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Household Histories and Methodological Triangulation

Jo Helle-Valle^{a,b} and Axel Borchgrevink^a

^a*Department of Development Studies and Interpreting, Oslo and Akershus University College (HIOA), Oslo, Norway;* ^b*AAAS, Harvard University, Cambridge, MA, USA*

Abstract The article uses the experience from the impact evaluation of a development programme to argue two points: first, the collection of *household histories* is a useful methodological tool, and, second, their analytical value is further enhanced by combining them with quantitative data. After giving examples of household histories, we first discuss what we call the ‘practical–methodological aspects’ of this approach, by presenting theories and perspectives on the household, the household development cycle and on the use of life and family histories as methods of data collection. Thereafter, we move to analytical–methodological aspects, where we argue that household histories are case studies, and discuss the insights that they may yield. We contend that case studies’ potential for generalization is grossly underrated but nevertheless acknowledge the serious analytical challenges associated with such a strategy and argue that generalizations must take the form of ‘social mechanisms’, i.e. as ‘tendential causality’ rather than general laws or propositions. Finally, we argue for the combination of qualitative case studies with quantitative data, demonstrating how the household histories analytically enrich survey data, and vice versa.

Keywords: household history; microfinance; impact evaluation; case study; social mechanism; triangulation

1. Introduction

‘This is what really shows us how the program works!’, Girmay,¹ one of the assistants working on our study enthusiastically exclaimed. Several of his colleagues assented. They told us that the household histories they had been collecting gave them a much better understanding of how the microcredit programme we were investigating actually functioned. We had just returned to Ethiopia for the final phase of data collection, and were meeting with them to learn how the interviews and survey they had conducted in our absence had worked out and what preliminary findings there were. We were in Tigray, Northern Ethiopia, for the purpose of doing an impact evaluation of the Dedebit Credit and Savings Institution (DECSI), a large microcredit

¹Names have been changed.

programme.² In the period since our last visit, the team of eight assistants had been busy working in selected localities throughout the region. While most of their time had been spent on the survey and the procedures for selecting a stratified sample of respondents, they had also conducted qualitative ‘household credit history interviews’.

These interviews were made among a smaller sample of households, and conducted according to interview guides with questions on the household’s general history (when it was formed, how it had developed in terms of membership, etc.), its assets (agricultural and otherwise) and how they had grown or diminished throughout the household’s lifetime, as well as its history of borrowing (whether from money lenders, relatives, the DECSI programme or otherwise) and what the results had been of the borrowing (developing successful economic activities, surviving a crisis period, ending in a debt trap, etc.). We had not given any strict methodological instructions on who to select for these interviews, apart from the requirement for a minimum number of women in the sample. Rather, they were asked to look for ‘interesting cases’: particularly successful or unsuccessful borrowers, people who appeared to be outspoken and willing to tell their stories fully. Our assistants were highly qualified: all of them had higher education and were working in state institutions, and many of them had previous experience of doing surveys. They had dutifully carried out this task, and in the process become ardent supporters of this methodology.

This article addresses this methodological approach, which we refer to as ‘household histories’, and discusses how they can be combined with data from other approaches. We analyse the merits and weaknesses of the approach by distinguishing between what we call a practical–methodological and an analytical–methodological aspect. The practical side refers to the craftsmanship – the ways in which data are collected in a particular social context. The analytical aspect refers to the epistemological and ontological premises and consequences of applying a method. We hold that the latter aspect needs to be incorporated in discussions of how to choose between and apply methodologies for several reasons – the most important being that we need to be clear about what kind of data we are actually generating through different methods so that we do not draw invalid analytical conclusions. An important dimension here is how additional insights can be gained by combining qualitative material with quantitative data.

We argue that household histories are a useful methodological tool in contexts where households are the unit of production as well as of consumption, as is the case in many peasant societies. The emphasis on histories gives a diachronic perspective that affords change a central place, a crucial aspect for development studies. Household histories can be understood as a form of case studies, and we discuss

²‘We’ refer to the consultants hired to do the impact evaluation, and include the Ethiopian economist Tassew Woldehanna in addition to the two authors of this article. The evaluation was commissioned by the NGO the Norwegian People’s Aid, one of the major donors to the DECSI programme. See Borchgrevink et al. (2003).

methodological advantages of the approach. However, we also point to the weaknesses it has in terms of representation and the ability to generalize. Thus, we conclude that in order to maximize the insights yielded by household histories, they need to be combined with quantitative data showing the extent of the various phenomena and cases presented. The DECSI study is an example of such triangulation of methods.

The article is divided into three parts. First, we present our study in more detail, principally by reproducing excerpts from some of the household histories collected, to show the type of data they yield. The second part deals with the practical–methodological aspects through a discussion of using households as our main unit of analysis. We draw on lessons from studies of the household development cycle and from the collection and analysis of family histories, as well as on the more common approach of using life histories. The third part deals with analytical–methodological issues. A household history can be understood as a form of case study, we argue, and consequently the debates on the merits and challenges of case study methodology are relevant for our purpose. This takes the discussion to an epistemological level, and we ask what kinds of explanations social science can provide, and what case studies such as household histories have to offer in this context.

2. Tigrayan household histories

DECSI is a huge microcredit programme in Tigray. In the years previous to our study, it had extended between 150,000 and 200,000 loans yearly. As is common in microfinance, lenders are organized in groups that are collectively responsible for the loans. This implies a strong group pressure for repayment and permits loans to be given to poor people without collateral. Loans are given for productive purposes, based on the assessments of community committees. Terms and loan periods differ according to the purposes of the loans: agricultural loans are repaid in full after harvest, while loans for other types of businesses have regular partial down payments. DECSI had shown impressive results in terms of repayment and overall institutional sustainability, especially given the very difficult ecological and economic situation of Tigray. Our task was to investigate the impacts of this programme on the clients and their communities. Data collection was divided into three phases. During the first we developed the methodology, did pilot interviews and tested questionnaires, and trained and practised with our assistants. We had one week for this part. The second phase consisted of the survey and qualitative interviews conducted by the assistants in our absence, which was carried out over four weeks. Before the third phase, the questionnaire data were processed and preliminary analysis carried out. In the third phase, we returned for two weeks of further qualitative interviews, partly guided by the preliminary findings from the survey. While the majority of our 50–60 household histories – including the 3 presented in this article – were collected by the assistants, we should also emphasize that 10–20 in-depth interviews were carried out by ourselves.

The following are excerpts from a few of the household credit histories we collected. While reading them, a few points should be kept in mind. Firstly, the stories reproduced here have been collected, translated and paraphrased by the assistants we employed. Basically, they have constructed the narratives, though based on the information, chronology and tone of the interviews. Thus, in a literal sense, they are the authors of this section.³ Their crucial mediating role does of course imply methodological challenges, which space does not allow us to discuss here.⁴ Their texts are reproduced directly, as they were submitted to us. Secondly, Tigray is one of the poorest areas in the world, where many households are dependent on relief throughout parts of most years. The stories of difficulties faced and years of crisis told by those interviewed should be understood in this context. And thirdly, the stories have been collected and written down within the overall project of evaluating a credit programme. The questions asked and the information highlighted are structured by that overall purpose.

2.1. Household #1 Berhane Gebrehiwot (male household head and DECSI client)

Berhane Gebrehiwot is DECSI client who is currently residing in Adigolo Tabia, Ofla Woreda. His household was established sometime in 1977 E.C. [Ethiopian Calendar] and used to live in town. The main source of income for the first years was relief food aid. Later on he started to engage himself in petty trade. But life did not become easy for Berhane. He could not afford the high cost of housing, rent and other basic necessities. Realizing this he and his family decided to leave town for good and started living in Adineba, one of the kushets (villages) of Adigolo Tabia.

Unfortunately, he could not get land of his own in the kushet. The only option he had at that time was to rent in land and oxen in exchange for labor and share of the produce. In this way he lead his life for some time.

In 1988 he came with the idea of re-engaging himself in petty trade. He borrowed money from individual money lenders at a rate of 10 per cent per month (sometimes per week). He bought kerosene from Koren – the nearby town – and retailed it in the kushet. As kerosene lamp was the main source of light for the kushet and Berhane was the only supplier of this product to the locality, he secured a good size of the market in the area. Meanwhile, DECSI started operation in the locality.

...

³We have no way of linking the histories directly to the individual assistant who wrote them down. But we are very happy to acknowledge all eight assistants as important contributors to the original evaluation report as well as to this article: Sintayoh Fissaha, Dr Woldegebriel Abreha, Edris Negus, Abreha Mehari, Getahun Teka, Haileselasie Wores, Melaku Gebremichael, Habtu Lemma.

⁴Borchgrevink (2003) discusses dilemmas of interpreter use, which covers some aspects of the challenges inherent in using assistants in qualitative data collection.

Berhane applied for 2,500 Birr and get the approval of the loan committee. He utilized some part of the loan for the purchase of barrel which he uses it as a container to transport kerosene from town to the kushet, and the remaining part to buy the kerosene. In the same year he also took additional two short-term loans (2,500 Birr each). He paid his debt on time and managed to save some amount of money.

He borrowed the same amount of money (2,500 Birr) in the consecutive year. This time he hand over the kerosene business to his wife and children and launched another business in the textile sector. He bought used clothes from Alamata (a neighbouring town) and sold it in another neighbouring town. For the third time, Berhane borrowed another 2,500 Birr from DECSI. This loan was utilized to buy sewing machines and rent in land. The sewing machines were intended to be utilized by his children, but for lack of experience in the field, his children could not operate them effectively and thereby generate income from the service. At this time repayment became a bit difficult since the investment did not start to generate income to the household. But somehow he managed to pay his debt on time from his past savings and from the sale of a tree he planted on his garden and the sale of crops he produced on leased-in land.

He then decided to train his children and took a fourth loan. He recruited a professional tailor from Sekota (a town found in Amhara region) at a monthly salary of 300 Birr for two months and brought him to the kushet where Berhane is living. His children got the required skill and now they are rendering the service to the community. He used the remaining amount to finance the working capital requirement of the kerosene trade. For the fifth time, Berhane borrowed another 2,500 Birr and now he is utilizing this loan to buy textile and apparel from distant areas and sell it in another distant town.

Berhane is now managing a diversified business activities and a farming. In the textile business, he travels as far as Dessie (some 250 km away from this locality) to buy at a cheaper price and to Sekota where he can get better price for his commodities. His children are also contributing income to the household by producing tailor-made cloths during their spare time.

Berhane now claims that his living standard has dramatically changed for the better since he left town and started living in the rural area. He has now acquired durable 'luxury' household assets and a better housing standard. His children are learning and dressing well. He also claims that he was awarded 350 Birr by the Woreda Council for his success story. He attributes DECSI credit as the main contributing factor for his achievements.

2.2. *Household #2 Giday Gebremedhin (female household head and ex-client), Mizan, Asgede Tsimbla*

Giday Gebremedhin is a 56 years old lady who was born and grown up there in Adi-gebru kushet, Mizan Tabia . . . Her father was working in the then highway construction, which passed across the small aggregate of houses Adi-gebru where Giday was born. She got married in 1967 while she was 12 years old, which lasted in less than a year. Later

after about 5 years she got three children (a son and two daughters) from different fathers and remained female headed then afterwards.

Giday believes that although sharing child care with a husband is advantageous she couldn't dare to accept the offer from father of her kids to go to other places leaving her birth-place Adi-gebru. She now regrets for her decision because she thinks it is a bad lesson even to her daughters, which are now in Endabaguna for schooling.

Childcare was the most challenging thing in her lifetime. She couldn't involve fully in farming due to lack of labour nor has any close relative to help her, therefore she shifted to trading of 'goshu' a main ingredient of local beer, and sometimes [to] beer processing leaving her small land or share cropping. The amount she is getting from her land is less than half a quintal which is negligible to meet her food demand, she is buying almost her food requirement from the income in the non-farm activities.

Giday's participation in the community was active earlier which seems decreasing since the last couple of years. She was an elected chairwoman of the women's association of Mizan tabia in 1989 where she came to know about DECSI. She was mobilizing the women under her association to take loan to improve their lives but she became a client [first] in 1990. The reason why she didn't took loan in the first round was she afraid she wouldn't have enough time to run her business and repay on time as she was spending most of her time for the association.

...

They [her credit group] had no repayment problem in the first round, however, in the second round loan in 1991 repayment, her friend and one of the villagers were not able to pay on time, thus Giday and the other two group members should have shared the penalty for the group. Atsede feels that the penalty was valid from the DECSI side but it was painful for her to share the responsibility of others fault.

Giday although she appreciates the importance of DECSI loan she didn't took loan the next year in 1992 in search of another reliable group. In 1993 she joined another group ...

Overall she took loan three times with an amount of 700, 300, 3000 in 1990, 1991 and 1993 respectively and she spent the first and second loan for the promised objective⁵ (brewery and petty trading) but the third loan was used for construction of house and schooling of her children living in Endabaguna 17 km away from Adi-gebru.

Giday repeatedly mentioned that she owned her house that made her free from house rent and unpleasant face of the owner every month, now the only assignment is to think about food for children away from her for schooling. She underlines that she manages to own her house due to in part from the profit in the first and second loan and the rest from half of the third loan. Now her first and second children are helping her in income generating activities such as selling tea during the school break that enabled her to repay the third loan which was settled on time.

⁵i.e. the one stated on the loan application.

She exited from DECSI loan because of default of her group members. She observed some of the members in her group were taking loan from private money lender at the rate of 5% per day to settle the DECSI loan at the time of repayment and take next day another loan from DECSI to repay the money lender. This is a bad lesson for Giday and left the group and DECSI until individual loan is allowed or she finds a better group, however she still believes that DECSI loan is by far better than any other alternative.

2.3. Household #3 Letensea Woldegabriel (female DECSI client), Adiha Tabia, Workamba

Letensea is an age of 41, married, with six children. She is 4th grade [graduate] and working as tabia chairwoman of Women's Association. Of the six children, five now students, having grade ranges of 3rd–10th.

...

Being a community organizer, she got training from DECSI and was actively involved in establishment of groups. While doing this assignment, one time one group member from the total 10 established groups asked her for [why she was] not becoming a member if she believed that the credit program is beneficial. It was a challenge for her, because if she did not become a member the whole groups will collapse. To solve such problem, she decided to discuss with her husband and make clear about the consequences to come the groups established will fail. At the same time, her husband was not experienced with such new program. After having repeated discussion, her husband agrees to take a loan for goat production. The labour problem (because the children are all students or not of the age to look after the goat) [was solved] by encouraging the small children to look after the goats till the students returned from school.

...

The loan history she experienced was very successful (at her household level) she said. The initial loan size was in fact small and not based on proper plan. As a result, the amount she borrowed was sometimes not sufficient, and forced to mobilize from her own source. The initial money borrowed was used for goat production as the household was not having goats. Initially, six goats were bought and now reached 30 goats. The loan repayment was managed from income earned by selling vegetables. Apart from the 30 goats available at present, she slaughtered and sold about 15 goats and four goats were eaten by wild animals.

In her experience, it was in 1992 [E.C.] that she failed to repay the loan size of 400 Birr. However, she managed it by taking loan from her relatives (with no interest), instead of selling the goats for repayment. And, the remittance she got from her son (soldier) was used for the loan she took from her relatives.

Except for agricultural inputs seeds (from seed bank) and food grain (from relatives), her household has not experienced credit from other sources before DECSI program.

However, after becoming client of DECSI, she became client of Women's Association of Tigray credit program. The reason for joining this program was the presence of insignificant interest rate and extended (more than one year) loan repayment period. She bought one goat, and that goat [has] become now 5 heads; and given as dowry to her ex-soldier son.

Her participation in the credit program has also helped her to know about money lenders. She observed that those borrowers who failed to repay their loan [to DECSI were] forced to borrow from money lenders at 5% interest rate a day.

Narratives such as these show how the credit programme interacts with the life and development of the participating households. Although they are relatively short stories, in the context of this evaluation they afford a contextualized and 'thick' description of how the credit programme actually functions in real life, for real persons. Moreover, they contain much information that might be seen as not directly useful for our analysis of the credit programme. But in our view, this richness is part of their value, as they yield insights into the social situation in which the programme operates. And beyond this, they also demonstrate some of the social mechanisms at work in this particular social context, and how they may serve to promote unexpected outcomes. In part, this was the reason for the enthusiasm of our assistants. For instance, they told us, various credit histories revealed that what from the outside appeared to be a solid borrower who had taken several DECSI loans and always repaid them on time was actually a household caught in a downward debt spiral. What this referred to was what we would in the report call 'the moneylender cycle' (Borchgrevink et al., 2003). The objective of any microcredit programme is to provide loans to poor people at affordable interest rates. As the poor do not have any collateral, and generally seek small loans, they are not of interest to normal banks. Thus, they are dependent on moneylenders who normally charge exorbitant interest rates on their loans. The microcredit programme aims to break this dependence and introduce affordable credit that may stimulate economic activities – and thereby development and growth. The 'moneylender cycle' subverts this objective. It refers to cases where a borrower is unable to repay his or her loan on time, and, in order to avoid the penalties associated with non-repayment, takes another loan with the local moneylender and uses this for repayment. As soon as possible – often after a month or so – a new DECSI loan is taken and used to repay the moneylender. Consequently, the borrower remains trapped in debt. Moreover, even though the moneylender loans are of short duration, the moneylender knows that the borrower is in a desperate position and uses the opportunity to set the interest rate even higher than his normal rates. Thus, the borrower is caught in a permanent debt squeeze where any surplus he or she is able to make is siphoned off to pay interest. It is this moneylender cycle which is referred to in both of the last two household histories reproduced above, where in each case the women interviewed report interest rates of 5 per cent per day charged by moneylenders under such circumstances.

This, the assistants maintained, was a form of insight into how the programme worked that was not available through the quantitative survey data. And it was not the only one. As the stories reproduced above show, this qualitative material also contained various success stories, which revealed ways in which a loan could enable a poor household to start new income-generating activities that greatly improved their economic situation.

The household credit history interviews thus allowed us to identify social mechanisms of fundamental importance for the programme's ability to reach its objectives. Even though our carefully designed survey questionnaire contained a large number of questions on credit use, economic activities and outcomes, the responses to its closed questions would not have allowed us to understand these mechanisms. At the same time, the quantitative data allowed us to establish correlations that showed us how the credit programme was able to reach different groups and what effects it had on them. A striking finding was for instance that the group that showed the highest rate of improvement through participating in the programme was among poor female-headed households. This was surprising in itself and something we could never have deduced from the household histories. On the other hand, once we knew of this correlation, we could use the detailed descriptions of the development trajectories of the different households to search for explanations for this finding, for instance in the story of Atsede above, who used the DECSI credit to get started in petty trading and beer brewing, as well as in order to construct her own house. Furthermore, the insights from both the quantitative and the qualitative material collected by the assistants raised questions and indicated areas for follow-up, to which we gave priority in the final phase of data collection, when we – the three lead researchers of the study – did qualitative interviews with individuals and focus groups in several of the localities selected for the study.

In sum, then, the lesson from this experience was two-fold: firstly, it showed the usefulness of household histories as a methodological tool. While there is an existing literature on the use of life histories in social science research, much less has been written on the use of household histories. Secondly, while quantitative and qualitative methods are logically contrasted against one another, they should not be seen as in opposition in actual scientific practice. Rather, they can be mutually supportive, and synergies arise from combining them (Denzin, 1989; Johnson et al., 2007; Teddlie and Tashakkori, 2009). While this latter point ought to be self-evident, it is obscured by the way in which many texts present quantitative and qualitative data as opposites. In the following we will discuss these two methodological lessons.

The investigation we use to exemplify our methodological discussion is a commissioned study carried out under restrictions common to many consultancies. This means that much of the data collection was carried out under less-than-ideal circumstances. Being dependent on assistants to collect most household histories, and the limited time at our disposal to build trust and carry out the interviews mean that the histories we have been able to collect are rough, without elaborate details, and liable to

contain inaccuracies. In this article, we have chosen not to discuss these weaknesses, but rather to focus on what our approach can illustrate about the method of household histories. As we have argued elsewhere, consultancies and commissioned studies are in principle subject to the same methodological requirements as basic research (Borchgrevink and Helle-Valle, 2004). It follows from that conclusion that discussions on the methods used in a commissioned study such as the one discussed is of relevance for all kinds of research, whether applied or basic.

3. Practical–methodological aspects: household as the unit of analysis

3.1. *Households*

The DECSI programme clients are individuals. Yet we collected household histories rather than the life histories of individual loan takers. This approach merits some reflection. The focus on households, we should point out, was not limited to the collection of loan histories. Rather, the household was the unit of analysis for the study as such. Our key questions concerned the extent to which households' economic status had improved or deteriorated, rather than on the successes or failures of individuals. This was a conscious methodological decision, based on our knowledge of the common forms of economic organization in rural Tigray. As is the case in many peasant societies, the household is both the unit of production and consumption. The household's productive assets – land, labour, tools and animals – are generally managed as a unit (even though there may be clear notions of which of the household members owns particular plots of land, depending on who brought them into the household through inheritance or otherwise). And there is a pooling of the produce of the household, so that also non-productive household members such as small children share in its consumption. Therefore, to understand economic choices related to the use of credit, and to assess whether the use of the DECSI programme actually contributed to the building of assets and reduction of vulnerabilities, it made more sense to focus on the household as a whole than on the individual loan takers. It is within this perspective that the collection of household histories becomes the logical choice.⁶

Now, there is always a danger of reifying the analytical concepts that one chooses to use. In our case it is important to bear in mind throughout the analytical work that who the household unit itself consists of is often not unequivocally evident. Households, as socio-economic and emotional units, respond to changing circumstances in various

⁶By extension, household histories could be a useful approach for studying other issues for which the household is the key unit. This would comprise issues related to production in peasant societies, to consumption in a wide range of societies, as well as to issues of socialization. A variant of the latter type of study is for instance exemplified by Stroschein's analysis of child mental health in relation to household income history (2005) or Alvi et al.'s study of the family history of homeless youth (Alvi et al., 2010).

ways that sometimes makes it difficult to ascertain who belongs to the household. In the practical methodological work, we found – as all field workers do – that those interviewed in the household were not always clear on who belonged to the household and who did not. For one, more or less permanent adoptions raise the question of whether and when the ‘adopted’ should be considered part of the household. The same is the case for other persons who only have a peripheral relationship to the household. Conversely, some people may be gradually becoming more and more distant. Typically it might be young adults who are not married but have migrated out of the local community and who send remittances back to the household. In many instances such persons are indisputably household members because they remit to and visit the household regularly. However, we often find that the link to the household becomes gradually weaker – i.e. the visits become rarer and the remittances fewer. Thus, at what point in time the person is no longer considered to be a member is not clear. And, more importantly, the researcher’s decision about the person’s membership status at the moment of the interview is problematic. The reason for this being a problem is first and foremost that the analytical framework often requires an either-or-answer. This is typically a requirement in quantitative methods because one needs to tick off the household according to pre-defined criteria (e.g. ‘who belongs to this household?’). This analytical and methodological challenge is thus another reason why household histories are important; one gets not only the opportunity to answer such questions with greater certainty but, more importantly, to get answers to *why* this is a difficult question to answer. Thus, through the typical dialogical interviews one conducts in relation to collecting household histories, we will most likely find out what dynamics and processes that lie behind this being a tricky question.

As with any choice of perspective, it means that some dimensions are foregrounded, while others become less visible. By seeing the household as the unit of economic decision-making, production and consumption, it means that to a certain extent, internal power differences, economic transactions and inequalities within households are ‘black-boxed’, or become invisible. That this danger exists is of course no new insight. In the 1970s it was a key point in feminist critiques of traditional approaches to the study of households and families (Chant and McIlwaine, 2009, pp. 237–254; Kabeer, 1994; Momsen, 2004). Still, it is a point to be aware of. Particularly so when one is evaluating a programme with the specific aim of promoting the position of women, as we were. However, that the analysis focuses on the household as the relevant unit for economic decisions does not mean that one cannot at the same time be aware of and open to the possibility of internal inequalities and conflicts of interest. In fact, most of the interviews conducted revealed internal dynamics of the households; conflicts as well as various forms of cooperation. For instance, in one interview in which the authors of this article participated, a woman told us a sad story: she had been a member of a credit group and taken a loan. However, her husband had taken the money and abandoned her and the children, leaving the village, perhaps to start a new life in the urban area. She was left with a debt she could not repay and the sole

responsibility of providing for the family, with few assets to draw on. While the quantitative survey approach was unable to discern such dynamics internal to the households, our qualitative methods were much more open to seeing also this aspect, even if we had at the outset taken the methodological decision to use the household as the relevant unit for the study.

Another challenge to the approach is that the focus on households may not be equally relevant in all cases. The decision was made on the basis of our existing knowledge of the organization of households in the rural area of Tigray, where, as stated, the household is both the unit of production and consumption. However, the DECSI programme extends to urban as well as to rural areas, and the household focus may be less relevant in towns and cities where people follow different livelihood strategies, and household members may possibly not be as dependent on each other for carrying out these strategies, nor as willing to pool and share their income, as is the case of peasant households in the rural area. Whether the household history approach is equally successful in urban settings is of course an empirical question. Based on our findings during the study, it seemed to work well also in this context. But given the brevity of the qualitative fieldwork we were able to carry out, it is quite possible that in reality there were issues among urban households that we were unable to capture and that made the approach less useful there. The general methodological point arising from this reflection is simply that the choice of making the household the fundamental unit must be based on a knowledge of what functions are normally organized at the household level.

3.2. *Household development cycles*

Given the universal importance of households, across very different societies, there is a considerable anthropological literature discussing this unit: how to define it; the various forms it takes and the typologies one may construct; the functions or tasks it organizes; the cultural ideas attached to it; its relationship to the dimensions of family and kinship; its internal relationships; the cultural ideas and values attached to it, and so on (Netting et al., 1984). This is not the place to review this broad literature. There is one element, though, which is highly relevant for our purpose and must be brought into the discussion: the analysis of the household development cycle, as discussed in the classic *The Developmental Cycle in Domestic Groups* (Goody, 1958). As Meyer Fortes writes in the introduction, this is a way of bringing time into the analysis of households by investigating and analysing the stages through which a household moves as its members are born, grow up, marry, have children, move out or die. Depending on how households or domestic units are organized in different societies, the forms of these stages may vary widely, yet in general it is possible to recognize a phase of expansion, during the ‘period of procreation’ – a phase of dispersion (often overlapping with the first phase) as children move out, and a phase of replacement as the original parents die and a couple of the next generation takes over (Fortes, 1958).

A key point is that the household's membership changes through these phases, and crucially, the ratio between the number of producing household members and the number of mouths to feed will vary considerably. One of the articles of the edited book, by Derrick J. Stenning, picks up on this point and expands on it by introducing the concept of household viability. This is based on a double relationship: a household 'is viable when the labour it can provide is suitable for the exploitation of its means of subsistence while the latter is adequate for the support of the members of the domestic unit' (Stenning, 1958, p. 92). Stenning illustrates his point by a detailed analysis of the typical household structure of a group of Fulani cattle-owning nomads of north-eastern Nigeria. The household structure of the polygamous pastoral Fulani is complicated, with several sub-units within a sex-segregated compound under the leadership of one male household head. We need not go into that complexity here; suffice to point out that for the Fulani, their means of subsistence are overwhelmingly linked to their cattle. Domestic viability, then, rests on maintaining a balance where the household at all points of time has sufficient labour power to take care of its animals, and sufficient animals to feed the household members. This balance may be strained at various times, owing both to internal factors, i.e. its stage in the household domestic cycle, and to external factors, such as a drought decimating the herd. In order to alleviate such stresses, complex strategies are employed – involving for instance the regulation of the household size or herd size by merging with the households or herds of kin, by temporarily borrowing members or animals or by the movement of people and cattle in search of better pastures. Stenning's concept of household viability serves to make sense of these complex strategies and the different household forms and structures to which they give rise. In the household histories reproduced above, the difficulty reported by Household #3 of labour for herding goats when children were either too small or at school is an explicit acknowledgement of household viability concerns.

This 'development cycle of domestic groups'-approach, and its emphasis on the dimension of time, has been of fundamental importance for our use of household histories. The understanding of the limitations imposed on households by their stage in the developmental cycle has been the reason why we instructed our assistants to start their interviews by asking about when the household was established and how its membership had evolved since then. Likewise, the concept of household viability brings attention to the means of subsistence available to the household and the labour requirements and productive potential they have. An important analytical insight we can draw from these perspectives is that a fundamental reason for why female-headed households are often vulnerable is not because the head of household is a woman but that in most cases a female-headed household consists of fewer producers than a male-headed one (since custom states that if a couple is cohabiting it is always the man who is defined as the head of household). Thus, female-headed households are more vulnerable not because of the sex of the head but because the status points to unfortunate structural-economic conditions.

The story of Household #2 illustrates the point: the household head tells that after she was widowed, she could not ‘involve fully in farming’ because of lack of labour. Instead, she concentrated her efforts on petty trading and beer brewing. She illustrates a trend that is confirmed by the wider material we have on female-headed households: they have tended to use credit as capital for non-agricultural activities, often those that can be carried out close to the house – thereby also facilitating the looking after small children, another limitation for many single mothers. As mentioned above, one surprising finding from the quantitative material was that female-headed households was the group that most consistently had improved their situation through participation in the credit programme. It is reasonable to assume that reasons for this may be that the trade and craft activities pursued by female household heads are less liable to the unpredictable droughts that plague Tigrayan agriculture, and that in contrast to agriculture, loans for these activities have regular down payments that make default less probable.

3.3. *Other stories: family and life histories*

While little is written on the methodology of household histories, there is somewhat more on family histories. The classic monographs of Oscar Lewis from Mexico and Puerto Rico (1961; 1966), for instance, used vivid and detailed descriptions of the history of slum dwelling families to paint a picture of what Lewis termed ‘the culture of poverty’. In our terminology, household and family refer to different phenomena. Household refers to a unit organizing practical tasks such as residence, food preparation and consumption, the socialization of children and, as we have seen, economic functions as well. We understand family as a kin-based structure, often referring to units (such as nuclear family, extended family) that may be co-extensions with the household. Yet, even if the terms sometimes overlap, they are logically distinct, in our understanding of the terms. However, not all social scientists share this understanding, and some of the work referring to family histories is actually very similar to what we tried to do in the DECSI study. Miller (2000; 2007) and Miller et al. (2011) have written about the method of using ‘family histories’ (that are similar to our household histories) and applied it in practical studies, including investigations of poverty dynamics in Poland and Kenya.

The use of individual life histories as a method is, however, much more common, and there is a considerable body of literature using this approach, including explicit discussions of the methodology. We cannot give an overview of this literature here, but the distinction sometimes made between life histories and life stories (Hagene, 2002) is useful for our methodological discussion. In this terminology, *life history* refers to an attempt at an objective description of changes through a lifetime, pieced together by the researcher, while *life story* is the individual’s presentation of his or her history, differing from the former in that it is structured by tricks of memory as well as by the

pragmatics of the interview situation and desires to present an image and to construct an identity.⁷

That an individual's telling of his or her story will thus differ from the more objective version can be seen both as a methodological problem and as an opportunity that gives an intake to narrative analysis and to understand identity construction. Depending on the objective of a study, the one or the other may be most relevant.

This issue is of course relevant also for household histories. It is clear that the version of the household history given by the household member (or even members) interviewed will diverge from more disinterested, objective versions. (The problem is exacerbated by the fact that one or a few individuals are speaking on behalf of a larger whole.) The excerpts of the household histories presented above are in this perspective really household *stories* – subjective narratives that we have not sought to confirm through cross-checking with other sources. However, as our goal is to investigate the impacts of a credit programme, we are seeking information on the objective changes the households have experienced. Thus, although we fully acknowledge the subjective, narrative aspect in the stories we have collected, what we search for are the household *histories*, not the stories. Given more time and resources for the study, we would have focused more on triangulating methods in order to maximize the data's reliability. However, given the limitations we were working under, it seemed (and still seems) a sensible methodological decision to take the stories we collected if not as approximations of 'real' histories, then at least as sufficiently true to actual events as to give us insights into the social mechanisms at work. In this sense, in our study, they serve as household histories, and this is therefore the terminology we use. It should also be added that to leave out the narrative parts of the data gathering (the 'stories') is no solution to this challenge, as the perspectives of the household members on the changes they have experienced are an irreducible part of the reality we study.

4. Analytical–methodological issues: case studies and social mechanisms

An important issue for our discussion is to clarify the nature of the data we are actually gathering and the strengths and limitations they have. Interviews provide first and foremost discursive data. That is, data on statements. It is pertinent to distinguish between that which is said and that which is done: interviews are methods that generate information in linguistic form; words are used to convey some kind of information about something. However, there is no simple dichotomy between saying and doing, because saying is (a special kind of) doing. Utterances (speech-acts) are acts. Thus saying *is* doing. However, an important distinction between types of linguistic data

⁷'Objective' is here used in a very limited sense. We do not propose that we as social scientists can uncover an undisputed reality beyond contestation. However, at the root of most academic positions is an *idea* that some accounts are more disinterested than others, even though none are indisputably objective. Thus, we hold on to the idea that there exist criteria for distinguishing between degrees of objectivity.

needs to be made: there is an important difference between whether an informant tells the interviewer that the household has no assets and is extremely poor, or whether the informant expresses his/her opinions about the DECSI programme. In the former case it is a claim about a set of facts; in the latter, it is a subjective opinion. The difference points to a difference in how to gather and treat data. (Of course, in reality many statements contain both aspects.)

In the former case language claims to picture a reality and hence the speech act is a sort of contention that might be (more or less) true or false. This means that the statements are only a secondary source – they could in principle be checked by referring back to the facts. In the latter case – expressing an opinion – there is an irrefutable element of evaluation, which is not susceptible to a truth-check. Rather, it is linked to attitudes and as such they are not directly referring to an outer reality and hence they are primarily linguistic acts in themselves.

We claim that interviews of the type we refer to here, talking with household members, in the household setting, holds advantages over alternative forms of data gathering. Firstly, being in the *setting* of relevance (the household) implies that the mind-frame of the interviewees are correct; research show that the thematic and physical context of parole affects the linguistic acts (Helle-Valle, 1997; Malinowski, 1974). Thus, the fact that we are ‘there’ makes a difference for the validity and reliability of the data produced. Secondly, the interviews were *dialogical*; the ideal is the (relatively) open exchange of points of view between those involved in the purpose of letting the dialogical dynamic generate a common understanding of the topics discussed (cf., e.g. Tanggaard, 2009). The interviews enable the interviewer to link the different statements both to other statements – by going back to a central issue, expanding on it, attacking it from a different angle – and to the hard, tangible facts of the setting. For instance, in cases where factual claims are made – like being destitute – the statement loses its credibility if one can observe that the household do in fact possess a reasonable amount of assets. Thus, confronting (in indirect, polite and ethical ways) the interviewee with the mismatch between the statement and the physical reality of the setting might serve as a fruitful source of expanding on a many-faceted and complex matter. In this sense the interview becomes much more than an interview – it is also a form of participant observation. Although the reports from the research assistants did not reveal such ‘corrections’, they had several stories of such dialogical interviews. Thirdly, statements that (primarily) belong to the second class – being opinions – are also much better handled in a dialogical, *in situ* interview. Opinions are always complex and often ephemeral and tied to particular contexts. Therefore they always require elaborations. Simple survey-based, or even monological interviews can never disclose the complexity and multi-facetedness of an opinion. One of the reasons for this is that opinions are always linked to the realm of normativity, which by necessity requires contextualizations for them to have meaning. They are linked to the interviewee’s concerns, world-view and social relations. In all; we can say that these ways of carrying out the interviews had a double positive effect; it made the discursive data we

gathered through the dialogues better, and it provided us with a lot of non-discursive data as well.⁸

4.1. Case studies

As argued above, the literature on (individual) life histories serves to throw light on the related issue of household histories. If we step further back, it is possible to see that household histories – at least in the way that we use them – are also an instance of the larger methodological category of case studies. It is thus useful to look at the literature on case studies in order to delve further into the methodological issues of household histories.

To be able to critically evaluate and reflect on household histories, it is necessary to discuss what a case study implies in terms of scientific practice. First, *case* is a polythetic concept, i.e. used in various ways by various scholars (cf., e.g. Mjøset, 2006; Thomas, 2011; Tight, 2010). We will pragmatically define a case as a particular event of some sort – something that has actually happened. It might involve one or more persons, and it could take place in an instant or over a period of time. An event becomes a case if it is used in a scientific context for an analytical purpose, usually to support a thesis or illustrate arguments. Thus, the household histories at the beginning of this article are cases. However, for specific purposes we might select more particular events in these household stories as cases also. For instance, the first part of Household #3's history is a rather typical case of poor Tigrayan households at the end of the twentieth century, while the period after she succeeded with her loans can be analysed as another case. The motive for using cases is double: it has actually happened, it is an exemplary of practice and it enables us to give a detailed description (and thereby analysis) of the event. The richness of such descriptions (Geertz, 1984) limits the number of cases we can present and analyse. Formulated in positivist terms a case is the presentation of a singular unit containing many variables. Thus, its singularity is not an aim in itself but the result of the time and space its 'thickness' requires.

The classic objection against case-based scientific practice is that it lacks rigour and the ability to generalize and reach sound conclusions. According to Flyvbjerg (2006), the conventional view of case-based research is that (i) general, theoretical knowledge is more valuable than particular, case-related knowledge; (ii) a case is not a sound basis for generalization; (iii) it can only be used for generating hypotheses; (iv) it contains a verification-bias and (v) it is a poor basis for generating theories and propositions (Flyvbjerg, 2006, p. 421). These *misunderstandings*, as he calls them, are refuted one by one. In short he argues that real expert knowledge

⁸This applies not only to the interviews we carried out ourselves. Our assistants were experienced and highly qualified, with the added advantages of being Tigrayan themselves and speaking the language. They were fully capable of drawing on these advantages of dialogical interviewing techniques.

is always generated by, and bases itself on, concrete, particular knowledge (cf. also Harré, 2009); that cases are a sound basis for generalizing; that they are ideal for not only generating but also testing hypotheses; that case studies are no more biased towards verification than quantitative research and that they form an ideal foundation for generating certain kinds of general knowledge.

We will not go into all the details of his arguments, but concentrate on one issue; that of case-based research's potential for generalizing. In short, he argues that the 'generalizability of case studies can be increased by the strategic selection of cases' (Flyvbjerg, 2006, p. 425). In addition to the possibility of a stratified selection of cases of the type we know from surveys, he lists four types: extreme/deviant cases; maximum variation cases; critical cases and so-called paradigmatic cases. The extreme/deviant case is exemplified by John and Jean Comaroff's article 'The Madman and the Migrant' (1987). The argument in the article – about the at times frustrating post-colonial condition of working men in South Africa – is centred around one case: that of a former labourer in the gold mines around Johannesburg. The argumentative logic is based on the idea that the behaviour of this mad man expresses, albeit in a perverse form, central concerns and contradictions of the working class condition. Thus, the insanity – through the focus on one case – can be said to represent a kind of a compressed cultural idiom. Likewise, in our material, the history of a female household head, who had gone from being poor to becoming among the wealthiest by using credit to construct houses that were subsequently let out and secured a steady income, could be said to represent an extreme success case. While not representative in an ordinary sense, the story embodies the possible potential of the credit programme.

The second type is maximum variation cases: one chooses (at least) two cases that is seen to represent the extremes of a linear variation. In our analysis, we contrasted two of the household histories – a particularly successful female-headed household and a particularly unsuccessful one – in order to explore the many factors that contribute to one or the other outcome in the two cases, and the different ramifications of these two credit histories. This gave an important insight into both the variation of experiences, and the many mechanisms that affect outcomes – and hence to the range of impacts of the credit programme.

The rationale behind the choice of critical cases is of the type; if contention *x* is true in this case it is true in many/all cases. For instance, some social scientists on Africa in the 1950s reasoned that witchcraft was a product of a traditional mentality and therefore if they found witchcraft widespread in the cities it would be everywhere. In fact, they did find it in the cities and hence the conclusion was that it was everywhere. This was a correct finding but the reasoning was in fact wrong. Later research on witchcraft practices in Africa suggests that in many cases witchcraft is becoming more rampant in cities than in rural due to the increased uncertainties linked to urban life (e.g. Austen, 1993). This illustrates that although the rationale behind the function of the critical case is valid enough a lack of intimate knowledge of the universe one is studying

can lead to flawed logic. Anyway, in our study, we did not find this logic particularly useful, and we do not have any examples of critical cases from our own work.

The last type Flyvbjerg proposes is the paradigmatic case. This case is not easy to define but it is given its status by being especially typical in one way or another. Flyvbjerg is strikingly vague in treating this type of case, which is no coincidence. Why, and in what way the chosen case is typical – and what it is typical of – is obviously hard to pinpoint. It seems that the judgement for deciding what counts as a paradigmatic case is based on experience and knowledge of the field in question, as well as related to the research questions that the case shall illustrate. The best example of a paradigmatic case is Foucault's Panopticon. The criteria for choosing this case is not straightforward but when analysed most agree that it is a very telling case of the quintessential modernist form of power (Foucault, 1991). An example from another one of our interviews concerns a household that had taken a loan for the purpose of agricultural investment. Instead, due to a critical economic situation, they had used the money to buy food. DECSI only gives loans for productive purposes. The possibility of fungibility – that is, using the loan for other purposes – is a concern for the institution, and among the issues we were expected to look into. As expected, the fact that the loan had been used for consumption rather than investment meant that the household had been unable to pay its debt. It was thus classified as an unsuccessful client, and at first sight a case that confirmed the correctness of DECSI's policy of only lending for productive purposes. However, when recounting the story, the interviewed woman expressed satisfaction with the DECSI programme, but maintained that using the credit to buy food had been the correct decision given the situation of her household: 'If not, we would have been dead', she said. She clearly preferred her current situation of having a debt she was struggling to repay, to the alternative. The case is paradigmatic for illustrating the point that it is always the borrower who is best placed to assess the true value of different options for using the loan they have taken. In our report we referred to the case when arguing that DECSI ought not to spend much effort in seeking to ensure that credit was actually used for the intended productive purposes.

What this typology of case selection suggests is that there are many ways to select cases in order to improve reliability and representation in relation to a research question and a researched universe. We agree that it is indeed necessary to reflect critically on such selection (which too many of a qualitative bent do not). However, we are uncertain about how far it takes us in solving the challenges we are confronted with if basing ourselves exclusively on qualitative methods. We return to this in the concluding section.

4.2. *Epistemological issues and social mechanisms*

There are two types of logic connected to a relaxed idea of representativity (cf. Mjøset, 2006): an essentialist one (structuralist-like); the idea that all cultural forms are more or less good expressions of a transcendent type; much like each instrument in an orchestra

are playing according to a musical score. Within such a logic the problem of representativity does not really exist since the proclaimed existence of an archetype ensures that all tangible cultural expressions are copies of that type (cf., e.g. Helle-Valle, 2010). Another way of getting away with escaping from the problem is by linking analyses to an epoch, like modernity. By arguing deductively that since modernity consists of certain constitutive traits then all particular practices within a modernist universe can be expressed, and explain in terms of those traits. Again, it is a variant of what, among others; DeLanda (2005) and Latour (2005) calls a hierarchical ontology – namely that particulars are explained by way of generals.⁹ We hold that such reasoning is invalid; patterns and causalities must be established as results of and in relation to the particular data we are confronted with. The methodological downside of such a position is, however, that we need to take the question of representation very seriously.

A related challenge is that of causality. Causal-oriented explanations in the social sciences are often linked to covering law-logic (cf. Hempel, 1966), which follows the same ontological logic as described above (e.g. DeLanda, 2005, 49ff). The main principal objection is that causality cannot be external to practice (covering laws). A more mundane criticism is that the (social) world is so complex, and contains reflexive subjectivities that cannot be wholly predictable, that it is impossible to discover/invent laws of the social. One interesting alternative to such logic is *social mechanism*. According to Jon Elster, one of the leading proponents of this perspective, we can at best propose general mechanisms that govern social life. Such mechanisms are characterized by not being laws – i.e. of a general compelling nature – but tendencies of how one factor is usually linked to another. Thus, a mechanism does not prove or explain anything in itself but suggests that there is a causal link between two or more factors that need to be explored further (Elster, 1989; 2007; Hedström, 2005). For instance, one need not be long in Tigray to see that there is a strong link between the sex of the household head and its wealth: female-headed households tend to be poor. However, this is not a law – we can easily find exceptions to this ‘rule’ – the ‘extreme case’ of the female household head who used DECSI loans to buy houses for rent and became one of the wealthiest persons in the community proves this. The point about classifying such a phenomena as a social mechanism and not a law is that we will always find exceptions, thus we know that there is not a direct and simple causal relationship between the two

⁹According to them this is a logically invalid form of reasoning because if a general type shall explain particular instances then one has to accord a so-called ontologically realist status to these types. If not it cannot have a causative effect upon the particular (e.g. Latour 2005, pp. 167ff). What they propose instead is what they call a *flat ontology*. This implies that ‘what is real is what we see’. To use an analogy from structuralist terminology it is not only to concentrate on *parole* but in fact dismiss the very existence of *langue*. The world is flat in the sense that there are no forces of any kind ‘above’ or ‘below’ the tangible world that we encounter through our being in the world (which is *not* to say that the question of epistemology is straightforward – cf. Harré, 2009; Mol, 2002).

variables (therefore not a law). This awareness should then prompt us to be, on the one hand, wary of establishing simple causative relationships but rather focus our attention on revealing the various complex relationships that exist between different factors. On the other hand, our caution should not lead us to dismiss the idea that there might be law-like mechanisms that help explain the patterns we discover through our research. To propose such (weak, cautious) claims to causality and generalizability enriches the analysis.

5. Concluding remarks

As is vividly revealed in the introductory example of Girmay we are enthusiastic about the scientific worth of using household life histories. From our own research we find that the great value of using case stories is that it gives insights into the nooks and crannies of everyday life – they provide us with an understanding of what is going on in a given social setting which cannot be reached in any other way. This reflects the fact that it is through concrete events that we can tap into the complexities that characterizes social life, and thereby also find cues as to what kind of mechanisms that operate in any given field.

As we have argued, in many studies of social life – and not the least in studies of development processes and development programmes – it is the household which is the most relevant unit. The household, as the unit for production and consumption, is the framework for decision-making as well as for considerations about what is rational, what is wise and what is right and wrong. Hence, for these reasons it is the household which very often must be seen as the relevant unit of study – even as the actor. This is not to say that there are no tensions among household members on questions of allocation of resources and the balancing between me and us. However, household histories are precisely the type of methodology that gives us the opportunity to study such tensions and dynamics.

Moreover, household histories add a dimension of time to the data collected, and by focusing on household development cycles and the households' quest for viability, we get a direct intake on processes of change (and maintenance) and how they play out over time. As development fundamentally refers to change over time, household histories is therefore a very useful methodological tool for development studies. We are not saying that this is the only intake – and in fact we do not necessarily consider it to be the best way – but in practice it is often the most realistic way to gather such information. The obvious weakness of relying on *stories* – i.e. the narratives about the household by household members – is their reliability. Stories, however honestly narrated, are always positioned and hence biased. This weakness can to some extent be corrected for by being an active and critical interviewer, and combining the stories with participant observation. By triangulating stories told, reflections provided by the informants and one's own observations of what is really going on we can minimize the reliability-problem while at the same time integrate the reflexive practices that life (in households) consists of.

However, we will in no way downplay the problem of representation inherent in case methodology. Most research has ambitions of providing reliable information about a social field, whether it is a small, bounded local community or a wider 'society'. The limited number of household histories we have collected represents only a small fraction of the universe we wish to say something about. Thus, we need to generalize on the basis of the particular cases. As we have argued, we hold that the conventional view that 'proper' research works by way of hypothetic-deductive reasoning is at best only a partial truth, and perhaps wrong. Worthwhile knowledge is at least in part inductive. Moreover, as social scientists we cannot, and should not, work by way of seeing specific cases as instances of universal covering laws. Rather, what we can hope to provide in terms of explanation and causality is to try to uncover *social mechanisms* – what can be called 'tendential causality'. This is so because the social fields we study are always so complex and heterogeneous that it is impossible to uncover laws – and explain by way of such laws.

Nevertheless, the issue of generalizing from qualitative data is a challenge that should be taken very seriously. While we acknowledge the value of Flyvbjerg's arguments about various strategic ways of selecting and combining cases, we maintain that the problem of representativity is best dealt with by combining case-based research with quantitative methods. Although we might be able to come a long way in learned reflections on how the particular cases relate to the universe in question, it is nevertheless only by gaining an overview of the community by way of reliable quantitative data that we can generalize with a high degree of certainty. While we argue that household histories are a useful tool for development studies, its full potential is only realized in combination with other and quantitative approaches.

Such triangulation (e.g. Denzin, 1989), combining qualitative and quantitative data (and narratives, dialogues and observations), is hence our ideal. Our DECSI study provided the opportunity to combine methods, and showed us the value of triangulation. For instance, female-headed households were most likely to improve their situation through the DECSI programme, the quantitative study told us. This was not something we could have known from our qualitative material. However, knowing this, we could use the qualitative material to search for possible explanations. The understanding of the complexities of running a household, afforded by the household histories, showed us the logic of the economic strategies followed by female-headed households. They focused on activities compatible with their limited labour power and mobility, such as petty trade, handicrafts and food processing. As these activities required capital (for instance through loans), were less susceptible to unpredictable external shocks than agriculture (which male borrowers tended to prioritize) and facilitated regular saving (and hence repayment on time), it is possible to see why these most vulnerable of loan takers actually did quite well within the programme. In sum, combining the two approaches yields findings that go beyond what could have been achieved by either of them alone.

Notes on contributors

Jo Helle-Valle is Professor at the Development Studies at Oslo and Akershus University College (HiOA) and Visiting Professor at the Department of African and African American Studies, Harvard University. He holds a Dr. polit. in Social Anthropology (from 1996) based on fieldwork in a local community in the Botswana part of the Kalahari. He has also conducted fieldwork in Uganda and Ethiopia, and has been a Visiting Scholar in Oxford, Gaborone and Vancouver. He has worked and published on a great variety of topics, including local politics, local economy, globalization, change, gender, as well as on media and ontological issues. From 2015 to 2018, he heads a comparative research project titled 'New media practices in a changing Africa' (www.mediafrica.no), involving a dozen scholars from institutions in six countries doing fieldwork on everyday media practices in Botswana, South Africa, Zambia and DR Congo.

Axel Borchgrevink is Professor of Development Studies at Oslo and Akershus University (HIOA). He holds a Ph.D. in social anthropology from the University of Oslo, and has done anthropological fieldwork in the Philippines and Nicaragua. His monograph *Clean and Green: Knowledge and Morality in a Philippine Farming Community* was published by Arterneo de Manila University Press in 2014, and he was co-editor (together with John McNeish and Owen Logan) of *Contested Powers: The Politics of Energy and Development in Latin America* (Zed Books, 2015). He has also published on politics, aid and development on the Horn of Africa.

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