PAKISTAN'S PPA

The last set of distinctions is essential to get a proper sense of the possible relevance to this workshop of the PPA in Pakistan. First, a word of introduction to the PPA itself.

This is a national exercise being organised by a partnership of Pakistan's Federal Planning Commission, provincial governments and national and provincial NGOs. It is intended to feed into the final drafting and then the monitoring of Pakistan's Poverty Reduction Strategy Paper.[1] It is not to be confused with the World Bank's current Poverty Assessment for Pakistan, which has an interesting component of participatory assessment.

Pakistan's PPA is designed to be highly participatory, in its organisation and in the style of its fieldwork. This is only partly because participatory fieldwork – guided by the principles of participatory reflection and action (PRA) – is a rich and robust source of understanding for policy purposes. It is also because this kind of data generated in this kind of way can have an *influence* that other kinds do not.

The use of the PRA approach imposes certain obligations. Pakistan's PPA incorporates a phase of follow-up action in the study areas that has not always been included in national PPAs. This should help to ensure that the process is not overwhelmingly "extractive", as PRA practitioners would put it.

There are legitimate concerns, especially among devotees of methods that are contextual but not rapid or participatory (e.g. anthropologists), that PRA work can be naïve, epistemologically and politically. The design of Pakistan's PPA takes this seriously.

This involves drawing fully on practices that are very much part of the PRA philosophy even if occasionally neglected when the distinctiveness of the participatory method is being stressed and "alternative paradigms" are in the air. The key concept is triangulation.

PPA fieldworkers, and those who will be responsible for synthesis at the provincial and higher levels, are being encouraged to triangulate everything, for the purposes of both validation and analysis. This is partly a matter of cross-referencing between the different focus groups and instruments of PRA facilitation. But it also involves drawing evidence from key-informant interviews and direct observation, and the results of sociological studies and small-scale surveys wherever these can be found. The timetable, although budget-constrained, aims to allow sufficient time for this to happen.

[1] There is some external funding and some external technical assistance, of which I am a relatively minor element. The views expressed here are mine. For further information on Pakistan's PPA, contact R. Dohad and M. Rimmer in the national support office: rdohad_ppa@isb.pol.com.pk, mrimmer_ppa@isb.pol.com.pk

Quantitative data may, in this sense, be quite a significant part of the PPA process, without taking into account the points about linkage made below.

III. LINKAGE AND SAMPLING

I am leaving aside here the role of iteration between surveys and contextual work (survey design teams read PPA reports and anthropological case studies, and vice versa, and adjust what they do in the light of this). This is important but I have nothing significant to add to what I expect will be a good discussion.

The other major topic is how surveys and other poverty-assessment instruments can be linked through their design – particularly how PPAs can be linked to major household surveys. There are a strong and a weak approach to this. The first involves using the survey's sampling frame, so that the PPA case studies can be integrated with data from the survey on the same communities and households. A high-water mark of practice in this regard is the 1995 Tanzania PPA and social capital study, which used the sampling frame and the data from a recent human resources survey with striking results.

Unfortunately, the conditions for doing this may not be all that common. The sampling frames of national household consumption surveys generally do not lend themselves to playing a part in PPA site selection. At least, that was found to be the case for the Pakistan Integrated Household Survey (PIHS).

The weaker approach is the one pioneered by Jesko Hentschel and his team in the first World Bank PPA in Ecuador. This is going to be adopted, with a possible additional element, in Pakistan's PPA.

Linkage with the PIHS is particularly important in Pakistan, which has a fairly survey-oriented official culture and one that does not accept the notion of purposive sampling all that readily. Linkage will be facilitated by the fact that, at the levels of the technical assistance, the national organisation and the composition of the fieldwork teams, PIHS personnel are involved in the PPA.

The PPA sites are being selected purposively, with a view to the diversity of Pakistan' poor and the desirability of some comparative cases or control-groups. As well as undertaking the PRA work, including triangulation, the teams will be administering short community and household questionnaires using items from the PIHS interview schedule.

The analysis of the resulting data will be used to "place" communities and groups of households *ex post* in the national distributions for a number of key variables. Work is being done by the PIHS support team from Oxford Policy Management on the selection of indicators. Several of these will be correlates of household consumption poverty.

The additional element is statistical work (logistic joint regression) on the latest PIHS data to generate a good set of predictors of consumption poverty. The intention is that the households should be able to be placed in the national distribution of consumption poverty, on the basis of which poverty-line and income-distribution studies are done, as

well as in the distributions for the indicators covered by the questionnaire. The procedure would be akin to that used in analysing results from CWIQ surveys in Ghana and elsewhere.

Assessment of the robustness of the predictors is continuing at the time of writing, and the outcome is uncertain. It seems likely that different sets will need to be used for rural and urban sites.

IV. SAMPLING WITHIN PRA: WEALTH/WELL-BEING RANKING

Another sampling issue relates not to the selection of PPA sites, but to what happens during the fieldwork in each site. PRA relies quite heavily on facilitated discussions in focus groups, whose composition is in practice only loosely controlled by the research team. With sufficient pre-planning and negotiation, however, focus group members can be selected in a reasonably systematic way.

In Pakistan's PPA, a substantial effort is going to be made in this respect. A major element of each site study will be a wealth ranking exercise. This will serve, first of all, as a means of exploring concepts of well being and perceptions about the distribution and causes of ill-being. But it will also be used to construct a ranking of households within a given population, which can serve as a sampling frame for the rest of the work. The household ranking will be able to be used as a guide to the constitution of focus groups (bearing in mind practical constraints). In large, densely settled sites, it may also be a useful basis for a stratified random sampling approach to the questionnaire work.

V. CONVERGENCE: GAINS AND LOSSES FROM STANDARDISING PRA

What I have just described is a sound use of well-being ranking, one that is squarely within the PRA tradition. We are not so convinced by another use of participatory rankings intended to provide linkage with survey-based poverty analysis. This is the procedure of aggregating a set of local well-being rankings and comparing the result with a national poverty profile derived from a household survey – usually for the purpose of showing that they are similar.

A number of PPA reports in the 1990s included an exercise of this sort, where the products of very different methodological procedures are treated as comparable. It exemplifies what has been a minor feature of PPA practice in the past, but one that may be becoming more common. We might call this the problem of convergence.

The worry about convergence (as opposed to iteration, linkage and triangulation, which are all unreserved gains) is largely a personal one of mine at this point. It has not yet been much discussed in Pakistan.

My concern is that, under pressure to produce bold conclusions and strong policy messages, PPA fieldwork will become more like survey work. The use of increasingly standardised schedules may serve to generate results that can be aggregated upwards in numerical form, and thus be more legitimately compared with survey findings. But this

will imply corresponding restrictions on exploratory, responsive and reflexive enquiries, which are rightly considered the *forte* of contextual methods. The danger is that this will result in a loss of precisely the features for which PRA has been recognised as a robust method, capable of generating valid, reliable and illuminating results. That would be a pity.

POTTERS AND SLUMS: TWO QUALITATIVE/QUANTITATIVE PROJECTS IN INDIA

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This note is written for the best practice session of the conference which is meant to highlight examples of studies that have used mixed-methods. I will therefore focus quite narrowly on two projects that I have been involved with that, from their inception, combined qualitative and quantitative methodologies to study poverty and well-being in poor communities. The first is a study of a "backward caste" community of potters from rural Karnataka in Southern India, where I conducted two rounds of fieldwork in 1992 and 1994. The second is an ongoing study of poverty, risk and networks in Delhi slums conducted jointly by a multi-disciplinary team with Lester Coutinho – an Anthropologist, Arup Mitra – an Economist, and Michael Woolcock – a Sociologist.

I. THE POTTERS PROJECT

The aim of this project was to understand how socio-cultural and economic systems interact to affect living standards, focussing on the role of marriage markets. I picked a community of potters spread across three villages mainly because they were an endogamous (a group that restricts marriage to partners within itself) caste that exchanged brides between the villages and, thus, represented a single marriage market. The first round of fieldwork was conducted with a team of social workers who were all extremely familiar with local conditions. We planned to conduct a series of PRA exercises, focus group discussions, and in-depth interviews as well as administer a structured quantitative questionnaire to every household in the community. The questionnaire was a standard LSMS-type instrument with a section on marriage markets. I also added a few questions on domestic violence because during an initial visit I witnessed a woman being severely beaten by her husband and wanted to explore the issue in greater detail.

In the first week our respondents told us, basically, that life was tough but manageable. Their main problems were with the government – lack of good schools, lack of adequate drinking water etc. But, their husbands were "good" and families tried to cope with problems as best as they could and helped each other all the time. After a week of staying in the village and continuing our interviews, one of the women finally opened up and said, "You have become our friends and we can't lie to you anymore. Let me tell you the truth. We feel like we are in jail. Our husbands beat us all the time and no one helps when that happens. They spend all the family money on alcohol, and when they come home – no one can help us." This then generated a crucial discussion on domestic

[1] I thank Martin Ravallion, Sita Reddy and Michael Woolcock for valuable comments.

violence that led me to understand its centrality in determining household behaviour. We then explored it further through in-depth interviews with men and women who outlined the cold rational calculation behind much of the violence. This led to two papers, Rao (1998) on the determinants of violence and its impact on intra-household resource allocation, and Block and Rao (forthcoming) on the link between marriage markets and domestic violence.

Qualitative fieldwork, unlike the quantitative survey method, allows issues to be probed in the field the moment they are observed. This permits "surprises" to be easily incorporated into the data gathering process. I observed two phenomena while in the field that led to a reorientation in the focus of the analysis. The first was that purchases of small quantities of food in most village shops tended to be sold at much higher unit prices. I spent a few days talking to shopkeepers about this practice and also investigated it in focus group discussions, and found that it was a serious problem that households had found ingenious ways to cope with.

The second thing I noticed happened quite by chance. We were conducting fieldwork for a few weeks, when we were told one day it would be "difficult to find people tomorrow because they would all be at Marriamma's festival." Observing the festival helped me appreciate the signal role that collective celebrations play in rural life. Unfortunately, since the quantitative survey instrument had already been constructed I could not study either issues quantitatively in any depth at the time.

In order to collect more information on prices and festivals, I constructed a specialised survey instrument for a second round of data collection that was conducted in 1994. The qualitative work conducted in the first round informed the construction of a specialised survey instrument. The work on prices shows that the quantity discount effect is severe enough in these communities that "real" inequality is 12-23 per cent greater than inequality measured with incomes uncorrected for price differentials (Rao, 2000). The work on festivals shows that expenditures on them amount to 15% of a household's budget. Families who participate more in festivals generate significant private returns – on social status, the extent of networks, and lower prices on food (Rao – forthcoming).

II. THE DELHI SLUMS PROJECT

This ongoing project has four main goals:

a) To understand networks not just from an economic perspective but to see how they were embedded in social and cultural systems.
b) To understand the sources of risk in urban areas, strategies employed to cope with them, and how these differ from rural risk.
c) To understand how slums are governed and their capacity for collective action.

Learning from the potters project, this study was designed to allow qualitative work to play a central role in survey design from the outset. We initially conducted three weeks of fieldwork in four slums with a team of ten graduate students from Delhi University. The

topics covered were deliberately left somewhat vague to allow for "surprises." At the end of the three-week period a questionnaire workshop with the entire team was organized where we systematically constructed a questionnaire on the basis of our qualitative findings. At the end of the questionnaire workshop, the team went back to conduct three more weeks of qualitative interviews in the four slums, during which period the structured survey instrument was also pre-tested and modified. In the 7th week, the questionnaire was administered to a multi-stage probability sample designed to represent all Delhi slums.

The data is now in the process of being analysed. All the text data from the interviews has been entered and coded into QSR-Nudist, a qualitative data analysis program, and the quantitative data has been entered and coded. We are in the process of analyzing our results but some initial findings are:

A. Networks

Survival and mobility involve inter-related strategies, but networks that work for survival may not necessarily work for mobility. However, households with good network endowments are clearly much better off. Risk is largely related to the threat of demolition and insecure property rights. Therefore, the acquisition of ration (PDS) cards, voter IDs and other marks of "citizenship" are an important survival and mobility strategy. Inequality is high with the Gini for expenditures at 0.42. Reduced form regressions of the determinants of living standards (expenditures, health risk, quality of housing, water quality) show that exogenous network endowments – who you knew in the city before moving to the city – are just as strongly correlated with living standards as wealth or human capital.

B. Governance

Slums have governance structures that are similar to village governance structures with a "panchayat" and a "Pradhan" (headman), but they are not usually religion or caste based. These governance structures are quite effective at resolving problems within the slum. Survey data show that Pradhans are the single most important source of resolutions to problems faced by slum dwellers. We also find that slums are very important in the political process as "vote banks" that can be tapped en masse to place ballots for one political party or another. This clout is effectively used by slum residents to obtain favors from politicians. In fact, 25% of slum residents have regular contact with Members of Parliament.

III. LESSONS LEARNED

A. "Classical" and "Bayesian" Methods of Integrating Qualitative and Quantitative Data[2]

Classical integration involves the development of hypothesis from qualitative data that are then tested for their generalizability with representative quantitative data. For economic analysis, an ideal way to do this is to construct theoretical models on the basis of qualitative findings, and then test predictions from these models with quantitative data (Rao, 1997).

Bayesian integration involves an iterative process. Start with qualitative data collection and construct a survey questionnaire. Then, on the basis of what you find, revisit the community to correct doubts and potential mis-interpretations. This may involve another quantitative survey. One of the great values of doing qualitative work is that issues can be instantaneously probed allowing for easy incorporation of surprising findings in the field.

B. Listening to What People Say Versus Understanding Why They Say What They Say

Surface impressions can be quite wrong, and quantitative structured questionnaires on sensitive issues conducted in communities where good rapport has not been established can generate inaccurate responses. Qualitative data can help uncover the nature and extent of measurement error in quantitative data. However, as indicated by the example with the potter women, qualitative data that is not the result of a relatively long-term stay is also often wrong and can be just as misleading.

It is not enough to record what people say; in-depth probing is required to understand *why* they say what they say, otherwise the analysts' own biases will have great influence on the interpretation of the data.

C. Qualitative Understandings Can Be of Great Value in Econometric Work

They can be very helpful in understanding the direction of causality and in finding identifying restrictions. For instance in studying festivals, I found that families with young daughters would spend more money during festival seasons to display them in the marriage market. This was uncorrelated with any of dependent variables I was trying to explain and therefore provided a good exclusion restriction.

Another example concerns some work on the loss in income suffered by sex workers when they use condoms due to a preference against condom use by clients (Rao et. al., 2001). The econometric problem here is that it is very difficult to identify such compensating differentials because they tend to be plagued by problems of unobserved heterogeneity and endogeneity. Qualitative work in this case helped solve the problem

[2] I should emphasize that this is my language and not standard usage in the qual-quant literature.

by locating an instrument to correct for the problem. We spent about half a day visiting brothels in the area where the survey was conducted and discovered that an HIV-AIDS intervention that instructed sex workers on the dangers of unsafe sex was administered in a manner uncorrelated with income or wages, but yet had a great influence on the sex workers' propensity to use condoms. Exposure to the intervention was therefore used as an exclusion restriction in simultaneously estimating equations for condom use and wages to find that sex workers suffered a 44% loss in wages by using condoms.

Qualitative work can also be helpful in understanding the nature of bias and measurement error – it helps to have had tea with an outlier.

D. How Much (Measurement) Versus Why and How (Behavior)

As demonstrated by the work on prices, qualitative data can shed light on improving measures of inequality and poverty. They can also provide insights and hypothesis on social and economic behavior and help interpret econometric results.

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THE QUAL-QUANT DEBATE WITHIN ITS EPISTEMOLOGICAL CONTEXT: SOME PRACTICAL IMPLICATIONS

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The discussion about qualitative versus quantitative poverty appraisals or any mix thereof, is as much a debate about what researchers allow/determine poverty to be, as it is a debate about the right methodology or methodology mix to assess, i.e. to gather, analyze and interpret information about it. While it would be an exaggeration to equate methodological preference with the analyst's perspective on the nature of knowledge, they are not completely independent either.

This note starts with a brief discussion of the – implicit – assumptions each method typically makes about the nature of reality and the best ways of gaining access to that reality to formulate knowledge about it. We then illustrate how these philosophical considerations partly drive the methodological choice by examining two sets of inquiry methods to assess undernourishment, a crucial dimension of poverty.

I. THE QUAL-QUANT DEBATE WITHIN ITS EPISTEMOLOGICAL CONTOURS

The school of thought associated with quantitative poverty assessments is logical positivism. In this view, there exists a single, external reality and it is the analyst's task to capture this as closely as possible. To do so, the analyst seeks to increase the likelihood of unbiased, objective answers mainly by relying on statistical principles in its study design (experimental, quasi-experimental, representative sampling) and structure, standardization and quantification in its data collection. The former principles are intended to guarantee representativity permitting a generalization of the results for the population under study. The latter aim – amongst others – to solve problems of bias and variability in the interviewer-interviewee interaction (Tourangeau, 1990).[1] The analyst's role is limited to the provision of objective information to the decision makers.

Qualitative research methods on the other hand are associated with the interpretivist and the constructivist tradition. These views start from the recognition of a multitude of realities, and believe that objectivity and value-free science are simply impossible (House, 1994; Hedrick, 1994). To fully understand the topic of interest within its context, the inquiry methods used seek to involve many stakeholders and to obtain

[1] The article by Suchman and Jordan (1990) on "Interactional troubles in Face-to-Face Survey Interviews" in the Journal of the American Statistical Association as well as the many comments it subsequently provoked provide an excellent overview of the issues involved.

multiple perspectives on the subject of research and the meaning of the concepts, through semi-or unstructured, exploratory data collection methods. In the constructivist tradition, the analyst does not only aim to provide and facilitate an understanding of the subject, but also seeks to bring about change and empowerment of the stakeholders in the process.

In their extreme form, the differences between the two paradigms typically associated with quantitative and qualitative methods could be summarized as dichotomies of objectivity versus subjectivity, fixed versus emergent categories, outsider versus insider perspectives, facts versus values, explanation versus understanding, and single versus multiple realities (House, 1994).

Thus, in using a particular methodological family to explore the subject of interest, one also tends to imply certain ontological assumptions about that reality. In this sense the methodological debate is as much about the kind of reality we want to discover as it is about methodology. This does not at all imply that both methods cannot be combined, but rather that the manner and the extent to which we use and combine both methods will affect the realities we discover.

It brings us to the heart of the discussion about poverty assessment. What do we want/allow our findings about a population's poverty to be? Are we assuming and looking for an objective, singular and universal kind of poverty which we can be externally defined? Or is poverty in essence a context specific, subjective reality to be defined by each subject herself? Is feeling poor, being poor? Or can we be poor, without even knowing it? The extent to and the manner in which either method drives our poverty assessments, will also affect where our findings are in the subjective-objective poverty space. To illustrate this point we examine two different mixes of qualitative and quantitative methods to assess people's undernourishment, a crucial dimension of poverty.

II. ASSESSING UNDERNOURISHMENT: INTEGRATED APPROACHES IN PRACTICE

A. Observed Individual Caloric Intake

This approach to measuring undernourishment, frequently practiced by nutritionists, seeks to obtain detailed data on the individual food intake. A representative sample of individuals is drawn from the population and an enumerator follows the individual throughout the entire day, measuring the amount of food served to and actually consumed during regular meals within the household as well as snacks eaten between meals or meals eaten outside the household. Using conversion tables the intake data are converted into calories and compared to objectively and externally determined standards of caloric needs, which are derived from laboratory experiments and adapted for individual characteristics such as age, sex, weight and activity level.

The spirit of this approach is clearly positivist. Hunger is externally defined in absolute and quantitative terms and the (only) task of the analyst is to provide policymakers with

the most accurate information possible about individual caloric intake. To do so, quantitative research methods dominate this approach.

Yet, proper implementation and interpretation of the approach also requires substantial contextual knowledge. Data are social products and to the extent that the analyst is unaware of the social context, he may be easily misled by his respondents in an attempt to avoid cultural taboos or to distort the information in their advantage. Knowledge of the "typical" organization of meals such as serving habits (e.g. man served first, women and children afterwards) and culturally sensitive issues, will help the analyst qualify and interpret his observations.

Exploratory discussions with key informants and focus groups can provide such information and thus greatly contribute to the collection of objective and accurate data. Such interactions may also help create an atmosphere of trust in which the respondents will be more willing to reveal their true consumption behavior. However, qualitative techniques will only be admitted in this approach to the extent that they improve the accuracy of the information collected on actual food intake. The potential introduction of subjective elements through the use of qualitative methods will not be tolerated. Similar observations hold for the economist's approach to measuring undernourishment which is most often based on reported food acquisition by the household obtained in the context of a household survey. This methodology results in less accurate estimates of actual food intake, though the underlying ontological and epistemological assumptions are the same.

B. Indices of Household Coping Strategy

In this approach, undernourishment is measured based on reported information on households' behavior in the presence or threat of food shortages. In a first step, focus group interviews are used to answer the general question: "What people in the community do when they do not have enough food, or enough money to buy food?". The reported strategies are subsequently ranked by the different focus groups in the community according to their severity and these ranks are translated into severity weights. In a second step, questions about these strategies (e.g. frequency of reliance on less preferred foods in the recent past (e.g last 7 days); frequency of purchase of food on credit; frequency of skipping meals by particular household members, etc.) are administered to the most informed person in the household. Households are selected through representative sampling. Finally, the responses are combined in a numerical index, with each strategy weighted by its frequency and severity. Ranking of households based on this index is used as an indication of relative undernourishment (Maxwell et al., 1999).

Undernourishment in this approach is not defined in terms of objectively countable calories, but rather in terms of people's reported subjective experience of food stress. Qualitative methods are used as they are more adept at capturing such a subjective and context specific reality. To enable comparison across households within the community and across communities, these methods are complemented with techniques from the quantitative tradition. However, quantitative techniques are not intended (nor allowed) to

strip the obtained results from their subjective and location specific content. Note for example, that different communities may behave differently under food stress, resulting in coping strategy indices based on different behavioral questions. In the interpretivist tradition, such indices may still be considered comparable across communities, given some normalization, even though their content is apparently different. They are comparable in the food stress perception space. Empowerment of the respondents may or may not be an explicit goal of the inquiry.

Alternatively, the coping strategy index approach could be seen as a quick and dirty way to obtain a crude estimate of undernourishment defined in caloric terms. Quick and dirty approaches are often needed in practice because of time and resource constraints. From a positivist perspective, they would be only defensible if the results are comparable to those from the observed individual caloric intake or the reported household food intake methods, which are assumed – especially the former of the two - to provide more accurate estimates of actual caloric intake. Validation is thus necessary and discrepancy between the findings from both methods must lead the positivist analyst to reject the coping strategy index as a valid alternative because of epistemological considerations.

III. CONCLUDING REMARKS

This note briefly illustrates how ontological and epistemological considerations partly drive the methodological choice. In that sense, the debate about qualitative or quantitative inquiry techniques to assess poverty, is a much a debate about the nature of reality and how to get access to it as it is a debate about methodological choice.

Despite these fundamental dichotomies at the philosophical level, this note also shows that there is clearly quite a bit of scope for combining qualitative and quantitative inquiry techniques in the design of studies and the collection of data about poverty. In using techniques from these different traditions, the analyst needs to be alert to the fact that each methodology is typically better at revealing a particular kind of reality and he needs to watch the extent to what the use of a particular inquiry methods confounds his findings about the reality he actually wants to obtain.

Finally, as much as the recent desire to combine qualitative and quantitative techniques in assessing poverty is driven by a more general paradigm shift towards postmodernism as well as the emergence of ethical theories of well-being which require more subjective input (e.g. Sen's capability theory), it is also driven by practical and budgetary considerations. Qualitative inquiry techniques are often propagated – and therefore popular – as inexpensive tools which allow quick and accurate answers to complex questions. It is especially in this field of tension between financial considerations and the still lingering desire to discover this one universal "truth" that the triangulation question arises. Yet, while popular, the argument is also misleading. Not only may qualitative techniques reveal different realities, thorough qualitative research is equally time and skill intensive.

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INTEGRATING THE QUAL AND THE QUAN: WHEN AND WHY?

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I. INTRODUCTION

The desirability and usefulness to combine quantitative and qualitative methods to analyze social realities is pretty much accepted in the literature today; voices of segregation – still quite powerful in the 1980s – have subsided notably. This note attempts to provide some classification when and why such combination is fruitful. It also offers some pointers as how this could be done.

II. CONTEXTUALITY AND QUAN-QUAL

Let me start by drawing on the distinction between the *methods* of data collection and the *type* of data that is collected as put forward by Boot el al. (1998) and Hentschel (1999). Often, the terms 'qualitative' and 'quantitative' are used to describe both the method of data collection (i.e., closed ended household survey, participatory ranking exercise, direct observation) as well as the type of data that is collected (percentage of children enrolled in school, parents reasons why they don't send their kids to school). But what is generally referred to as 'quantitative' methods (large-scale household survey) often produce 'qualitative' data (reasons of parents not to send their children to school) and vice versa. So distinguishing method and type might be useful for our discussion:

- For data types, the quantitative – qualitative distinction is maintained;
- For data collection methods, however, the term 'contextuality' is introduced. Methods are contextual if they attempt to understand human behavior within the social, cultural, economic and political environment of a locality.

We could then think of both methods and data to lie on a continuum stretching from a *more* to a *less* contextual methodology and from *more* to *less* qualitative data output. For example, large-scale household surveys or the census tend to be less contextual with largely quantitative data being collected. Especially designed village surveys (e.g., Lanjouw and Stern 1991) would also tend to collect rather quantitative data but with a more contextual methodology. Social mapping exercises could be classified as contextual data collection with a qualitative output and soon.

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III. PRIMARY, LEAD AND CONTROL FUNCTIONS

To explore the links between different data collection methods, it might be useful to distinguish different functions they perform to fill information needs. Let me distinguish the following functions:

'primary :	collection method is most important source of information;
'lead':	collection method exploring information needs that another one can then investigate further;
'check & follow-up':	potential of one collection methods to confirm or contradict ('triangulate') a primary one, deepen understanding of 'lead' research or test validity on different scale.

The consensus to combine 'qual' and 'quan' stems from the necessity that, to understand social phenomena, combinations of data collection methods are necessary. I would also hold, though, that similar data collection methods perform different functions (as to primary, lead, check & follow-up roles) to fill such information needs. Hence, I think the argument whether data collection methods should be done simultaneously or in sequence (Carvalho and White 1997, Chung 2000) – and which one should come first – depends on the nature of the specific information need.

Consider for example the role of contextual data collection methods such as open interviews, participant observation and focus groups discussions to understand health service utilization:

primary:	such data collection methods are the primary (and at times unique) source of information for a whole host of factors influencing the health service utilization behavior. The importance of traditional health beliefs (which can vary considerably between different ethnic groups in a country), local perceptions of the severity of disease (which can be quite contrary to 'objective' severity), the importance of traditional providers in social life are among these as well as corruption of local health center staff, or intra- and inter-community perceptions of feud and conflict;
lead:	exploring functions of such data collection methods – crucial for the design of others – are in the areas of the staffing of the health center (gender, language, behavior), importance of infrastructure and drug availability, knowledge of health center staff, seasonality of disease and welfare etc.
check & follow-up:	the same contextual methods can be used to test and explore at the local level several 'general' results which largely stem from non-contextual data collection methods. For example, they are important to understand if general results on the functioning of user cost exemption policies apply to local contexts or what meaning 'distance' to health centers has in a particular setting.

Contextual data collection methods (with quantitative and qualitative data output) could be assigned a 'primary' function for all information needs if our aim was to understand a social phenomena in a locality only. But such locality specific information is not sufficient for policy or planning decisions. Hence the importance of non-contextual methods of data collection (to obtain average price elasticities of demand or average provider shares).

IV. UNIQUENESS AND COMPLEMENTARITY

The above described 'uniqueness and complementarity' of different data collection methods has implications:

- If information can be obtained through contextual methods of data generation only, strict statistical representability will have to give way to inductive conclusion, internal validity and replicability of results. Especially in these cases, contextual data collection procedures have to be very clear about how they select the 'sample', how recording takes place, how it is interpreted etc. If at all possible (and the data collection method allows for it), random sampling techniques should be applied in localities to avoid the potential bias of researchers ("we went to the poorest segments of town – the slums – and explored what people understood by poverty"). Increasingly, such sampling takes place (e.g., Kozel and Parker 2000, Moser 1996, Mwenesi 1995). Also, careful quantification of qualitative data can take place using scales that would allow for probing of significance within the local context (e.g. Carey 1993, Loos 1995). At times, carefully conducted contextual research might also have to substitute for missing or bad quality non-contextual surveys (Dudwick 1995)
- For some information needs, contextual methods are needed to design appropriate non-contextual data collection tools (e.g. Holland 1997)
- Where information requires non-contextual data collection methods, contextual methods play an important role for assessing the validity and interpretation of the result at the local level (e.g., Kozel and Parker 2000, Rawlings 2000). Here, formal combinations between the different data collection tools would need to be established. Some examples are:

(a) Collect information in contextual data collection exercises that allow to 'place' or 'fit' community/locality in broader picture – "how similar to an average community in the lowest income decile is the study community"? (Kozel and Parker 2000, World Bank 1996).
(b) Choose sub-set of communities for contextual data collection that form also part of non-contextual data collection sample (Kozel and Parker 2000, Rawlings 2000 for schools);
(c) Choose sub-set of same households for in both non-contextual and contextual (Kozel and Parker 2000).

V. FUTURE RESEARCH

I would suggest that – when discussing the integration of 'quan' and 'qual' we have to make an important distinction in future research. On the one hand, future research will need to tackle how best to integrate *contextual methods producing more quantitative data outputs* with *contextual methods linked to more qualitative data as outputs.* The contributions of Chris Barrett and Vijayendra Rao's contributions to this workshop is an excellent example how this can be done and what options exist. On the other hand, however, future research will have to tackle how *data generated by contextual methods* can best be combined with *data generated by non-contextual methods*. Here, the crucial building block, in my view, will be an assessment when and how contextual information – i.e., that gathered in the context of a specific locality – can and need to be 'generalized' to lend itself for the combination with (representative) non-contextual information. Answers to this latter question will become more and more important if contextual data collection methods are going to increasingly influence planning and design of national social policies.

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DIFFICULTIES IN COMBINING INCOME/CONSUMPTION AND PARTICIPATORY APPROACHES TO POVERTY: ISSUES AND EXAMPLES

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I. INTRODUCTION

This short note identifies a number of difficulties which arise when combining the income/consumption (I/C) and participatory (PA) approaches to poverty. These difficulties stem from the different types of data collected and the different processes of data collection and analysis. The qualitative/ quantitative distinction does not illuminate the most important of these differences (though it maps onto some). Empirical examples illustrate the issues raised.

II. DIFFERENCES BETWEEN THE I/C AND PA POVERTY APPROACHES

Table 1 below identifies four differences between the I/C and PA poverty approaches which may complicate, or invalidate, attempts to combine the results which they generate and/or the methods they use. The three means of combining approaches – confirming/refuting, integrating methods, merging results – is based on the typology of Carvalho and White (1997).

DIFFICULTIES IN COMBINING THE I/C AND PA POVERTY APPROACHES

Key Difference		***Poverty Approaches***		***Means of Combining***
		I/C	**PA**	
1	Well-being Measure	Common Unit	No Common Unit	Confirming/ Refuting
2	Basis of Extrapolation	Statistical Inference	Empirical Generalization	Integrating
3	Conception of Ill-being	Non-Fulfillment of Basic Preferences	Multidimensional	Confirming/ Refuting; Merging
4	Prioritization of Policies	Statistical Valuation (e.g. Determinants of Poverty)	Group/Self Valuation (Priority Rankings)	Confirming/ Refuting; Merging

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The first difference hinges on whether there is a common welfare or well-being measure across the domain of the comparison. A common measure or unit is required in order to make consistent interpersonal comparisons of well-being. The welfare metric used in the I/C approach, income, expenditure or some variant, allows for interpersonal comparisons of welfare because there is a common unit across the domain of the comparison, which implies a common welfare referent.[2] Findings from well-being rankings conducted in different communities do not facilitate interpersonal comparisons because there is no common well-being referent across the domain of the comparison. As a consequence, aggregating results from well-being rankings to arrive at a average across communities of 'poor' or 'worse-off' persons is misleading, *even* in the extremely unlikely event that the same well-being criteria is applied in all rankings (it is averaging across categories whose levels may be completely different). Further, and *contra* to what is often reported, it provides no basis to confirm or refute national (or regional) poverty incidence data culled from household surveys.

A second difference relates to the basis for extrapolating research results across a broader population. The I/C approach does this on the basis of statistical inference given known probabilities of household selection within a household survey. The PA approach may also use statistical inference if site selection allows for it and if statistical data is collected. At times, however, the key information collected in a PPA is not statistical nor should it be transformed into a statistic. The very point of doing a detailed village study is to come up with a detailed account of some social phenomenon (and not to count or scale it). In these cases, extrapolation cannot make use of statistical inference and must rely on empirical generalization. Empirical generalization requires establishing the typicality of research results over a broader population (Hammersley 1997). Establishing typicality is extremely difficult and, unlike statistical inference, there are no hard set rules about how to do it. One approach would be to construct detailed typologies of conditions which differ in important poverty-relevant ways, and pick villages which are typical of the selected types. An example from Myanmar illustrates this approach (Shaffer forthcoming). The research project in Myanmar combined household survey data (DHS-type) with data from a PPA conducted in twelve villages in the country. A central focus of the PPA was to get a better understanding of how villagers respond to downward pressures (coping strategies) and opportunities (enabling strategies).

The Myanmar PPA used a four stage process in an attempt to establish the typicality of village-based results. This entailed: 1) specifying the population characteristics in question to generalise; 2) identifying indicators of those characteristics; 3) selecting broad regions or areas which are *predominantly* characterised by these characteristics; 4) selecting a limited number of villages which are typical of these broad areas. More concretely, the primary focus of the study was on coping and enabling strategies of the poor. Variation in coping/enabling strategies is closely related to variation in livelihood strategies (point #1). The selected indicators much be those which *best* differentiate between major livelihood types in a given region (point #2). The actual indicators

[2] This basic point is not invalidated by the fact that interpersonal comparisons may be quite sensitive to the way that spatial price differences or behavioural differences affecting consumption choices are handled.

selected include land use type (Northern Rakhine State), access to water (Dry Zone), elevation and access to water (Shan State) and water type and land use (Delta Region) (point #3). Selection of particular villages within different land use categories requires particular knowledge that the village is typical of the broader category to which it belongs (point #4). One way to better inform typology construction would have been to use regression results on significant determinants of poverty in particular regions. In any event, the construction of detailed real world typologies as the basis for the generalisation of village-based studies has been underutilised in poverty analysis and is a promising area for future research.[3]

A third difference relates to the underlying conception or aspect of ill-being. In the I/C approach, it is non-fulfillment of basic preferences, represented by low levels of income or consumption expenditure. In the PA approach it is multidimensional and generated by participants in dialogue. These differences have implications for both confirming/refuting and merging results from the poverty approaches. In the first case, if results differ because the underlying conception of well-being differs there is no direct scope to use results from one approach to confirm/refute results of another. In the second case, results are insufficient to make, say, a resource allocation decision, because there is no clear way to determine their relative importance. Research results from the Republic of Guinea provide an example.

In Guinea, (SDA-Integrated) household survey data suggest that women are not more likely than men to be consumption poor or to suffer greater consumption poverty. The incidence, intensity and severity of poverty is higher in male-headed households than female-headed households. Sensitivity analysis using different adult equivalence scales and different poverty lines (stochastic dominance tests) affirms this result. In addition, both women and all females are underrepresented in poor and ultrapoor households. Further, almost no indicators of intrahousehold distribution of food or health care, (nutritional outcome and mortality indicators, aggregate female-male ratio) reveal that girls or women are worse off than men or boys.[4] PPA data from the village of Kamatiguia however, suggest that women as a group are worse off than men as a group. In focus group discussions, two dimensions of deprivation were singled out by men and women which disproportionately affect women: excessive work load and restricted decision-making authority. In group discussions, a substantial majority of men and women maintained that women were 'worse off' than men, and a larger majority held that in a second life they would prefer to be born male than female. Further, in well-being ranking exercises, groups of both men and women separately ranked *all but two* married village women below *all* male household heads in terms of their own criteria of well-being/ill-being. PPA results cannot serve to refute I/C results because differences relate to different underlying conceptions of ill-being (and not, say, sampling or non-sampling error).[5] Further, reliance on both sets of results to inform policy does not absolve policy

[3] For discussion of typology-based analysis see Diesing (1971).
[4] The one exception relates to infant mortality indicators when using "relative-difference" or model life table norms.
[5] They do indeed show that consumption poverty doesn't exhaust all aspects of ill-being, but the I/C approach nowhere maintains that it does.

makers of having to make a judgment about the relative importance of different conceptions of ill-being (and about the political constituencies likely to favour one set of ensuing policies over another).

A final difference relates to the process of determining policy priorities. The I/C approach may use various modeling exercises to determine the relative importance of 'determinants' of poverty (if a regression is given a causal interpretation and assumptions are made about spatial and temporal projectability). The PA approach uses priority ranking techniques which rank people's preferred courses of action which are then aggregated across communities. Here, there is no reason to expect that results will be the same as the two poverty approaches are doing different things (soliciting people's priority actions is different from inferring priorities from statistical determinants of poverty). There is no obvious case to use the results of one approach to confirm/refute results of the other. In addition, there are two potential problems when using aggregate data from priority rankings to inform national or regional level policy. First, aggregating priority ranking results from non-representative PPAs is likely to lead to bias. The reason is that people's priorities are conditional on the particular endowments of local communities. For example, health is unlikely to rank as a priority area in those communities with quality and accessible health services. If PPAs are not representative, then aggregating results will overstate the particular mix of endowments, and ensuing priority actions, of communities which happen to be selected. Second, aggregation tends to lose the most important informational content of priority rankings. That is, by aggregating across communities to arrive at regional or national averages we lose community-specific rankings, which is arguably the reason for conducting the ranking in the first place.[6] These issues should be explicitly addressed when merging results to inform policy.

III. CONCLUSION

The move to a more interdisciplinary approach to the analysis of poverty is long overdue. There are many fruitful ways to combine different approaches to the analysis of poverty. There are also many ways not to. This note has focused on the later set of issues with a view to better inform practice.

[6] This point doesn't apply to priority rankings which are not conducted for this purpose and/or when ranking results are very similar across many communities.

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QUALITATIVE AND QUANTITATIVE POVERTY APPRAISAL WORKSHOP: SOME REFLECTIONS AND RESPONSES

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According to the workshop programme I am among those asked to pull together responses to the question 'What is required to reduce tensions and increase complementarity?', at the end of the workshop. I anticipate that the workshop itself will provide many more ideas and answers to this question, which it will be our task to summarize. What follows here are therefore just some reactions to a couple of the questions raised in Ravi's preliminary note, which I want to contribute to the discussions at this stage.

I. TERMINOLOGY AND CONCEPTUAL CLARITY

Much confusion could be avoided if all practitioners were careful to clarify terms and concepts, as Ravi' note does at the outset. Much confusion could be avoided if we could reach a consensus that income poverty should be referred to as just that, and that when 'poverty' is discussed it means a lot more than that. Some would say that agreement has already been reached on this, but in practice 'poverty' is still often used to refer to income poverty.

II. WILL COMBINING QUALITATIVE TECHNIQUES WITH QUANTITATIVE TECHNIQUES SIMPLY MAKE THE FORMER MORE 'EXTRACTIVE'?

This notion seems to me to be based on one erroneous conflation and one piece of fiction, or perhaps wishful thinking. The erroneous conflation is of qualitative approaches with participatory approaches. The fiction is that participatory approaches are inherently non-extractive.

Qualitative approaches are not the same thing as participatory approaches. In any case, participatory approaches do not necessarily empower, and are not by definition non-extractive. Quantitative approaches are not necessarily, or uniformly, extractive. Much depends on how much attention practitioners of all approaches pay to self-critique and research ethics, to the personal conduct of researchers, and to the opportunity costs respondents incur through their involvement in the research.

So far, work on combining methods for poverty assessment seems to have focused on technical aspects, to the neglect of these matters. Given the complex technical issues arising in combining methods, there seems to be a tendency to seek the lowest common denominator in ethics and behaviour. It is in the interests of all those who ever rely on

the good will of people to answer researchers' questions – whether quantitative or qualitative in nature – to assume greater responsibility in this respect. More reflection is needed by all on what is in it for the respondents (whether 'it' is a questionnaire or an in-depth ethnographic case-study). Imagining oneself in the respondent's or participant's position provides a fresh perspective on how much it is fair to ask of her/him. That participatory research practitioners seem more willing to confront this uncomfortable question than quantitative practitioners, perhaps reflects the fact that their methods often demand more of respondents' time or greater access to their private lives. But in responding to the question, participatory practitioners have sometimes been guilty of hubris – of claiming or assuming scope for empowerment of respondents or participants where none actually exists. This is detrimental to participatory research as a whole, some instances of which are actually empowering; and is certainly unhelpful to the overall cause of avoiding extractive research practices.

Here too, much depends on context. Maybe poverty research – quantitative, qualitative or participatory - conducted by or for an institution like the World Bank, so distant from poor people and so unaccountable to them, is always going to be extractive, unless some dramatic changes occurs within the institution which alter the terms of engagement between the institution and the research's respondents or participants. In situations where poor people suffer the consequences of bad governance and unresponsive or discriminatory state institutions, it is surely less acceptable for researchers from the state statistics bureau to bother them with questionnaires than in situations where they feel institutions are working for their benefit. On the other hand, poverty assessment in the former sort of situation might contribute – albeit indirectly – to policy changes which are of more marginal benefit to poor people; for some this would make it ethically defensible. In situations where an NGO has invested considerable resources in a region it may be considered more ethical for its personnel to conduct research or PRA than in situations where the community has no prior relationship with the NGO. It is also inevitable that the prior relationship will affect the research encounter and its outcomes; but providing that approaches and researcher attitudes are deployed which address rather than ignoring or attempting to exclude issues of context, history, relationships and social and political environment, this is not necessarily detrimental to the research, or to the inhabitants.

Self-critical awareness and context are central to the question of what is ethical and what is not; there is no general rule. There should be some non-negotiables: a minimum one which most participatory practitioners agree on but some still fail to honour is the right of participants to have accessible, appropriate feedback on the findings and uses to which participatory research is put. I do not know of any codes of practice for quantitative research; but with increased interest in combining methods a matter which needs to be pushed up the agenda is the question of how to avoid the default mode of adopting the lowest common denominator in ethical terms.

III. STRATEGIES FOR COMBINING METHODS

Much has certainly been done to bring the two schools closer in last decade. This tendency has been aided by the recognition that each is good at different things which the other is not good at or cannot do at all. Simultaneously, our understanding of poverty has developed to a point where we recognise that without drawing on both approaches we cannot gain a comprehensive picture. If both are vital to obtaining a full picture, it follows that the two approaches should command equal status as poverty assessment approaches.

Yet 'equality in difference' remains elusive. The use of qualitative findings in mainstream poverty literature is still subject to questions – whether to use them at all, whether to foreground them or to play them down – which are not commonly asked about quantitative findings, the use of which is seen as 'natural'. When brought together, the two approaches rarely have similar status. Carvalho and White (1997), reviewing eleven World Bank Poverty Assessments, note that, while all combine quantitative and qualitative assessments to some extent, quantitative work is drawn on more systematically, and in a more visible way, than qualitative work (p 24). Whitehead and Lockwood's (1998) review of six African PPAs concluded that having PPA component did not contribute significantly to the policy messages contained therein.

Qualitative findings are easily invisible if only used alongside, and not integrated with, quantitative ones. This relative invisibility hinders recognition of the qualitative school's contributions to poverty analysis; and probably contributes to maintaining the inequality of status between the two approaches. Of the three strategies proposed by Carvalho and White, systematic 'integration', done throughout the poverty assessment initiative (not just, for example, at the final stage of deriving policy recommendations) seems to be the means of combination which affords the most equal status to both approaches and ensures that each contributes to outputs (policy recommendations or operational decisions).

Most demand for poverty assessment comes from policy-makers and institutions. Traditionally, they have generated more demand for quantitative than for qualitative approaches (Brannen 1992; Carvalho & White 1997). In many cases this is because of the macro-level at which policies are made and applied, which means that readily generalizable findings are often needed. But, although we already know some of the ways in which qualitative poverty information is useful for policy (e.g. by complementing survey data with information about intra-household distribution, vulnerability etc), this area is under-explored. One interesting contemporary challenge in this respect is the monitoring of Poverty Reduction Strategies, where qualitative approaches have important roles to play in assessing the policy measures used, the PRSP process itself (since the nature of the process is deemed important as well as the strategy's policy content), and wellbeing outcomes arising from both the process and the policy content.

IV. ARE THE DIFFERENCES JUST TO DO WITH DATA COLLECTION METHODOLOGIES?

To this question of Ravi's I would respond that in the example he cites, the difference is not to do with data collection methodologies, but with what data was being collected, and ultimately with the concept of poverty underpinning the investigation, which dictates what data is sought. Current quantitative assessment makes the assumption that higher expenditure means less 'poverty', and so attempts to trace changes over time by gathering data on expenditure. In fact the assumption is only about income poverty, and is questionable even so – does having more income mean one is less poor, no matter what that income is spent on? The case cited reveals that different findings result from different concepts of poverty: by gathering data on what people define as poverty (the absence of wellbeing, in a broader sense), qualitative assessment revealed that these expenditures decrease wellbeing (i.e. increase poverty as people define it).

Some of the difference is down to the nature of the research encounter: how much attention is paid to understanding context, or to trying systematically to exclude it. The 'contextualized'–'decontextualized' distinction is useful in focusing attention on this.

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WHAT IS REQUIRED TO REDUCE TENSIONS AND INCREASE COMPLEMENTARITY?

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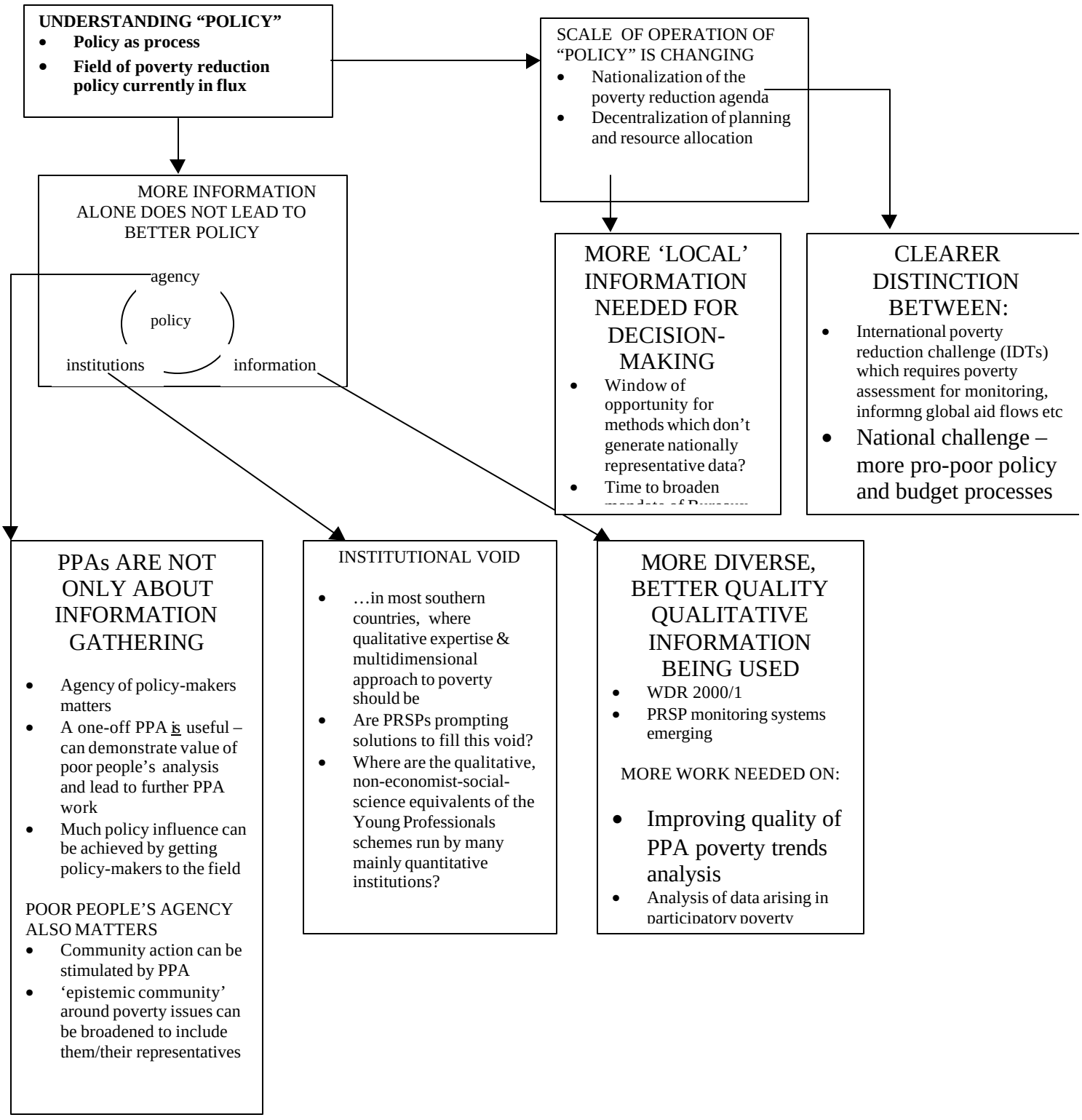
At this final session, in trying to sum up for myself and others, rather than focusing on what we have covered, I am focusing on some things that we have not covered explicitly but which are crucial issues underpinning many of the discussions we have had. My presentation is self-indulgent in centering on what participatory practitioners like myself can do to move the debate forward.

A common thread running implicitly through our discussions seems to have been policy. This is the main context in which poverty assessment occurs, the main source of demand for poverty data, whether qualitative or quantitative. The fact that I have pulled these perspectives out from all of our discussions no doubt reflects the fact that at IDS we are currently engaged in a research project on 'Getting it Right: Poverty Knowledge and Policy Process'. I want to present (mainly visually) a set of ideas about the way forward, centering on how we can sharpen poverty assessors' understanding of the policy process that our poverty information aims to influence. I would contend that addressing these points will:

- deepen understanding of what participatory approaches to poverty assessment can achieve and cannot achieve, thus reducing tensions arising from misunderstanding of their scope; and
- allow us to take cognisance of complementarity, not only in terms of potential for information generation but also of other kinds of potential certain methodological approaches offer .

(See flow diagram)


UNDERSTANDING "POLICY"
Policy as process
Field of poverty reduction policy currently in flux
SCALE OF OPERATION OF "POLICY" IS CHANGING
Nationalization of the poverty reduction agenda
Decentralization of planning and resource allocation
MORE INFORMATION ALONE DOES NOT LEAD TO BETTER POLICY
agency
policy
institutions
information
MORE 'LOCAL' INFORMATION NEEDED FOR DECISION-MAKING
Window of opportunity for methods which don't generate nationally representative data?
Time to broaden
CLEARER DISTINCTION BETWEEN:
International poverty reduction challenge (IDTs) which requires poverty assessment for monitoring, informng global aid flows etc
National challenge – more pro-poor policy and budget processes
PPAs ARE NOT ONLY ABOUT INFORMATION GATHERING
Agency of policy-makers matters
A one-off PPA is useful – can demonstrate value of poor people's analysis and lead to further PPA work
Much policy influence can be achieved by getting policy-makers to the field
POOR PEOPLE'S AGENCY ALSO MATTERS
Community action can be stimulated by PPA
'epistemic community' around poverty issues can be broadened to include them/their representatives
INSTITUTIONAL VOID
…in most southern countries, where qualitative expertise & multidimensional approach to poverty should be
Are PRSPs prompting solutions to fill this void?
Where are the qualitative, non-economist-social-science equivalents of the Young Professionals schemes run by many mainly quantitative institutions?
MORE DIVERSE, BETTER QUALITY QUALITATIVE INFORMATION BEING USED
WDR 2000/1
PRSP monitoring systems emerging
MORE WORK NEEDED ON:
Improving quality of PPA poverty trends analysis
Analysis of data arising in participatory poverty

DATA AS SOCIAL PRODUCT: PROBLEMS IN QUALITATIVE AND QUANTITATIVE WORK

Ron Herring
Cornell University

In response to Ravi Kanbur's call for a two-pager, and having the benefit of his slightly expansive definition of the genre, this piece will briefly sketch the terrain as I see it, building on his paper. My central point is that the qualitative/quantitative divide may be less important than methodological problems that arise in assessment of poverty or economic conditions in either tradition. These revolve around vexed issues of assessing the relationship between indicators and phenomena once data are recognized as products of social interactions and situated in a specific social matrix of production. At the point of production, some individual confronts another, both with interests and cognitive frames that affect the nature of the outcome. My arguments are based on my own experience with both forms of data.

I. THE INSTRUMENTAL NATURE OF KNOWLEDGE: RESPONSES AS BEHAVIORS

Social scientists, and even more natural scientists, tend to believe that knowledge for its own sake is a pure good. The truth has value. Ordinary people are not so committed to this view (nor are all scientists in the dark corners of their labs or computers). Anyone who has ever done field work has had to contend with the following questions, whether explicit or implicit:

How might answering this question hurt me? how might it help me?

When asked how my own questions might hurt respondents, I always responded: not at all. But the follow was often, "well, then how will answering your questions help me?" Seeking to be honest, I would say, probably not at all, though there is some chance that someone will read my report and it will make a difference. But the speaking-truth-to-power phenomenon is so pervasive that it would be dishonest to tell anyone that finding the right answers would have positive consequences of a certainty. The response of many rural people was then more or less: "well, if it's not going to help me, and cannot hurt me, why should I bother answering your questions? There is work to do, you know." This response underlines the ethical issue of extractive techniques; there is always a cost.

My experience is not unique: I look no more threatening or helpful than the next guy. If interviewers have not had this experience when looking into land ownership, tenancy arrangements, policy failures, and political allegiances, they may have lacked a certain ethnographic sensibility that would be good to impart to field researchers of whatever methodology.

The implication is that getting information always implies a social context: data are social products. Responses to questionnaires – of whatever format – are not "attitudes" or "facts" but rather markers of behaviors. Their relation to truth is variable, depending on their mode of production. We do not escape context by moving from one kind of instrument to another. One striking example I discovered early in my career was the question of the extent of tenancy in India, a question one would think easy to settle given the colonial obsession with land records and independent India's commitment to data collection for planning, poverty reduction, land reform, taxation, etc. It turns out that estimates of the extent of tenancy in India varied enormously. But all was not random: estimates varied directly with the length of time the investigators spent in the village.[1] Precisely because being listed as a tenant in any written document could help or hurt an individual, the questions were answered instrumentally. Land records were likewise imperfect reflections of ground reality, as their social context was decisive.[2] Looking at the land records yielded very little tenancy [landlords knew to bribe or intimidate *patwaris*]; ethnographic work in the same village yielded quite a bit of tenancy; the National Sample Surveys yielded a number in between. Ethnographies (the most intense form of contextual investigations) yielded a more true result, but emphasize the particularities of small communities. Ethnographic method does not solve the interest-mediation problem, but does indicate the importance of specification of the interests likely to be engaged by shorter-term work of broader sampling frame. Understanding this phenomenon should help to specify the trade-offs in choice of method.

Variables more important than instrument and method may therefore be interest and trust – the relationship between interviewer and subject. No one would think that either quantitative or qualitative instruments would yield accurate data on the numbers of drug dealers in a neighborhood: powerful interests are at play in the behavior of respondents. There is then a very difficult question involving the status of researcher and object of research. Without trust engendered by some relation between interviewer and object of study, why should anyone believe the answers? Why should strangers trust interviewers? If someone came to my house and wanted to come in and ask questions about my possessions and income, I'd send him/her packing with something about "none of your business." Some subjects want to please interviewers by saying what they think the interviewer wants to hear; others sequester information for various reasons. Interviewers are strongly interested in completion of forms when there is a conflict with accuracy. Poverty introduces special problems in this regard – compared, say, to agronomics. How does poverty become other people's business? What people? At what point does a participatory approach undermine claims to objectivity?

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[1] H. Laxminarayan and S.S. Tyagi, l977, *Economic and Political Weekly*, 12:22 (May 28) "Tenancy: Extent and Interstate Variation."

[2] One of the most dramatic moments in Vikram Seth's novelistic treatment in *A Suitable Boy* results from this reality: the disappearance of a sharecropper's rights. This point is well established in the ethnographic literature and is much a part of local knowledge that there are numerous proverbs in Indian languages to the effect that "above there is God, below there is the land-record keeper, implying no one else matters much [e.g. *Uper kartar, niche patwar.*] See Ronald J. Herring, Land to the Tiller (New Haven and Delhi, Yale and Oxford University Press, l983) Chapters 2-3.

II. COGNITIVE INCOHERENCE

Ravi Kanbur usefully asks us what we might learn about this problematic from commercial, including political polling, experience. My own experience with both is that the questions often seem not quite right. That is, whether one is using mass survey technologies or small-n methods, shared cognitive fields are a necessary condition. Experts in these fields do a lot of pretesting, but the available answers nevertheless usually seem to me either too rigid, too bald, too unsophisticated, or all wrong. These problems plagued the US Census on the issue of "race." The folks who design these questions do not live in the cognitive world I inhabit, and I would be equally lost trying to tease out complex responses from them, despite the fact that at some level we share a culture in the aggregate sense. Again, an ethnographic sensibility would seem essential to making sense of either numbers or open-ended responses.

III. WALKING ON BOTH LEGS?

More information is certainly better than less information and there is good reason to think iteration across methods would improve both approaches – given attention to sequencing. It would take a real ideologue to believe that nothing can be learned from rigorous surveys utilizing either method, assuming such instruments are sensitive to the interest/behavior question and have been pre-tested by people with a good head on their shoulders sharing some cognitive assumptions with subjects. [These are very strong conditions, however.] Once we recognize data as social product, we are then moved to triangulate among all the information available, sensitive to the mode of production of the data we are using. Triangulation would be especially valuable to avoid contaminating the international aggregate data pool. I have seen researchers use data from a rural credit survey in Pakistan, for example, that I know to be bogus: there were many areas of the country into which the questioners were afraid to go for reasons of personal safety but felt compelled to fill up the forms anyway; the same phenomenon plagues studies of US cities. There were also questioners in this survey who abandoned efforts to find the sampled households and made up answers on the basis of conversations at the local tea shop (which I dubbed the Chai-stall Error). When these issues were made apparent to the Government, they simply did not want to hear it; their interest was in getting it out. The survey was published as was, without cleaning operations. Here a little local knowledge and disaggregation would have gone a long way: it was clear that the aggregates made sense but the local numbers made no sense at all. Walking on both legs implies to me that we should make sure our international work engages area-studies expertise.

IV. NOISE AND ERROR TERMS

The concerns expressed above could be dismissed as insignificant in very large samples: errors will cancel out. There is no space to consider this problem systematically, but one point of this exercize should give pause: errors introduced by social relations of production of data may be systematic. Certainly this is the case for the land tenure issue discussed above. This phenomenon has been strikingly evident in errors in the US Census: somewhere between 4 and 7 million people were not counted in the recent round,

and such people are likely to share certain characteristics, minority ethnicity and poverty being prominent among them. There are important implications for distribution of dollars and representation in political institutions at play in interpreting the Census. The politics around this issue in the United States clearly indicate the social nature of data production and interpretation. It is contextual knowledge that helps bring such errors to light, but there are clearly trade-offs between quality of data and representativeness of samples.

V. IMPLICATIONS FOR BETTER WORK

First, grounded contextual expertise needs to be available for survey design, focus groups, pretests, working groups in the field and interpretation of results. Universities need to ensure a supply of such people and poverty researchers need to heed them. It is striking how often generic social science fails for want of attention to this simple desideratum. If data are social products, it is crucial to understand the society which produces them, specified from very micro to very macro levels. Second, understanding so complex a phenomenon as poverty requires iterative processes with different methodologies.[3] One small step that could be taken would be to send analysts of either form of data down to the field, to sit in on the process from start to finish, so that problems of interests, frames and interpretation could be apprehended as part of the analysis. Failing to understand the relations of power and interests that condition the production of either qualitative or quantitative data reduces the confidence that can be placed in the results. The assumption of frictionless epistemological surfaces often encountered in the genre is clearly unwarranted. Since policy often follows from perceptions of causality, distribution and character of poverty, there is an ethical imperative to take as many steps in this direction as possible.

[3] See the paper by Chris Barrett for this conference for a model. For a broader treatment, S. Carvalho and H. White, "Combining the Quantitative and Qualitative Approaches to Poverty Measurement and Analysis," World Bank Technical Paper Number 366 (Washington, DC, l997).

THE EMPLOYMENT PROBLEM IN SOUTH AFRICA: FROM COINTEGRATION TO MR. ISAACS

Gary S. Fields
Cornell University

I should like to use this workshop on quantitative and qualitative approaches to economic analysis to talk about my ongoing work on South Africa's employment problem. The two approaches have played an important role not only in research but also in policy.

In the summer of 1999, I first visited South Africa at the request of the South African government. The government was concerned about the nation's drastic unemployment situation, which in recent years has been estimated at 12-20% using the standard ILO definition (not working but actively looking for work) and which reached and as high as 34% when account is also taken of persons who did not work, did not look for work, but who reported themselves willing to take a job if one were offered. Government believed that unemployment was caused by excessively high wages – excessive, that is, relative to market-clearing levels – so they asked us to estimate, inter alia, the wage elasticity of demand for labor. Note the role of both types of research here: government's core hypothesis came both from talking to business-people, who claimed that high wages discouraged them from employing more workers, and from prior econometric estimates.

Our terms of reference, given to us by the government, was to estimate *the* wage elasticity of demand for labor. We painstakingly gathered time series data on the key variables (employment, wages, user cost of capital, and per capita national income), started with time series regressions, and then moved on to cointegration techniques. Alas, the series proved not to be cointegrated – an unhappy state of affairs for concluding anything about *the* elasticity of demand for labor. So perhaps the reason we found no cointegrating relationship is that the structure changed during the period.

Qualitative analysis came to the fore again. The problem, we were told, is that beginning around 1990 (a key date in South Africa: the release of Nelson Mandela from prison, marking the imminence of majority rule government), what they were calling in South Africa "the labor hassle factor" suddenly became a major issue. Labor demand fell, it was said, because of a shift to a new regime, under which it would be difficult to dismiss a worker if business conditions worsened or if the worker did not perform adequately.

So we looked for a structural break in the data in 1990, tested it econometrically, and voilà: there it was. The following figure shows the regression line fit to the eighties and nineties together and to the two decades separately:

Figures 1 and 2.

Relationship between Employment and Real Product Wage, 1980s and 1990s

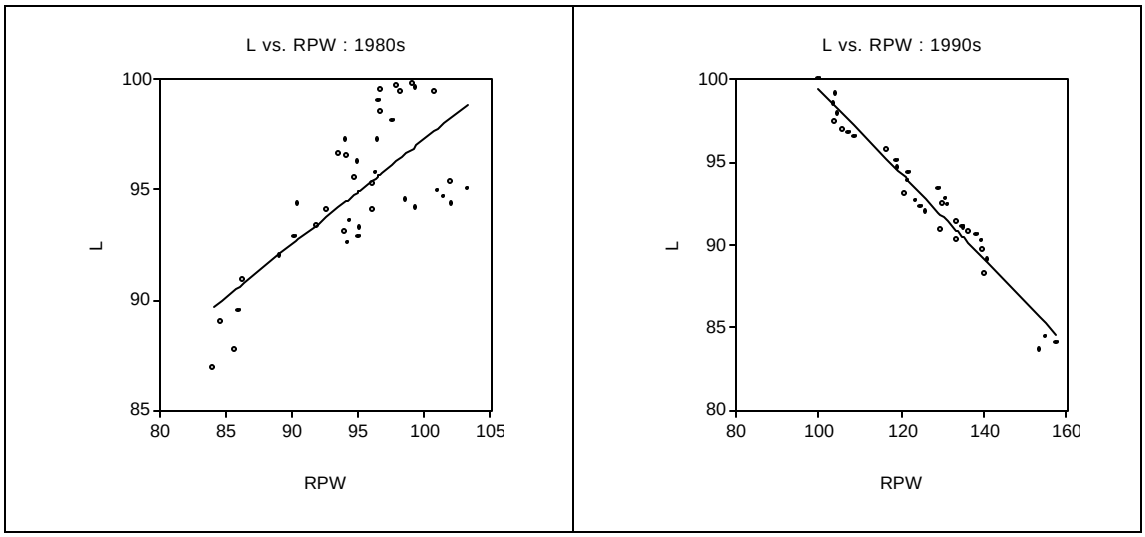
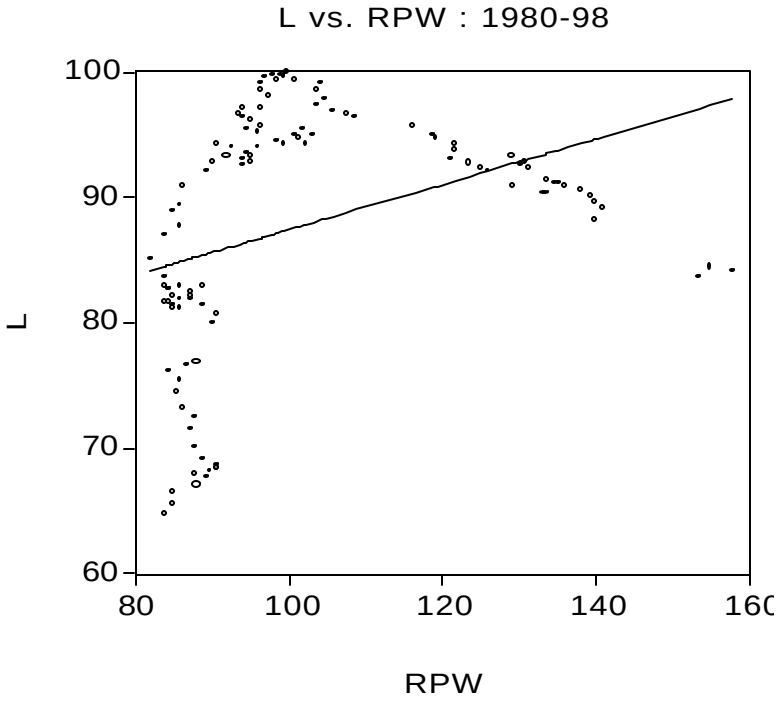


Figure 3.
Relationship between Employment and Real Product Wages, 1980s and 1990s Together



Source: Fields, Leibbrandt and Wakeford (2000).

No wonder we found a weak relationship between employment and wages: we had tried to fit a line to an inverted-V relationship! The line fit terribly, but look how close two separate regression lines fit.

What does this mean? We concluded that the nineties were indeed different from the eighties, but in a different way from what had been thought. In the eighties, real wages rose and employment did too. In the nineties, real wages continued to rise but employment fell. A neoclassical interpretation makes perfect sense: In the eighties, wages were being pulled up by supply and demand, which is why employment was rising, but in the nineties, wages were being pushed up by trade unions and other institutional forces, moving the economy along a negatively-sloped labor demand curve, driving employment down.

Of course, the wage-employment loci shown in Figures 1-3 are neither labor demand curves nor labor supply curves. Given our interest in labor demand elasticities, we therefore estimated labor demand equations as functions of real product wage, real user cost of capital, and real output, including an AR(1) term for autocorrelation. And indeed the estimated labor demand elasticities in the private sector were found to be quite different in different time periods: -0.11 for 1980-89, -0.35 for 1990-93, and –0.53 for 1994-98.

Our econometric analysis thus led us to the conclusion that **part** (but only part) of the reason for falling employment in South Africa was exogenously rising real wages. Another part was due to a rising wage elasticity of demand for labor. The policy implication of this finding was that the country faced a tradeoff between earnings levels of those employed and the number employed and that a national dialogue was needed to decide where the socially optimal point on the employment-earnings frontier would be for South Africa.

At the same time, qualitative research was undertaken on business problems in South Africa, and here is what it showed. According to a research study conducted jointly by the Greater Johannesburg Metropolitan Council and the World Bank, corporate CEOs in South Africa identify the leading constraints to business growth as crime and violence, labor regulations, interest rates, exchange rates, corruption in government, skills shortage, and tax rates. The leading priorities for remedying this situation, according to these CEOs, are for the national government to promote an efficient and flexible wage policy in the labor market, maintain macroeconomic policy stability, and promote an efficient and flexible interest rate policy. Note well: Labor regulations are number two on the list of problems and wage policy is number one on the list of remedies. Remember, though, that high wages and regulated labor markets are by no means the same thing.

Which brings us at last to the case of Mr. Isaacs. I met Mr. Isaacs a few months ago in a Rexall Pharmacy on the Camps Bay Road outside of Cape Town. In Mr. Isaacs' shop is a 1998 newspaper article lauding him for being in the Guinness Book of World Records as the oldest practicing pharmacist in the world: 93 years old. So we got to talking about his work, his business, and the larger South African society. Mr. Isaacs told me the following

story. For many years, he ran his pharmacy with the aid of an assistant. Thirty-two years ago, he said, he took her on for a month and she stayed with him for thirty-one years, until she got too old and sick and had to retire. Since then, he said, he runs the business all by himself. "Why," I asked, "do you do it all on your own?" and here is how he answered. "You see, Professor, if I take on an assistant in these times and that person doesn't work out, I can't dismiss him/her. I don't want to be stuck with somebody like that, so I'm doing business on my own." This from a 95 year old man in a country with 34% unemployment!

So why is unemployment in South Africa so tragically high? Above market-clearing wages? The labor hassle factor? Quantitative research reveals the importance of the first, qualitative research the importance of the second. Both are true. There can be no question that both types of research are needed.

If I do my homework before going to a country, I find that nine out of ten of my prior hypotheses are borne out, but it is the tenth one that makes the trip worthwhile. At the same time, I fear that we run a great risk if, once in the field, we start by asking people what problems they perceive and what should be done about them. Why? Because people respond to perceived private benefits and private costs, given their frame of reference and knowledge. However, policy must be based on social benefits and social costs, which usually diverge from the private ones.

So where do we start? For me, quantitative research is the most informative place to begin. If I had to do only one kind of research, that is what I would do, and I would do it as carefully as my skill with statistical and econometric methods would allow. But I don't have to do one kind of research, nor does anyone else, which is why it is good that we get away from our Stata commands and out in the field every once in a while.

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POVERTY RESEARCH: EXTRACTIVE OR EMPOWERING?

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Bretton Woods Project

> "Administrative man recognises that the world he perceives is a drastically simplified model of the buzzing, blooming confusion that constitutes the real world. He is content with the gross simplification because he believes that the real world is mostly empty - that most of the facts of the real world have no great relevance to any particular situation he is facing and that most significant chains of causes and consequences are short and simple." Herbert Simon.[1]

> "Policy can be influenced by participatory processes themselves, as well as by the information they generate." Rosemary McGee with Andy Norton.[2]

Among the questions which Ravi Kanbur's 2 pager for this workshop asked us to comment on are whether: "combining qualitative techniques with quantitative techniques … simply make the former more 'extractive'? Is a key aspect of qualitative techniques a philosophy of 'empowering' the poor and bringing them into the decision making process, and will the current trends towards combination threaten this?"

Some of the other 2 pagers I have had a chance to read imply that there are no fundamental issues outstanding, or philosophical differences to be overcome in the way that different researchers set about analysing poverty. I believe, however, that a number of fundamental questions DO remain about how people carry out research, and about the assumptions which underlie much theorising about poverty and what should be done about it. Here are some brief pointers to what I mean.

I. COMMUNITIES/RESEARCHER INTERACTION

Before explicitly addressing the competing methodologies in themselves I think we need to examine the roles of commissioning institutions, researchers and communities. In other words, to what extent does poverty analysis empower the people it studies? For me too much research appears to be framed to try to reach bold conclusions about the state of poverty and what should be done about it. It would be better if more was geared towards contributing to public debate,

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[1] Quoted in Seeing Like State, Why Certain Schemes to Improve the Human Condition Have Failed, J. Scott, 1998, Yale University, p 45.

[2] *Participation in Poverty Reduction Strategies, a Synthesis of Experience with Participatory* A*pproaches to Policy Design, Implementation and Monitoring*. Rosemary McGee and Andy Norton, IDS Working Paper 109, May, 2000, p.4, www.ids.ac.uk

presenting data and results for different audiences to chew over.[3] If research is viewed in this way, then it will force researchers to write more clearly, unpack their assumptions, spend more time with communities, and ensure that data is both validated by and also owned or co-owned by communities themselves. In other words view themselves less as experts and more as facilitators.

In many cases taking this view will also force those who commission research to ensure that they allow their staff or consultants to spend sufficient time with the communities they are studying, and that this relationship continues through all stages of the work. These points were well made in a recent statement by Manuel Fernández de Villegas, who works with Mexican NGO Trasparencia:

> "From our point of view, it would be worthwhile to propose something like a code of ethics for those social development professionals who work as consultants, involving the terms of their relationships with the communities and social organizations who are the target of their research.
>
> For example, it is very rare for the communities and organizations mentioned in the reports and diagnoses to receive copies of them, so that they are not even aware of what is said about them, much less have an opportunity to state their opinion about the findings or the way in which the results that are obtained during their (always brief) field visits. Consultants are giving priority to their source of temporary employment, over and above the interests and rights of the communities and social organizations that are trying to defend their autonomy and their right to participate in the government programs that are carried out in their area of influence – [already] a complicated and difficult challenge."[4]

II. THE EXTENSION OF ECONOMIC REDUCTIONISM

The criticisms from my Mexican civil society colleague above were levelled at anthropologists, scarcely the torch-bearers for quantitative methods or the most powerful body of specialists when it comes to making decisions on development. Economists' tend to work at a far greater remove from the people they are analysing and draw on scientific language and tools, using maths and often working with aggregate data. Economists are normally prepared to generalise far more than people trained in, for example, an area studies approach. To people with training in other social sciences, or without much formal academic training at all, the use of econometric

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[3] The World Bank categorises its Economic and Sector Work (the in-country research it commissions) in terms of intended audience and objective. The audience categories are government, bank, donor, public dissemination, while the objectives are: knowledge generation, problem-solving and public debate. A surprising number of reports are NOT intended for public dissemination and to foster public debate. I.e. in Bolivia in 1998 the following studies were categorised as non-public: Study on Nutrition, Food Security and Rural Water; Rural Participatory Investments: Impact Assessment; Study on Secondary and Higher Education, and Poverty Report. *Memorandum of the President of the International Development Association and the International Finance Corporation to the Executive Directors on a Country Assistance Strategy of the World Bank Group for the Republic of Bolivia*, World Bank, May 21, 1998, Annex D.

[4] *The Role of Anthropologists in World Bank activities: A wake-up call*, Trasparencia, SC, Oaxaca, Mexico, November, 2000 www.laneta.apc.org/trasparencia/

analyses (particularly cross-country regressions) to try to "prove" causal relationships is extremely mystifying and almost totally impenetrable. Many people see these exercises as attempts to "blind with science" people who see the world in other ways. If you see research as supposed to contribute to public debate (and by extension more democratic decision-making) rather than "solve" problems then such number-crunching leaves a lot to be desired.

It is now increasingly recognised, even by official agencies which formerly championed very reductionist approaches, that poverty has many dimensions (income, social, political, gender, environmental etc). This multi-dimensionality poses challenges for quantifiers. Some seem to have reacted by trying to quantify ever more aspects of life, so that they may then be plugged into orthodox models. One example is the construction of elaborate indices of 'social capital'. (Professor of Economics) Ben Fine, in his wide-ranging critique of the coherence and usefulness of the concept of social capital, argues that social capital is an aspect of "the colonisation of the non-economic by the economists. [This] is a severe setback to development studies, with key topics such as industrialisation, gender and ethnicity being heavily stripped of their empirical and intellectual traditions."[5]

The introduction of quantification into research procedures and decision-making practices is often supposed to make them more objective. It is also said that they lead to more rational and accountable decision-making. However, as sociologist Wendy Espeland has commented, referring to the use of different types of cost-benefit analysis to rationalise bureaucratic decisions: "quantification hides as well as constrains discretion. It privileges some forms of expertise at the expense of others. Those who perform the commensurating, those who fix the terms of what is being disaggregated and integrated, and those who evaluate the technical adequacy of it, do so at the expense of local, practical knowledge . . . . commensuration alters relations of authority in profound ways."[6] Taking peoples' perceptions of their social interactions and conjuring up a social capital index can be seen in the same way. Contingent valuation and similar techniques, which are used to bring more areas into a discussion of economic trade-offs which will not contradict efficiency principles, are seen by many not as aids to illuminate what are the best development options but as efforts to ignore or elide complex political discussions on property rights and deeply held beliefs. Thus such processes tend to be disempowering.

Overall I agree with the statement made by Heilbronner and Milberg: "Economics must come to regard itself as a discipline much more closely allied with the imprecise knowledge of political, psychological, and anthropological insights than with the precise scientific knowledge of the physical sciences. Indeed, the challenge may in fact require that economics come to recognize itself as a discipline that follows in the wake of sociology and politics rather than proudly leading the way for them."[7] If a better balance can be achieved between (dominant schools of) economics and other disciplines I think this would have a healthy effect on discussions of when it is appropriate to quantify what.

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[5] Social Capital versus Social Theory, Ben Fine, Routledge, 2000, p 169.

[6] *The Cost Benefit Analysis Dilemma: Strategies and Alternatives*. Yale University, October 1999. Summary of conference presentations and discussions, p 3.

[7] *The Crisis of Vision in Modern Economic Thought*, Robert Heilbronner and William Milberg, Cambridge University Press, 1995, p 126.

III. CONCLUDING POINTS

If we agree that the purpose of poverty research is to inform decisions to alleviate it, and that decisions should not just be taken by academic specialists and officials, then this implies that research should be rooted in communities. This means not just at the level of their participation in contributing their opinions and experience. But also prior to the study in helping to frame it as well as after the main research has been done to help discuss the results and what conclusions may be drawn from them, as well as have access to the data and reports so they can make direct use of them.[8] Highly technical analyses which rely on detailed algebra, complex charts or intimidating jargon are less likely to lead to good discussions with people on the ground, leaving them solely as the passive objects of study and of high-level speculation about their situation.

In terms of decision-making on policy interventions, methods like cost benefit analysis and multi-country regression analyses tend to produce incredulous reactions from many people as to how the figures were arrived at and interpreted. Using decision-facilitating tools such as multi-criteria analysis or scenario modelling (using transparent and configurable models) may be the best ways forward. These have potential both to ensure that people can really get their views across and also understand what is being done with them. Using such techniques more would also I believe help ensure that basic research is not distorted by the need to produce figures which can be punched into simplistic technical models or cost benefit analyses which appear to aid rational and apolitical decision-making but are in fact laden with assumptions and values.[9]

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[8] Organisations such as Community Information, Empowerment and Transparency (www.ciet.org) do much research in this way.

[9] See, for example, Multi-criteria mapping, Andrew Stirling, in *Valuing Nature, Economics, Ethics and the Environment*, John Foster (Ed.), Routledge, 1997.
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TENSIONS, COMPLEMENTARITIES AND POSSIBLE CONVERGENCE BETWEEN THE QUALITATIVE AND QUANTITATIVE APPROACHES TO POVERTY ASSESSMENT

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The workshop that we have just participated in proved to be extremely rich and fruitful in identifying and clarifying the major differences between the two approaches. An attempt is made in this note to highlight some of the major contrasting features of the two approaches (Qual and Quan, for short) as well as their relative strengths and weaknesses. The motivation throughout is to suggest what each approach can learn from the other and how bridges might be built to enhance the potential complementarity between them.

I. METHODOLOGY

Qual appears to follow a relatively more *inductive* approach than Quan. The "hands on" iterative interviewing technique generates hypotheses that can be formally and quantitatively tested by the more *deductive* Quan methodology that relies on econometric and statistical tools. These hypotheses might be either confirmed or rejected after having been subjected to quantitative testing. If the hypotheses are rejected or only weakly confirmed this information can be conveyed to practitioners of the Participatory Poverty Assessment (PPA) approach who could then try to generate new (modified) hypotheses to be subsequently tested by Quan researchers. This iterative process could lead to a productive dialogue between the two schools and the identification of a set of richer hypotheses.

II. MEASUREMENT

In general, Qual relies more on the use of categorical variables and an *ordinal* approach in its assessment of poverty. In contrast, Quan uses *cardinal* data in its measurement of poverty (such as the value of consumption per adult-equivalent). Another difference is that Qual typically generates its informational set from *primary data*, while Quan tends to rely more on *secondary data* such as large scale household income and expenditure surveys. This creates two types of potential problems. In the Qual case, attempts at quantification through the use of alternative scaling of categorical variables can affect the results in fundamentally different ways, including rank reversal. In the Quan case, it is well known that many *secondary* variables contain large margins of error (e.g. land area under cultivation, food output and consumption, population size). Many economists tend to accept secondary data uncritically and assume often incorrectly that these data sets are accurate and reliable. Improving the quality of secondary data would have high social

benefits but is not attractive to many researchers who are more interested in using sophisticated techniques.

III. MATCH BETWEEN RESEARCHERS' OBJECTIVES AND DATA

It would appear that, in general, this match is relatively better with the Qual approach. Typically, PPA researchers have a direct input in the formulation of the questionnaires used to generate the information required to address the questions they are exploring. Often, these same researchers are involved in the information gathering process in the field so that they can modify and clarify questionnaires after pre-testing.

On the other hand, the match between the researchers' objectives and data is often quite indirect in the Quan case – particularly as they depend on secondary data. For example, any attempt at constructing an household asset indicator in the rural areas based on the Demographic and Health Surveys would be grossly incomplete as land holdings are not reported.

IV. DEFINITION OF POVERTY: A MULTIDIMENSIONAL CONCEPT

There is unanimous agreement that poverty is an illusive, highly multidimensional concept. Amartya Sen's Capabilities and Functioning approach provides a good start at any attempt to capture poverty. Poverty can be thought of as consisting of a vector of dimensions and characteristics. However there is no general agreement on the set of dimensions to include in this vector and even less on the relative weights to assign to those dimensions. Francois Bourguignon's analogy of two mountaineers climbing the same mountain from two different sides and seeing very different things is descriptive. The first alpinist climbing the Mont Blanc from the northern side might conclude that it entailed a most strenuous effort while the alpinist climbing it from the southern side might find it an easy climb. Similarly, different definitions of poverty can lead to very different assessments of the magnitude and depth of poverty. Clearly *there is need for some degree of agreement among followers of the two schools*.

V. THE WALL BETWEEN QUAL AND QUAN IS FURTHER ACCENTUATED BY THE WALL AMONG DISCIPLINES

The Qual approach is largely influenced and draws most of its practitioners from such disciplines as anthropology and sociology while the Quan approach relies mostly on economics. Thus, improving the flow of communication among practitioners of the two schools requires as well a sustained effort to acquaint oneself with the basic principles of both sets of disciplines. As anyone who has ever been involved in any attempt at multi-(inter-)disciplinary research this is not an easy task. In any case, it would not be desirable to strive for nor expect full integration between Qual and Quan. Both have their place, strengths and weaknesses.

VI. CRUCIAL IMPORTANCE OF TAXONOMY IN BOTH APPROACHES

In the Quan case each socioeconomic model, reduced form equation or construct that is to be empirically tested is based on an underlying consistent classification scheme. In other words the conceptual variables researchers are focusing on must be measurable with closely corresponding empirical data sets. In a number of instances, the match is imperfect and poor proxy variables are substituted for the desired variable in multiple regression equations. In the Qual case, there is a risk that the researcher influences (partially predetermines) the responses through the preselection of questions and through being actively engaged in the interviewing process. This is somewhat akin to the Heisenberg principle in physics where the process of observing a phenomenon influences behavior.

VII. TRADE-OFF BETWEEN CONSISTENCY (NEEDED FOR GENERALIZATIONS AND COMPARISONS) AND SPECIFICITY

Quan uses two different methods to derive the poverty line. The first method is based on the Cost of Basic Needs(CBN) approach. A basket of food products yielding precisely the caloric threshold for the population is specified. This basket is kept constant and applied to all households nationally. The maintenance of a constant basket across different regional, ethnic and religious groups allows *welfare-consistent* comparisons to be made but is oblivious to the fact that different diets might, in fact, be consumed by those various groups. The second method is the Food-Energy Intake (FEI) approach that observes the diet actually consumed by those households around the minimum prescribed caloric threshold in specific settings. Thus the poverty line in the FEI method reflects the actual tastes, preferences of and relative prices faced by households in a specific setting and can be said to satisfy the *specificity criterion*. In short the CBN method satisfies the *consistency criterion* but not the *specificity* one and vice versa for the FEI method.

In general, Qual tends to be highly *context-specific*. Researchers and interviewers focus on specific villages and communities and obtain a mass of useful and comprehensive information on the socioeconomic structure of each village studied. The use of different methods and orientations makes it difficult to arrive at valid comparisons across settings.

VIII. TWO SUGGESTIONS FOR IMPROVING THE INTEGRATION BETWEEN QUAL AND QUAN

1. Creation of Mixed Qual/Quan Teams to Address Specific Poverty Issues

The interaction among members of mixed teams would help to reduce the inter-methodological divide and improve the interdisciplinary communication. It would also contribute to a joint and iterative process of hypotheses-generation and testing. One possible fruitful topic on which a mixed team could concentrate on is the *dynamics of poverty.* A good example of what appears to be a successful multidisciplinary team is that described in Chris Barrett's note.

2. Multiplex Surveys and Panel Data Focusing on the Same Households

A comprehensive multiplex survey combining Living Standard Measurement Surveys, Income and Expenditure Surveys, Demographic and Health Surveys and Participatory Poverty Assessments covering the same households would provide information on a variety of dimensions of poverty for the same households. A key issue is whether it is possible to identify (formulate) one sample frame and design consistent with the various data requirements above? More generally, it appears essential to achieve a better match between the sample design and the research questions explored. This calls for closer communication and a better dialogue between social scientists and statisticians at an early stage of research.

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