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Risk, Growth and Poverty: what do we know, what do we need to know?

Stefan Dercon[1*]

This note has three objectives: first, it aims to take stock of the nature of the evidence available and on the links between uninsured risk and shocks on the one hand, and growth and poverty on the other, both at a macro and micro level. Secondly, it makes a number of suggestions of the type of work that could be fruitfully implemented. Finally, it tries to strike a balance between the needs for the policy maker and the requirements for academic scrutiny of evidence, in offering suggestions for priorities in work.

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*Department of International Development, University of Oxford

[1] Stefan Dercon is Professor of Development Economics at the University of Oxford. stefan.dercon@economics.ox.ac.uk. This paper originated from a briefing note to DfID, as part of a project on risk and growth, and the need for interventions.

Introduction

This note has three objectives: first, it aims to take stock of the nature of the evidence available and on the links between uninsured risk and shocks on the one hand, and growth and poverty on the other, both at a macro and micro level. Secondly, it makes a number of suggestions of the type of work that could be fruitfully implemented. Finally, it tries to strike a balance between the needs for the policy maker and the requirements for academic scrutiny of evidence, in offering suggestions for priorities in work.[2]

Let me offer the main conclusions of this paper. First, we emphasize that risk and its consequences should not be viewed as a 'temporary' problem, something that is just part of life, but in fact has long term and cumulative implications for the poor. Secondly, these long-term implications can also be viewed in terms of the activities people can enter into and the earnings they can obtain: as such, risk has implications for growth of substantial parts of the population. It is therefore important to start integrating risk not just in the analysis of poverty but also of growth. Risk forces people to disengage from profitable activities. The evidence is suggestive for crucial impacts on pro-poor growth: pro-poor growth policies need to have a specific space for . This lack of participation has nothing to do with 'preferences' or 'attitudes' by the poor towards risk: they are relate to the fundamental failure of markets and policy to offer credible protection against risk. As this informs us about a possibly important barrier to the participation of the poor in growth, pro-poor growth interventions need to take this seriously. But making these points to enter the consciousness of the policy makers, we need to build up further the evidence base. The most central gap is the link between micro-level analysis and the macroeconomic and growth implications.

This conclusions follow from the underlying view in this note is that risk is a cause of poverty, affecting growth and the extent to which the poor can take part. This implies that social protection activities can have very substantial economic and social returns. But we need to be cautious with this: the evidence base is growing but it is still very limited, and for this view to gain more broad acceptance, this evidence must be clear, convincing and transparent. This work could be done using the (at times berated) technique of cross-country comparisons, focusing on developing countries and the risks they face. This work should be done using panel data, to deepen the knowledge base on links between risk and poverty processes, supplemented by careful contextual evidence. We should do some experimenting with policy instruments – evaluating them, possibly using randomised trials. And we should not avoid the more difficult institutional questions, such as the scaling up of interventions.

But one should be clear from the onset that the aim of this paper is *not* to give a comprehensive summary of the issues related to risk, growth and poverty. The stock taking in this paper is done largely in order to ask some (at times difficult) questions

[2] This is note was first drafted as a background note for work on Risk and Growth, funded by the DFID via the World Bank. It benefited from inputs by Valerie Kozel, Matthew Greenslade, Jillian Popkins and Robert Holzmann and participants at a workshop in Leuven, June 2005, as well as other background notes and presentations by Alain de Janvry and Elizabeth Sadoulet, Adeel Malik, Michael Carter and Chris Barrett, and Chris Elbers and Jan Willem Gunning. All errors are obviously mine.

for future work. Although there is overlap, one is encouraged to look at related papers such as Gunning and Elbers (2005) or Barrett and Carter (2005)). More specifically, in the terms of the available, and some its direct implications for policy, the reader is encouraged to revisit Dercon (2005). Section 2.4 in the current paper draws heavily on that paper as well.

To put the discussion into context, a first section will introduce the way poverty and growth, and risk are typically treated as distinct issues in theoretical, empirical and policy analysis. To do so, I first give a brief impressionist view of the current policy discourse of poverty and growth. Then, I will briefly discuss where risk is (not) entering in standard analysis. The next section will then discuss a simple microeconomic framework, discuss sources of risk and the micro-evidence on the link between risk and poverty. We also ask whether we are identifying risk correctly and discuss the problems with identifying the impact of risk. Section 3 aims to see how this evidence can be moved to the macro-level – to growth. I will first discuss whether the micro-evidence is enough to construct a *growth* argument. I will also spell out the link between risk and poverty traps. Finally, we will identify some issues of research at the macro-level and subsequently the micro-level work that could be expanded (or at least some issues identified). The final section will then raise some problems with translating this evidence into a broad policy conclusion for public action, as part of a conclusion.

1. Poverty, growth and risk: some introductory remarks

1.1 An impressionist view of the policy discourse on poverty and growth

Most of the current policy analysis and advice on poverty focuses on stimulating broad-based growth, as a means of achieving the Millennium Development Goals (MDGs). Much of this analysis – and the debate on the relative importance of factors - appears (when translated into the economists' lens) to come down to some simple growth accounting framework, spiced up with some basic ideas from classical and modern growth theory.

The focus on how to achieve these goals tends to drift around some of the fundamental processes of accumulation: achieving growth in the capital stock (focusing on investment, investment climate and the complementarities from infrastructure, public goods) and growth in the human capital stock (broad based education, and somewhat more recently, health). To achieve a sufficient impact on the MDGs, the concerns of distribution and inequality have been moved back onto the agenda (this time largely for its instrumental value, not its intrinsic value – see the latest WDR on inequality). The underlying parameters of the relevant relationships describing both the growth and poverty reduction relationships (TFP growth, poverty-growth elasticities, and all the underlying instrumental relationships) are, in the current discourse, mediated by the broad spectrum of issues, often summarised as 'policies' versus 'institutions'. Much of the debate then focuses on whether these factors are internal to LDCs versus international, and whether it is the institutional side or the 'policy' side that dominates. The main protagonists in this debate, including recipient LDC governments, bilateral and international agencies, multinational NGOs and the recent emergence of populist advocacy specialists take sides on these factors, with good and sometimes not very good evidence.

The economics underlying this is, most of the time, obviously not very sophisticated: it is basic growth theory, mixed with simple trade theory, often devoid of strong micro-foundations and evidence. Poverty relationships are based on simple views of the functional distribution of incomes (e.g. the poor only have labour, and limited skills and education, while the rich have more education and also capital). It works nevertheless reasonably well to understand what is going on. Growth requires a situation where there are opportunities and where people are willing to invest: to part with funds they have today to get more later on. Taking advantage of opportunities requires, beyond the means to invest, some skills, some complementary inputs and an environment where the profits of one's effort cannot be expropriated. Sharing in growth by relatively poorer people involves access to the endowments or assets necessary to take part in this process – access to skills, health, credit - and the 'social' possibility to share in wealth creation and the social and physical infrastructure in the society, suggesting issues of empowerment.

The macro-growth thinking behind this is well beyond the simple microeconomic general equilibrium framework with complete markets. It strikes me, however, that the foundations for the policies towards the poor are still largely based on a variation of the so-called "second welfare theorem"[3], whereby simple redistribution is being replaced with something more sophisticated, at least in political economy terms, with a focus on educational and health asset accumulation, and voice and democracy issues. I should stress that there is intrinsically nothing wrong with this – and nothing in the note wants to argue against this.

There are however two, possibly linked, issues that are worth considering that are fundamentally missing in this framework. First, once it is recognised that this perfect competitive world is an illusion and at least some market imperfections are fundamentally difficult to overcome, then there may be processes of low growth, even growth traps and poverty traps that are poorly understood in this framework. Secondly, risk is dismally treated in this framework, and we are only recently started to address its implications, not least in a world with other imperfections to markets and where 'traps' become plausible. Taken together, this suggests that by introducing risk more properly into the analysis, we could focus on two related issues: first, that risk may well be crucial limiting factor for 'growth' especially in the high risk environments characterising many poor developing countries, and that risk may well have important implications for the extent to which poverty is reduced in the face of growth, since the poor, in their endeavour to have a relatively secure existence take actions that excludes them from taking advantage of 'growth'.

[3] A reminder: the first welfare theorem suggests that competitive markets result in equilibrium in a 'pareto' efficient allocation, so that no-one can be made better off without at least one person being made worse off. The second welfare theorem states that every pareto efficient allocation in an economy can be obtained for a particular, given distribution of endowments. In short, efficiency can be obtained after first allocating endowments in any way a 'social planner' – akin to Plato's philosopher-king – may find socially optimal. Markets do not have to become 'distorted' even if one cares about distribution. The corollary is that distorting markets for fairness reasons has to result in efficiency losses.

2. 2. The problem of risk in the economic and policy literature

"Risk" features only peripherally in most of the economic diagnosis and policy discourse related to the MDGs. It is rarely central to the discussions of growth and poverty. It is viewed as a source of 'temporary' problems, that one may want to solve, but is not a cause of 'permanent' low GDP, low growth or high poverty. Designing policies to help with risk is then almost a luxury, to be considered after some more basic and essential problems have been resolved.

In most macro-growth analysis, risk is just a nuisance parameter – noise to the econometrician, and unspecified randomness in theory modelling. In most empirical growth models, risk enters in the error term, i.e. as a stationary shifter of the efficiency parameter. This means that risk nor shocks have permanent or persistent effects. Any impact will only be transitory, and 'natural' processes will in the long-run resolve them.

In empirical analysis, there is some work that enters descriptions of 'shocks' (Easterly et al., Rodrik, Aghion et al., 2005), and at least one obvious attempt of entering 'risk' in empirical cross-country growth analysis. Risk is here defined as commodity price risk (Ramey and Ramey, 1992). But otherwise it is largely lacking, d despite the fact that risk is one of the defining characteristics of the investment, capital accumulation and growth problem. The emerging literature on 'volatility and growth', while taking a rather different focus, and tends to be biased toward middle- and rich economies, may bring some elements relevant. Furthermore, its emphasis on 'ex-post' volatility (such as using the standard error of growth over relatively short periods of time as the relevant indicator) is not clearly or easily translated in a framework of 'ex-ante' risk.

When focusing on welfare, risk is seen as a source of 'temporary', or 'transitory' poverty: a source of misfortune, pushing some people below a socially accepted norm temporarily. This view is prevalent in the macroeconomic literature, or more precisely, the literature on the welfare implications of macroeconomic crisis and its responses in the form of economic reform. Important economic shocks, such as a sudden exchange rate depreciation or (policy-driven) devaluation, have important welfare implications, in the form of losers and gainers. If its welfare implications are discussed, these consequences are defined as 'temporary' shocks, requiring some 'safety nets'.

Similarly, in the more microeconomic literature, risk is also typically treated as causing 'temporary' or 'transitory' effects. To illustrate this consider the following. A standard way of assessing the implications of risk in micro-data contexts is to measure the consumption or nutrition losses linked to different shocks, such as rainfall, illness, pests, etc. Essentially, outcome variables are correlated with shocks in a multivariate context, and the contribution to the latter can be quantified. For example, in Ethiopia, we recently calculated that shocks between 1999 and 2004 can 'explain' about a third of the poverty levels in 2004 – i.e. that a third of the poor would not have been below the poverty line in 2004 had sufficient protection existed so no impact of shocks had to be felt and they could have been allowed to keep consumption 'smooth'. This is another way of stating that 'a third of poverty is transitory'.

But this type of analysis – still much in vogue, and unfortunately including in relatively widespread 'risk and vulnerability analysis' – is moving risk again to the periphery of the poverty discourse. The reason is simple: two-thirds of the calculated poverty in 2004 would still have existed even if there had been normal rains, no-one in the family had been ill and no pests had affected crops. These two-thirds are then clearly more permanently poor. This 'core' of the poverty problem are the 'chronic poor', to put it simply, those below the norm in good *and* in bad years. The counterfactual calculations on Ethiopia would also suggest that the chronic poor are a more substantial group anyway: 66 percent of the poor! As a result, prioritizing these 'chronic poor' may be a more appropriate strategy.

Of course, this view contains an important fallacy: by treating risk as fundamentally just being the 'shocks' in a (stationary or unchanging) consumption or other outcome process, it assumes that even if households cannot cope in the period of the shock, this has no implications in the long-run. This ignores the 'ex-post' consequences of shocks, in terms of destruction of assets, human capital and social institutions. Furthermore, it ignores the sophisticated risk management strategies employed by the poor to reduce risk exposure and its consequences, which are partially effective but in the end force risk to be traded for lower returns: profitable opportunities have to be avoided – the 'ex-ante' consequence of risk. These 'ex-ante' and 'ex-post' costs of risk are the two fundamental pillars of the view that *risk is a cause of poverty* and needs to be put more at the core of the poverty reduction debate and the MDGs. Details of this view is developed in Dercon (2005). The conceptual and theoretical arguments are not repeated here; the rest of this paper focuses rather on outstanding issues, *not on a comprehensive review of concepts and evidence*.

The lack of risk at the centre of the discourse is striking six years after the 2000/01 World Development Report on Poverty, which had three pillars: opportunities, empowerment and 'security'. The latter was meant to include a substantial focus on risk and related vulnerability. This has already two crucial implications for research: first, find a way to get those researchers working on 'growth' issues to take risk more seriously. While one may be deeply sceptical about cross-country growth regressions, there must be ways of getting these dimensions better covered in this type of analysis. Below I give a few suggestions. Secondly, the modelling of the link between poverty and risk, already an active area, needs to be expanded. Many (including other background notes) make excellent suggestions. I will add a few more below.

2. Risk and Poverty: inference from microeconomic analysis

2.1. Getting the questions right: a conceptual framework

At the risk of stating the obvious, when launching further work on risk, growth and poverty, whether at the micro and macro-level, it is instructive to place this work in the simple framework below (table 1). Households, firms – or societies as a whole, face a multitude of risk, and given their options and characteristics, they will make risk management decisions, or at least decisions with implications for risk management. This decision-making 'ex-ante' has implications for outcomes, in terms of levels and growth, in the short run and long run. Next, shocks may occur – effectively a realisation of the state of the world whose risk may or may not have been recognised beforehand. People's responses or inability to respond again will have

implications for outcomes in terms of levels and growth, in the short run and in the long run.

Not all research will articulate all these dimensions. For example, when studying the long term consequence of a serious drought, via a focus on the evolution of assets, limited attention is likely to be paid to the underlying risk management decisions and other risks faced. But it is instructive to cross-check where different questions are addressed. Not least: avoiding the confusion that analysis is about 'shocks' compared to 'risk' would be helpful. Below, we will use this table loosely to identify some outstanding issues.

Table 1 Risk and outcomes

UNINSURED RISK 'sources of risk'	→	*risk management decisions*	→	Implications for OUTCOMES In terms of LEVELS In terms of GROWTH In the SHORT RUN In the LONG RUN	→	SHOCK "realisation of the state of the world"	→	*'risk coping' decisions*	→	Implications for OUTCOMES In terms of LEVELS In terms of GROWTH In the SHORT RUN In the LONG RUN

At this stage, it is helpful to get one crucial fallacy out of the way. It is sometimes suggested that risk is problem for some families or firms *because* of risk aversion. Those that take risks are less risk averse and 'entrepreneurial', it is suggested, and therefore their decisions result in lower earnings in the long run. If people were less risk averse, risk would not be a problem: risk is just a matter of choice, and preferences drive the entire outcome. The fallacy in this reasoning is that risk is only a problem *because* of the lack of insurance, and in general, missing 'risk' markets: even with risk aversion, risk does not need to have detrimental consequences, since people can spend a little money to protect themselves if things go wrong. It becomes an important problem – with possibly disastrous consequences – if people do not have access to the means to protect themselves. In other words, it is a problem that should concern any policy maker when they lack the opportunities to choose a particular degree of protection, in line with their preferences.

2.2 Identifying risk

Since the analysis starts with risk, at least as important is how to narrow down which sources of risk one should focus with. Most of the work on risk and its consequences has a stylised example of a rural households, extensively engaged in agriculture, mostly crops and at least some livestock. The risk envisaged is related to climate, and if a shock is identified, it is a drought. There are of course good reasons to do so, given that most poor people in the world live in rural areas. Furthermore, when researching which shocks affect people in recent times, drought risk features highly. Table 1 gives a recent update on this from rural Ethiopia. Drought features highly, family illness and death is the next category.

Table 1: The incidence of serious shocks 1999-2004 in rural Ethiopia

Type of shocks households reported to be affected by, leading to serious loss of assets, income or consumption, of those affected by a shock (note: 95 percent of households reporting such a shock)	**Percentage**
Drought	46.8
Death of head, spouse or another person	42.7
Illness of head, spouse or another person	28.1
Inability to sell outputs or decreases in output prices	14.5
Pests or diseases that affected crops	13.8
Crime	12.7
Difficulty in obtaining inputs or increases in input prices	11.3
Policy/political shocks (land redistribution, state confiscation of assets, resettlement, forced contributions or arbitrary taxation)	7.4
Pests or diseases that affected livestock	7.0

Source: Ethiopian Rural Household Survey, 2004, and Dercon, Hoddinott and Woldehanna (2005). Based on recorded three worst shocks per household, leading to serious loss of income, consumption or assets. 95 percent of households report at least one serious shock.

But, for once looking very critically at these data, a few points should be emphasised. Investigating shock realisations is not the same as identifying the perceived risk faced by people. This is not as obvious a point. The more recent psychological literature on risk (confirmed by the behavioural work by Kahnemann and others) has focused on the fact that low probability events are given a much higher weight by people than objective risk assessments would give. So, even though 'political risks' or 'crime' may feature relatively lower in terms of realisations, responses in terms of their ex-ante behaviour to certain risks may be more substantial than may seem objectively warranted. For example, if a rational response to the risk of arbitrary taxation (e.g. the sudden introduction of extra local taxes, rather common during the 1980s) is to keep more assets liquid, then overestimating this risk may result in substantial untapped resources for self-improvement. This effect may possibly be more important than responses to other more 'normal' risks. At present, there is no evidence on this.

Another issue is that similar lists as in table 1 do not exist for other, increasingly more important settings such as those facing urban households or recent migrants. Of course, for some risks they may be easily collected – for example formal labour market risks of unemployment – but in general these lists are best at picking up simply identifiable shocks. Or increasing 'social risks' – risks that may imply that people fear that they will not boycotted in their activities by their local society, affecting their willingness to take on activities. And possibly most important: if people adjust their activities to deal with risk, the risks that will actually affect them are endogenously chosen from all the risk they could have faced: if typically once a week the electricity goes off for a few hours, then investing in selling ice cream may not be a good idea at all, removing profitable ice-cream production from the portfolio possibilities.

A related challenge involves finding ways of introducing risk that is relatively common to all people in a particular context (a region, a country) in analysis, but that is nevertheless likely to be crucial. Macro-work, comparing different countries, typically can do this, and this explains that if risk is introduced in macro-growth

analysis, they can focus on important 'institutional' risks, such as the 'risks of expropriation', 'lack of rule of law', or 'conflict'. Similarly, general economic insecurity is difficult to identify as well. Furthermore, some of the risks that economic analysis has emphasized, such as tenure insecurity and other property rights issues, are poorly picked up (with some exceptions). Empirical micro work needs to find ways of introducing these relevant risks more into the analysis.

Surely, there is no easy solution and many people have recognised the need to collect better data to get at 'risk distributions' faced by households and firms. Many studies use rules of thumb or econometric techniques– but are they really satisfactory? Some survey instruments have been used to elicit these and even focus group techniques have been applied, but there has not been a systematic review of these – which techniques work, where, why? Contrasting subjective perceptions of risk and objective realisations should be part of this work.

2.3 The behavioural challenge

In this section – standing slightly separate from the broad sequence of sections and sub-sections in this paper – I want to briefly raise some difficult questions related to our conceptual understanding, more specifically by briefly drawing on recent developments in economics, most notably behavioural economics. This is branch of economics, with inspiration from psychology, trying to investigate and take into account behaviour as it appears to take place, even if not quite consistent with the rational choice framework more usually applied in economics. Behavioural economics is on the ascent and further work on risk cannot just ignore this. Most researchers working on risk and its implications for poverty and growth have generally ignored this work. Formal modelling tends to be based on expected utility, explicitly identifying risk aversion in outcomes and/or wealth. The challenges posed by the behavioural evidence are not taking into account. Let me make a caricature of much of this work. "If we cannot explain it easily, let's redraw the utility function to fit the evidence." Some complicated discounting, discontinuous functions, fanning out of low probabilities and other 'tricks' can be applied. There are excellent reasons why one should not necessarily go in the direction: it redirects the focus of many observed outcomes back into the sphere of preferences, drawing attention away from the bread and butter of the economist, focusing on how constraints shape behaviour and outcome. Arguably, when dealing with risk and poverty, preferences are not the sphere we should start work: risk fundamentally only matters for behaviour because insurance markets fail, and it matters only a great deal for poverty because the lack of insurance and other mechanisms to deal with it imply ex-ante ex-post strategies and particular poverty and growth outcomes. It took a lot of work and effort of researchers to elucidate how the role of different market failures means that simply reducing observed behaviour to 'risk aversion' on the part of the poor is misleading. As Kochar (1995) notes, "the set of options faced by farmers offers little role for preferences" (p. 159). The behaviour of the poor with few insurance possibilities may *look* as if they have more (innate) risk-averse preferences, but it is the lack of insurance and credit, and the set of options available to them that forces them to take less risk and therefore forego income (see Eswaran and Kotwal, 1989, for a careful theoretical discussion).[4]

[4] A possible source of confusion in the literature is the concept of 'asset integration' (see for example, Newbery and Stiglitz, 1981), arguing that risk preferences should be measured relative to final wealth levels. With imperfect credit and insurance markets, wealth is a *constraint* in the choice set and other

Even if much of the emerging behavioural literature may seem opportunistic, one cannot just ignore it, in order to focus rightful attention to the policy implications of failing markets for the poor. Not least since the work on risk is probably the best established of all work on behavioural economics. Three strands can be mentioned, which despite suggestions to the contrary in most writings on behavioural economics, are not easily squared in a general theory of behaviour towards risk, let alone a clear representation of preferences. First, loss aversion: people may well have a calculus focusing on losses, so that at any point of wealth, a loss is much more heavily valued than a gain. Secondly, as mentioned before, a relatively higher weight is attached to low probability events. Thirdly, when comparing two losses, risk loving behaviour may apply to the larger losses. More specifically, if there is a choice between a moderate loss with certainty, and a far greater loss with some large non-zero probability and a small probability to make only a small loss, in experiments, people mostly choose the latter option: they hold on to the hope that they may not lose very much, even if with small probability, even though the expected outcome could be a very serious loss.

Loss aversion is probably best established and most widely accepted. Its dependence on a reference point is difficult to translate in normative analysis (i.e. what to do about it?). But it does not have to undermine most of the existing evidence on the importance of downside risk for people, not least for those who find it hard to protect themselves. The third issue, on risk seeking behaviour when faced with losses may well be relevant especially during crisis periods: for example, it is well recorded that during crises, such as the 1984-85 famine in Ethiopia, farmers desperately held on to their livestock, rather than selling in time, even at the expense of many of their household members' and their own life. The possibility of losing only little, however remote, may induce this risk-seeking behaviour.

In any case, it would be good for research on risk, growth and poverty to link itself better with the emerging behavioural evidence, and not least push them also to think in terms of policy implications in risk related issues.

2.4 A selected summary of the micro-evidence on risk as a cause of poverty[5]

There are at least three literatures on development issues that have long recognised that risk is an important factor, explaining levels of poverty and deprivation. It is helpful to briefly discuss them, and explain how they fit in with the more general issue of risk as a cause of poverty, as argued in this paper. The first is the fertility literature, where it is commonly argued that high infant and child mortality, i.e. the risk that children will not survive beyond a certain age, increases the fertility rate. Behind this view are more general arguments of the family-level benefits of more labour or old age security, not least in circumstances of limited entitlement to alternative social protection measures, and even though it often puts pressure on women's health and well-being, as well as causing e.g. some of the well-documented externalities on environment, land pressure and well-being of others (for a careful, balanced discussion on these issues, see Dasgupta (1993)). Note that this is an

constraints could be entered in assessing the behaviour towards risk, but this is arguably different from assessing preferences, before constraints on choices are considered.

[5] This section draws on Dercon (2005).

example whereby the risk inherent in living conditions induces 'ex-ante' behavioural responses by households (effectively an over-investment in children), that may well divert resources from more profitable assets.

A second literature is largely based on evidence from agricultural economics although is making a broader point, well-established in basic textbook economics. It focuses on preferences towards risk, and more specifically risk aversion (a preference trait, whereby people are willing to pay to avoid being faced with a risky choice, in favour of a less risky choice). Risk aversion will lead to profitable opportunities not to be taken up in favour of less risky choices with lower expected returns. There is wide evidence of behaviour consistent with risk aversion, and more importantly, of risk aversion to be higher when expected incomes are lower, not least in developing countries (Newbery and Stiglitz (1981), Binswanger (1981)). It leads to a well-established view that the 'poor' are more risk averse, and this will contribute to persistence in poverty, since they will not take the entrepreneurial risk required to enter into particular profitable activities.[6] While some of the points made have some link with this view, it will be argued that its emphasis on preferences is essentially misleading both as a complete theory of how risk causes poverty and as a guide to policy.

A third literature is the nutrition literature, whereby poor nutrition in particular periods in a child's early life may contribute to poorer 'long-term' nutrition circumstances, in the form of stunting (height-for-age levels below some level observed in healthy populations). Short-term shocks to nutrition may then contribute lower nutritional outcomes in the long-run as well, i.e. a persistent health effect. While there is evidence for this process, this is not exactly borne out by all studies: some have suggested that 'catch-up' remains possible: i.e. that *over time* children may recover the lost nutrition and return to their personal growth curve. Again, this is an empirical issue, and most evidence would suggest that stunting is a serious, permanent problem, not least in early years, since evidence suggests a strong correlation between child height at age three and adult height (Martorell, 1995, 1999).[7]

These nutritional effects may have far reaching consequences. Children with slow height growth are found to perform less well in school, score poorly on tests of cognitive functions and develop slower. Adult height is correlated with earnings and productivity, poorer cognitive outcomes and premature mortality due to increased risk of cardiovascular and obstructive lung disease. Taller women experience lower risks of child and maternal mortality. In the case of adults, an increasing body of evidence links adult weight or BMI[8] (the Body Mass Index, also known as the Quetelet Index) to agricultural productivity and wages (Dasgupta, 1993; Dercon and Krishnan, 2000; Strauss and Thomas, 1998; Pitt, Rosenzweig and Hassan, 1990). Low BMI is

[6] The fact that the expanding experimental literature on risk and preferences has questioned the validity of some of the underlying behavioural models for this analysis is not necessarily changing this view. Kahnemann and Tversky's work has shown that 'risk aversion' may not be the appropriate concept, but instead that agents, at any level of income do not like losses, leading to a concept of 'loss aversion'. Ideas of 'safety-first', while seemingly not consistent with most experimental evidence, would also entail preference-led persistence in poverty.

[7] The discussion of the evidence is based on Dercon and Hoddinott (2004).

[8] BMI is the Body Mass Index, defined as weight in kg, divided by the square of height in meters.

correlated with a large number of health-related indicators, including early onset of chronic conditions and increased risk of premature mortality (North, 1999).

Alderman, Hoddinott and Kinsey (2004) trace the impact of the 1982/83/84 droughts in Zimbabwe, as well as exposure to the civil war preceding independence, on longer-term measures of child health and education in the 1990s in a rich panel data set in particular resettlement areas. They focus on shocks if the child is in the critical 12-24 month age category – generally recognised as the most critical time for child growth. These children were interviewed again 13 to 16 years later. Using an instrumental variables-maternal fixed effects estimator, they show that lowered stature as a pre-schooler leads to lowered in late adolescence as well as delays in school enrollment and reductions in grade completion. The magnitudes of these impacts are meaningful. Using careful estimation methods, they found that the 1982/83/84 drought shock resulted in a loss of stature of 2.3 centimetres, 0.4 grades of schooling, and a delay in starting school of 3.7 months for this particular age-group. Using the values for the returns to education and age/job experience in the Zimbabwean manufacturing sector provided by Bigsten *et al*. (2000, Table 5), the impact of the shock translates into a 7 per cent loss in lifetime earnings.

These *permanent* effects from effectively *transitory* events are not restricted to nutrition or health. Lack of insurance and credit markets implies that recovery of assets lost to cope with a crisis or destroyed by it will not be straightforward and immediate. For example, Rosenzweig and Wolpin (1993) show that bullocks are one of the mechanisms used to cope with shocks in their rural South Indian setting, resulting in sub-optimal levels of capital goods. These effects are also not restricted to physical capital: for example, studies in India have found that negative income shocks caused households to withdraw children from schools. Even if children may later on return to school, this causes lower educational levels, affecting the children's ability to build up a better life for themselves (Jacoby and Skoufias, 1995). Recent work on Zambia has shown that teacher absenteeism, closely linked to illness shocks in the context of the HIV/AIDS epidemic, reduces cognitive achievement by children, again affecting long-term outcomes (Das, Dercon, Habyarimana and Krishnan, 2004).

This evidence would suggest processes in which incomes and levels of wellbeing are permanently affected by transitory shocks. It is possible to conceptualise these as poverty traps[9], equilibrium levels of poverty from which there is no possible recovery without 'outside' intervention. One mechanism could be the classic nutrition-productivity poverty trap (Dasgupta and Ray, 1986). It is well established that below some critical level nutritional level, no productive activity of any sort is possible. So if during a crisis, all assets are wiped out except for a person's labour, and if the crisis also pushes the person's nutritional status below this threshold, there is no hope of ever recovering using own productive means. Only a serious windfall, such as in the form of aid, could induce the person to climb out of poverty, provided it is sufficient to pass the threshold value of nutritional status. While the evidence for this to be a

[9]A poverty trap can be defined as an equilibrium outcome and a situation from which one cannot emerge without outside help, for example, via a positive windfall to a particular group, such as by redistribution or aid, or via a fundamental change in the functioning of markets. Poverty traps are often conceptualised as caused by the presence of increasing returns, or a threshold, although other mechanisms are possible, such as credit market failures or externalities. Dercon (2004) has a review of models relevant for poverty analysis, as has Barrett (2004).

direct description of actual poverty traps is limited, it provides a useful narrative for more general poverty traps: there may be thresholds in some productive assets, which, if pushed below them, there is no possible recovery, but rather an equilibrium level of very low asset holdings and poverty. Barrett and Carter (2004) use evidence from Kenya to suggest that such thresholds can be observed at least among pastoralists, given that minimum herd size are required for possible accumulation and leading to 'asset poverty traps'.

The existence of poverty traps has been tested more directly by Ravallion and Lokshin (2000) and Jalan and Ravallion (2004) for Bulgaria and China. Most interestingly, they find no evidence of poverty traps, but in any case, of relatively long persistence of the effects on shocks: it takes many years for them to recover, and the recovery was longer for the poor. Their method exploited the insight that transition paths of incomes or consumption, when poverty traps exist, would be non-linear and allowing for multiple equilibria. Another way of looking at whether there is evidence of long-lasting effects from shocks was used by Dercon (2004), using a subset of the the same panel data households reported in tables 1 to 3.[10]

In this paper, detailed data were exploited on the experience during the 1984/85 famine, more specifically the extent they had to resort to famine coping strategies, such as cutting meals and portions, selling valuables, relying on wild foods and moving to feeding camps. An index of these experiences in the mid-1980s was then introduced in a model of consumption growth based on data from 1989 to 1997, regressing changes in food consumption on initial levels of food consumption at the household and community level and a number of common and idiosyncratic shocks. Note that if shocks only have transitory effects, then lagged shocks should have no effect. However, it was found that rainfall shocks several years before the period in which growth was measured, still affect growth. Most strikingly, the extent of the famine impact, as measured by the index of severity of coping strategies, strongly affected growth in the 1990s. This growth impact was substantial: depending on the estimation method, comparing the 25th and 75 percentile of households in terms of the severity of suffering, the latter had about 4 to 16 percentage points lower growth in the 1990s, a period of on average substantial recovery of food consumption and nutrition levels after crisis and war in the 1980s. Furthermore, it took on average ten years for livestock holdings, a key form of savings and assets for accumulation in rural Ethiopia, to recover to the levels seen before the 1984-85 famine.

[10] This paper did not allow for the non-linearities implied by multiple equilibria, as in more direct tests of poverty traps.

Table 2 Testing for persistent effects of shocks on food consumption growth. Dependent variable: change in ln food consumption per adult between survey waves (1989-94 and 1994-97). Hausman-Taylor and Jalan and Ravallion estimators.

	Δln food cons (1) (HT)		Δln food cons (2) (JR)	
	Coeff	p-value	Coeff	p-value
ln food consumption$_{t-1}$	-0.318	0.000	-0.204	0.000
Village mean ln food cons$_{t-1}$	0.211	0.000	0.135	0.004
rainfall shocks$_t$	0.622	0.000	0.614	0.002
rainfall shocks$_{t-1}$	0.069	0.016	0.195	0.013
adult serious illness	-0.043	0.076	-0.053	0.064
crop shock (-1 is worst)	-0.014	0.757	-0.217	0.041
livestock shock (-1 is worst)	-0.018	0.704	-0.009	0.910
severity of famine impact	-0.116	0.079	-0.397	0.068
Constant	0.519	0.000	0.920	0.071
Number of observations	636		319	

Source: Dercon (2004), table 6. Regression (1) use the Hausman-Taylor model, and assume rainfall shocks, livestock shocks and crop shocks as time-varying, exogenous variables, and demographic changes, illness shocks and lagged consumption at household and village level as time-varying endogenous variables. The index of the severity of the crisis experienced (coping index) was treated as time-invariant exogenous, as was (if applicable) whether there was a road available. As time-invariant exogenous variables and instruments, the presence of harvest failure during the famine period, the estimated percentage of households suffering in each village and the ln of livestock before the famine were used. Regression (2) uses the Jalan-Ravallion estimator (Jalan and Ravallion (2002)).

In general, we have only limited evidence on persistent effects of shocks but this is largely related to the lack of data available for this purpose. Still, careful analysis of available evidence can typically also uncover some of these effects. An example is recent work on the longer-term impact of the Indonesian crisis in 1998. Suryahadi, Sumarto and Pritchett (2003) estimated that the poverty rate more than doubled between the outset of the crisis and its peak, effectively one year. The results in Thomas et al. (2004) suggested that there was some disinvestment in schooling, particularly amongst the poorest households. Subsequently, GDP recovered fast, and positive growth was restored by 2000, and poverty may even have fallen between 1997 and 2000 (Strauss et al., 2004). Lokshin and Ravallion (2005) argue nevertheless that this hides a geographically diverse picture. Using a series of extensive cross-section data sets, they find that living standards in many districts are still affected by the shock, even five years after it began, and three years after the sharp recovery. They suggest that a majority of those living below the poverty line in 2002 would not have done so except for the 1998 crisis: in other words, they experience persistent poverty effects from the 1998 shock.

All this evidence is related to a persistent or permanent effect from a shock, so that uninsured risk is a cause of poverty. There is also evidence of the other effect: that the mere presence of uninsured risk changes household behaviour in terms of investment and activity portfolios. The fertility example at the start of this section can be viewed in this way. Beyond the fertility example, there is further evidence that such behaviour may be directly linked to risk and be a cause of perpetuating poverty. In Morduch (1990), using the ICRISAT sample, it is shown that asset-poor households devote a larger share of land to safer traditional varieties of rice and castor than to riskier but higher-return varieties. Dercon (1996) finds that Tanzanian households with limited liquid assets (livestock) grow proportionately more sweet potatoes, a low-return, low-risk crop. A household with an average livestock holding allocates 20

percent less of its land to sweet potatoes than a household with no liquid assets. The crop portfolio of the wealthiest quintile yields 25 percent more per adult than that of the poorest quintile. Choosing a less risky crop portfolio thus has substantial negative consequences for incomes.

Rosenzweig and Binswanger (1993) suggest that the portfolio of activities (and investments) in the ICRISAT villages is affected by high risk. Increasing the coefficient of variation of rainfall timing by one standard deviation reduces farm profits of the poorest quartile by 35 percent; for the richest quartile the effect is negligible. Efficiency is affected, and the average incomes of the poor decline. Wealthier farmers are not affected and are therefore able to earn higher incomes. This phenomenon affects the wealth distribution: 54 percent of wealth is held by the top 20 percent of households. Jalan and Ravallion (2001) cite other examples, focusing on both asset and activity portfolios, although their evidence is more mixed.

In a careful study, Elbers and Gunning (2003), use simulation based econometric methods to calibrate a growth model that explicitly accounts for risk and risk responses, applied to panel data from rural Zimbabwe. They found that risk substantially reduces growth, reducing the capital stock (in the steady state) by more than 40 percent. Two-thirds of this loss is due to ex-ante strategies by which households try to minimize the impact of risk, i.e. the build-up of livestock holdings to cope with consumption risk. Dercon and Christiaensen (2005), using the same data set on Ethiopia discussed above, find a significant increase in fertiliser use if some insurance were to be offered against downside consumption risk, since when rains fail, financial returns to fertiliser use are typically very low. They reach this conclusion of finding significant sensitivity of fertiliser use to the predicted levels of consumption when rains were to fail, despite controlling for actual current levels of assets, so that the problem is not just a problem of seasonal credit or working capital. They find that fertiliser application rates would increase by about 8 percent if downside risk could be insured.

In sum, there is increasing evidence that uninsured risk increases poverty, through ex-ante behavioural responses, affecting activities, assets and technology choices, as well as through persistent and possibly permanent effects from transitory shocks via the loss of different types of assets. This clearly has important implications for the design of policies, putting policies to reduce risk and the vulnerability it entails at the core of poverty reduction efforts.

2.5 Poverty and Risk - Do we really believe the evidence?

As the previous section has shown, there is a small but growing microeconometric literature on risk as a cause of poverty. It focuses on (a) how shocks have long-term implications due to losses of physical, human and social capital and (b) how uninsured risk shapes behaviour resulting in low-return, low risk portfolios.

But we should be conscious that this evidence base is still weak. On the first effect, very long term panel data are needed, since recall questions tend in general too unreliable evidence. It would be good to replicate further studies. An excellent example is the Hoddinott et al. study on Zimbabwe, referred to above, tracing the impact from a shock (in 1982/83/84), through childhood nutrition, educational

achievement up to (an extrapolation) of earnings. The 'poverty trap' work – including Barrett, Carter and, using very different methods, Ravallion and associates should also be further replicated (and is left for others to discuss).

To conduct such work, a few important issues need to be addressed. First, the panel data need to be long term but also they need to pay particular attention to attrition, implying that tracing of families that have moved away is essential. This is not least important if migration is one way of coping with shocks, while not being able to migrate may also be a consequence of an inability to take risks. Secondly, one needs detailed data on the shocks and events that shape people's lives. As mentioned before, getting a better understanding of the risks people face and the shocks experienced (for example as distinct from slow trends) is difficult but very important.

But the insights gained can be very interesting. Below I report the first evidence from an elaborate long-term panel in Tanzania, in Kagera. The recent work involved an attempt to trace any *individual* interviewed during a four round household survey conducted in 1991-1994. It meant that all split-off households were traced, increasing the household sample size from about 750 to close to 3000. 94 percent of the 'old' households were found back, and 88 percent of the surviving individuals were traced in the region, around Tanzania and even in neighbouring countries. We collected detailed data on the incidence of orphanhood in this area of high HIV prevalence, and investigated the long-term implications of becoming an orphan. Table 3 reports results from the sub-sample that reached adulthood (above 19 or older) by 2004, based on a sample of non-orphans in the baseline period 1991-94, so that the effects are effectively irreversible for both height and (most likely) for education. We find that losing one's mother during childhood reduces height at adulthood by about 2 cm, while orphans have about one less year of education than others.

Table 3: Determinants of Height and Years of Schooling in 2004

	(2)	(4)
	reached adulthood	reached adulthood
	Ln height	Years of schooling
Mother died between ages 0-15	-0.011	-0.757
	[1.87]*	[2.07]**
Father died between ages 0-15	0.001	-0.366
	[0.12]	[1.16]
Number of observations	681	681

Notes: OLS estimates with community fixed effects and robust standard errors.. "Reached adulthood" (n=902) restricts the full sample to those age 19 and older in 2004. T-statistics in brackets. * significant at 10%, ** significant at 5%, *** significant at 1 %. Includes controls for child characteristics (sex and age dummies), baseline characteristics (residing with mother and residing with father; household consumption, flooring material, age, years of education and sex of the household head). The height regressions include the height of the child at baseline and, when available, mother's height from the baseline data. The schooling regressions include the years of education of the child and whether the child was at school in the baseline.

Similar data set being constructed in Ethiopia and India (where all individuals interviewed in the old widely studied Village Level Studies of ICRISAT are currently being traced). This will open up further increased understanding of the long-term

implications of shocks on livelihoods. Parallel work on other data sets should have real high returns.

The evidence on the ex-ante impact of risk is arguably harder to compile. The previous section lists a number of examples, most notably Rosenzweig and Binswanger (1993)'s evidence. Arguably it is methodologically the strongest evidence, but it is not contradicted by work using other methods such as Dercon (1996) on Tanzania or Elbers and Gunning (2003) on Zimbabwe (see also Elbers and Gunning's background note for this study). But they all share one characteristic: the impact is calculated to be huge. For example, in the Rosenzweig and Binswanger case, offering a reduction in the variance of the consumption risk faced by the 'poor' to the level of the variance of the 'rich' would imply an increased return per dollar of assets owned of at least a quarter. To put it simply, protecting the poor as well as the rich can protect themselves would make yearly income of the poor jump up 25 percent. We need much more evidence on this because if these results are robust, then they are huge, and would provide a massive vindication of more activities in this area! The problems with such work should not be underestimated. The key issue is to identify the consumption (or other outcome) risk that households would face when taking on different portfolios. This typically will involve counterfactual simulation in one way or another.[11]

3. From micro to macro-evidence: does risk matter for growth?

3.1 Micro-level poverty traps

The arguments above are suggestive of general arguments of 'poverty traps' – situations whereby even if the household does as well as it can, whoever hard it tries, it cannot escape from poverty. Households lose different forms of capital (financial and physical, human, social capital) due to shocks, while 'ex-ante' behaviour trying to avoid even more harsh conditions results in livelihoods (in terms of activity and asset portfolios) that will have to choose lower risk at the expense of lower returns. We should nevertheless be careful to call this immediately a "poverty trap" – and we return to this below..

[11] In one recent paper, we tried to solve this in a particular way. In Dercon and Christiaensen (2005), we tried to estimate the impact of potential downside risk in consumption on the demand for fertiliser. This is in a context of widespread seasonal credit availability. Still, to control for working capital constraints, the model controls for liquid assets, as well as land and labour endowments. The trick was to first model consumption changes (using panel data) as a function of, inter alia, shocks, such as rainfall, interacted by variables, such as wealth variables, to reflect differential ability to cope ex-post. Then, using historical distributions of shocks (say, the rainfall distribution), we simulate the counterfactual distribution of consumption and generate values if rains were to fail. Assuming rational expectations, and entering the counterfactual log consumption levels into the fertiliser adoption equation. We find strongly significant effects of this 'counterfactual' downside risk-related level of consumption, controlling for working capital, using a fixed effects logit (for the adoption decision) and random effects tobit model on application rates. The impact of risk is shown to be quantitatively important: being able to avoid downside risk in consumption would result in 8 percent higher application rates for those using fertiliser in the sample.

There are three types of 'market failures' that, combined with some non-convexities or thresholds, have been shown to lead to poverty traps (Dercon (2003) has an overview). The first one related to credit market failures. Here, the combination of thresholds plus the impossibility of credit for some people means that (in particular circumstances) the equilibrium configurations implies certain people not being able to get out of poverty despite their best efforts. The second relates to externalities, for example 'spatial externalities'. In these models, when no movement between areas is possible, just the mere fact that one lives in a particular area results in external effects so that despite your best efforts you would (together with the others in the area) remain trapped in poverty. These externalities could be related 'neighbourhood effects', so that, for example, markets cannot be developed or human capital is not worth accumulating.

The third group of poverty trap models relates to risk. There is one class of models that is quite widely developed. They relate to thresholds, whereby a particular threshold needs to be passed (an area of increasing returns could do the trick as well). Examples are Banerjee and Newman (1993, 1995) and some of the more informal asset poverty trap models. They typically assume thresholds, and some other market failure, usually related to credit market failures. 'Shocks' enter in the narrative of the model, as the force that may push households below the threshold and towards a low income equilibrium, and indeed that may make people escape. However, risk has a surprisingly limited role to play in these models: if anything, risk-neutrality is assumed and no behavioural response is effectively modelled. In fact, this is a rather more general result: if we only allow for 'shocks' to which no behavioural response is possible, these models suggest that it could be relatively easy to fall into the trap, but because of the interaction of thresholds and market failures, it would be very hard to escape. But shocks could still make you escape.[12] My reading of the work by Barrett and Carter (2005 and other contributions) is in this spirit: behavioural responses to risk are not explicitly modelled. Still, they are of course very suggestive of possible processes of impoverishment and poverty traps.

Other models do introduce risk behaviour (of the ex-ante behaviour outlined), beyond the effects. But typically, risk preferences (i.e. that risk aversion reduces with wealth) typically are necessary to introduce something like a poverty trap. Banerjee (2005) is an example: some households settle for low return activities to reduce their exposure to risk. The emphasis on risk preferences as the route of a poverty traps is somewhat unsatisfactory, so we should realise that even in the 'theoretical' development there are some more gaps to clarify how risk and poverty may interact. I am aware of current work that tries to remove some of these restrictive preference assumptions to show the presence of poverty traps.

There is therefore still much scope for elucidating the exact role of risk in poverty trap processes, accounting for behavioural responses towards risk. Some may consider this work too theoretical and abstract. But one should not underestimate the role of theory to provide the narrative to take to data.

[12] This leads to a surprising result: once there is the possibility of positive shocks, and if they can be large enough, then there will always be a few lucky ones that can escape. Strictly speaking, there is no 'poverty trap' in which no-one can ever escape without outside policy intervention.

In practice, we should also be careful to just conclude from empirical work that poverty traps are there. Observed long term poverty or other phenomena suggesting that low incomes are being earned for long periods of time is not the same as showing that there are poverty traps. A first reason for caution is that we may 'just' be observing a situation of 'poverty persistence': poverty that lasts for long periods of time but from which own effort can allow an escape in due course. For example, one could slowly save enough to build up enough savings to handle shocks, and then start engaging into more risky, profitable activities.

Against this comment, it can be very reasonably argued that long-term persistence and poverty traps may in practice result in very similar dismal living conditions: the knowledge that you or some generation of your family in the future may escape poverty after many years of harsh conditions offers little solace to a poor family. The distinction between a poverty trap and poverty persistence is then academic. Nevertheless, it is likely to have implications for the design of policies – and more work on this is definitely needed.

A second reason for caution is that a poverty trap typically requires more than just 'uninsured risk'. I also think that it is more complicated conceptually than some recent writings have tried to suggest. But it may help briefly to suggest ways in which risk tends to be treated in the poverty trap literature. Essentially, I like the distinction made by two of the foremost theorists on poverty traps, Ray and Mookherjee. The theoretically best understood models of poverty traps, but, in my view, possibly least useful ones are related to pure coordination failure: multiple equilibria exist, but the combination of market failures (such as the inability to make enforceable contracts with side payments) and certain 'expectations' may make one isolated group settle for a low outcome equilibrium, while another unrelated group settles for a high outcome equilibrium. In a simple sense, any 'shock' could make a group shift to another equilibrium – but it is not a 'shock' as we know it from talking about risk, but an 'expectations' shock. Understanding this intuitively is not self-evident (maybe ask a game theorist – and they will give you a long explanations related to 'shared beliefs' or even 'culture'), but I take it that the 'theory' of how such shocks may function and how another equilibrium may materialise is surely not simply translated into a straightforward policy message. It is not a world in which 'risk' really features, and 'shocks' are just a term unrelated to risk.

The other form of poverty traps – much more familiar from policy analysis, and most relevant for our purposes – is the world of 'history-dependent' multiple equilibria. Here, some unstable equilibria may exist, for example due to what is technically called "nonconvexities" in technology or preferences, linked to increasing returns or similar behaviour linked to threshold effects. The simplest example of such a "nonconvexity" is the presence of some fixed 'threshold' that needs to be overcome to obtain access to a higher source of earnings: in order to go to another livelihood path, one has to be able to get over a short but steep hill, for example in terms of assets. Households with particular configurations of assets and characteristics, just at the bottom of the hill, may be attracted to one particular stable but low-level equilibrium, while another group, with slightly but not much better configuration (who happen to be on top of the hill) may end up in a much higher equilibrium. Furthermore, a shock (that households cannot cope with and with real impact) may then make some households forced to leave the attraction area of one 'good' equilibrium and end up in

another, 'bad' one: they are pushed just past the hill top, sliding down quickly to the bottom.

This type of model is clearly not a simple configuration in which anyone poor is in a poverty trap. For example, simply stating that poor people are trapped in poverty because they have low endowments is not quite the same as 'they face a poverty trap' in a more puritan, but conceptually correct usage of the term. Being 'asset poor' does not have to mean a poverty trap – it is just being very poor. It helps to keep the term 'trap' reserved to the more complicated, but plausible very relevant processes related to thresholds and nonconvexities or other sources of multiple equilibria, such as particular configurations of market failures. A poverty trap exists if no amount of effort of the person trapped can get him or her out.

Of course, much policy rhetoric uses the term 'poverty trap' to referring to some process of conditional convergence, whereby one group may converge to one steady state and another, with other current ('initial') characteristics, to another. I doubt there is much mileage to be had from applying the poverty trap rhetoric to such a context, not least when discussing risk and shocks. The reason is that it may be rather 'easy' to go the other path: just patiently saving to go the higher path (in other words, people can climb out on their own accord), or just offering a small transfer can get them out (say, a small loan or a literacy course). Poverty traps relate to seriously high thresholds that have to be overcome:

There is another problem with the poverty trap rethoric. Thresholds may have to be defined in different spaces. In most simple analysis, the threshold tends to be defined in a unidimensional 'asset' space. Transfer programmes offering enough cash to buy this 'asset' level would then be enough to overcome the trap. This assumes enormous fungibility in the asset space, which in the short run is unlikely to exist. Suppose you need particular minimum combinations of assets to overcome a poverty 'trap': for example, the threshold consists of a plot of fertile land, a pair of oxen, basic education and good health. In technical terms, this is referred to the problem of complementarities, first highlighted in the first contributions on poverty traps by Rodan-Rosenstein. In a world of poverty traps, offering a pair of oxen to a household also lacking basic education and good health is the a waste of money: given the threshold, it would be predicted that the pair of oxen is just wasted, since it can only lift people above the threshold if the other conditions are satisfied. Since building up human capital in the form of health and education takes time, we are faced with a fundamental coordination problem – and a rather depressing message: offering land and oxen now may just not be enough to lift anyone out of the trap.

3.2. From micro-level poverty traps to macro-growth impacts

How important are these micro-linkages between risk, poverty and growth in macroeconomic terms? Does it really affect overall growth? This is at present the hardest question to answer. It is correct to state that we have evidence that risk results in inefficiencies in the allocation of resources, with lower accumulation of human, financial or physical capital as a consequence. Furthermore, because underlying starting levels of wealth are correlated to with ability to cope ex-post with shocks, the poor will face the relatively largest impact in terms of lost income and opportunities.

The micro-evidence above suggest that the earnings lost due to the ex-ante and ex-post effects of risk are substantial, reducing growth for those affected. If many people are affected, then these may well add up to substantial sums with serious implications for the long run growth rate. For example, as the discussion above had suggested, the losses in the Ethiopian economy from the 1984-85 famine or the losses in Zimbabwe or at least in specific areas due to drought and other shocks in the early 1980s would appear substantial enough to expect macroeconomic effects.

However, it is far from established that these effects are really substantial in macroeconomic terms. A key reason is that relatively little work exists that tries to investigate this. As a microeconomist working on the topic of risk, growth and poverty, the macro-growth evidence is a very disappointing. There does not seem to exist a good treatment of the impact of risk on growth, with possibly of few interesting recent exceptions. A recent paper by Aghion et al. (2005) is possibly an exception. They provide a model where by ex-post liquidity risk by firms if a recession where to hit leads them ex-ante to cut back investment, implying 'persistent' or at least 'long-lasting' effects. They also show, theoretically, that the degree of financial intermediation would affect how strong the growth impact would be. To my knowledge, this is the only paper that explicitly has a model of ex-ante risk strategies affecting behaviour, and levels and growth of outcomes at the macro level (see table 1). Interestingly, they link this model to the data, and introduce commodity price shocks (with some different lags) in a model explaining 5-year growth. While I am not entirely clear that they really pin down empirically that 'risk' is affecting growth (it still reads much like 'shocks' doing it), it is an interesting approach. There would be some mileage of trying to do this analysis with a data set more focused on the poorer countries and introducing other shocks (e.g. the recent cross-country data sets on rainfall) into the analysis.

My own preference would be to introduce at least the idea of 'persistence' as in Campbell and Mankiw, and used in Dercon (2004) to study microlevel growth in Ethiopia. The idea is testing whether shocks occurring at t-1 do not just affect growth during t-1 to t, but also t and t+1, then shocks have 'persistent' growth effects. More specifically, if shocks only have transitory effects (the usual assumption in the empirical growth literature, and seemingly also for most of the analysis in Aghion et al. 2005), then lagged shocks should have no effect. However, as discussed before, in Ethiopia, it was found that rainfall shocks several years before the period in which growth was measured and indeed the serious drought and famine of the 1980s, still affect growth.

A second approach would be to expand the Ramey and Ramey (1993) analysis on the impact of commodity price risk (measured by the country-specific standard deviation of commodity prices in a time series) to include shocks more relevant for developing countries. Aghion et al (2005) also re-test their relationship, and allowing for the degree of financial intermediation, and could provide a further basis for more analysis.

Another issue should also be realised and possibly be exploited. To some extent, the growth literature has gone further than the micro-literature I am more familiar with. Two strands of the literature can provide inspiration for building the evidence base:

the literature on conflict and risk of civil strife or war, and the literature on issues of risk of appropriation and other property rights issues. Although little of the underlying analysis is properly dealing with risk, there are insights and evidence that could be fruitfully put together and contrasted with the evidence on other sources of risk. (Note that the microeconomic equivalent of this literature on property rights, looking at tenure insecurity and other property rights issue should also be placed much more directly into the literature on risk, poverty and growth.[13])

But finally, some words of warning is in order. First, there is an emerging literature on 'macroeconomic' poverty and growth traps (e.g. Quah, Agenor). While interesting, the microfoundations of this literature cannot easily be found in the literature on poverty traps quoted above, especially those related to problems of the credit markets or the presence of risk. The reason is that the macro-literature models effectively a representative agent, while the micro-literature (in theory and evidence) clearly shows that different agents (not least the initially wealthy versus the initially poor) are differentially affected by the market failures, so that the poor are forced to be much more inefficient than others. Differential marginal returns or differential total factor productivities is definitely not consistent with a representative agent model. In short, the theoretical basis of some of this macro-work is not based on strong foundations nested in micro-theory and evidence.

Secondly, the micro-evidence quoted before is devoid of general equilibrium effects. The issue is that markets and incentives to others may adjust if some are faced with a micro-level poverty trap. Generating a poverty trap in general equilibrium is quite different from generating in a micro, partial equilibrium sense. Furthermore, while poverty traps may imply that even the best effort of the person trapped will not get him or her out, they may be unravelled by changes in other parts of the economy. When moving to thinking about policy, these issues will need to be taken into account.

Conclusions

This paper had three objectives: first, it aims to take stock of the nature of the evidence available and on the links between uninsured risk and shocks on the one hand, and growth and poverty on the other, both at a macro and micro level. The conclusion is that we have some micro-evidence on a strong impact of uninsured risk and shocks on poverty and growth at the micro-level. However, the macro-evidence is far weaker. We make the case for more work and especially quantifying its relevance in macro-terms. Secondly, we aimed to show some areas in which work is needed. The macro-dimension is one, but we are still lacking much systematic evidence on the relevance of 'ex-ante' impact of risk on activities and portfolios. Linking the emerging evidence from behavioural economics to this literature is also a challenge. For policy makers (in line with the third objective), it is important to realize that the current 'macro' evidence is not as strong as the micro-evidence. Since macroeconomic evidence involves general equilibrium and scale effects, caution in jumping to conclusions is clearly necessary. In other words, we may observe long-term impacts of risk and shocks in micro-data, whether this means that they are

[13] For example, recent evidence from Ethiopia in Ayalew, Dercon and Gautam (2005) suggests that trying to move perceptions of land tenure insecurity from 59 percent with secure rights to 100 percent may increase coffee tree planting by about a third, which would have important earnings implications.

sufficiently important in the context of the macroeconomy, or even in the context of the ability of growth to deliver poverty reduction, is still to be researched.

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