

Livelihoods, conflict and aid programming: is the evidence base good enough?

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In conflict-affected situations, aid-funded livelihood interventions are often tasked with a dual imperative: to generate material welfare benefits and to contribute to peacebuilding outcomes. There may be some logic to such a transformative agenda, but does the reality square with the rhetoric? Through a review of the effectiveness of a range of livelihood promotion interventions—from job creation to microfinance—this paper finds that high quality empirical evidence is hard to come by in conflict-affected situations. Many evaluations appear to conflate outputs with impacts and numerous studies fail to include adequate information on their methodologies and datasets, making it difficult to appraise the reliability of their conclusions. Given the primary purpose of this literature—to provide policy guidance on effective ways to promote livelihoods—this silence is particularly concerning. As such, there is a strong case to be made for a restrained and nuanced handling of such interventions in conflict-affected settings.

Keywords: aid, conflict, evidence, impact, livelihoods, programming

Introduction

Livelihood security and inclusive growth are generally seen to be good for development and peace. By extension, so too are interventions that seek to promote livelihoods and stimulate economic activity. The UN Secretary General's 2006 progress report on the prevention of armed conflict, for example, drew linkages between effective livelihoods and food security interventions and increased stability, arguing that 'tackling food insecurity and related problems of agricultural underproduction and resource scarcity can do much to stabilize a fragile situation' (Alinovi et al., 2007, p. 5). More recently, the 2011 World Development Report (World Bank, 2011) placed significant emphasis on the instrumental role that job creation is seen to play in creating safer societies in the aftermath of war.

Following conflict, therefore, livelihood and economic recovery interventions are often tasked, implicitly at least, with a dual imperative: to generate material welfare benefits (for example, through supporting livelihoods and stimulating local economic activity) and to contribute to peacebuilding outcomes (for example, through raising the opportunity cost of going to war and minimising grievances). This framing may be compelling and appear deductively sound, but catalysing livelihood change and altering market dynamics are complex, highly ambitious objectives for any intervention—

particularly so in the challenging environments of (post-)conflict spaces. In contexts where it is difficult to achieve success against even basic outcome indicators (for example, nutritional status, school attendance) (Torres and Anderson, 2004), the question must surely then be asked: does ascribing livelihoods programming with ambitious transformative objectives, such as promoting stability, represent the kind of ‘premature load bearing’ discussed by Pritchett et al. (2012)? Is it often, in fact, the case that donors and governments are pushing ‘too hard, too soon’, and thus risking failure?

With these questions in mind, the contribution of this paper is to explore the effectiveness of a range of interventions designed to promote the livelihoods of people living in fragile and conflict-affected situations, and to reflect on the nature and quality of the impact evidence base. Through a survey of the impact literature on five interventions, our intention is to provide a useful synthesis of relevant studies and evidence to examine whether claims of the ‘transformative potential’ of programming are backed up by strong empirical evidence, and to contribute more broadly to evidence-informed policy making in this area.

The paper is structured as follows. In the following section, we provide some basic information on methodology and our conceptual framing of livelihood interventions. We then explore the synthesised empirical evidence on a programme-by-programme basis. Interventions covered include (in order): microfinance; value chain development; ‘making markets work for the poor’ (M4P); job creation programmes; and skills training. In the following section, we broaden out the analysis and ask: how far does the evidence base actually take us? We make six key points about the nature of the literature in order to inform the way researchers and decision makers handle evidence in the future. Finally, we conclude with some brief remarks and observations.

Methodology

This paper is based on a longer working paper, produced during the inception year of the Secure Livelihoods Research Consortium (Mallett and Slater, 2012).¹ The purpose of the working paper was to synthesise available empirical evidence on economic growth and livelihoods in fragile and conflict-affected situations, with a view to taking stock of the evidence base, identifying key themes and findings, and highlighting knowledge gaps. The methodological approach (detailed in Mallett and Slater, 2012, pp. 100–106) involved three iterative stages: i) a three-track evidence gathering exercise; ii) an ongoing process of synthesis and analysis; and iii) the inclusion of insights from leading experts in livelihoods and growth in reference to fragile and conflict-affected situations.

Of particular relevance here is the approach to evidence gathering. As mentioned, this involved three mechanisms. First, researchers undertook a series of systematic reviews into the impacts of selected interventions in fragile and conflict-affected

situations, including M4P, which is reported on below (for more information on the use of systematic reviews in international development see Hagen-Zanker et al., 2012; White and Waddington, 2012).² Second, researchers used a snowballing approach to identify relevant material, starting with a list of studies determined by recognised experts working on livelihoods and economic growth (again, in reference to fragile and conflict-affected situations). Third, in addition to the above, a more orthodox literature review was undertaken, involving the use of academic databases, institutional websites and internet searches.

A number of key themes emerged from the subsequent analysis, one of which was the effectiveness of aid programming in conflict-affected situations. Evidence within this thematic area is concerned principally with the practical mechanics of promoting economic activity and supporting livelihoods both during and (particularly) after conflict. Much of the evidence comes from donor reports and is used to inform ‘lessons learned’ documents and policy guidance notes.

While some of the evidence concentrates on broad policy measures, such as investment climate reforms and public financial management, our focus here is on livelihood promotion interventions targeted primarily at the micro level (that is, individual, household, community). Livelihood promotion is one of three types of livelihood intervention, the other two being livelihood provision and livelihood protection (Jaspars and Maxwell, 2009). These three types of intervention differ primarily in terms of their main objective(s) (Table 1), although it is recognised that interventions are often tasked with multiple objectives at the same time. Livelihood promotion interventions, broadly speaking, aim to go beyond immediate needs by creating new assets, improving the economic productivity of beneficiaries, and improving people’s access to markets and services (Jaspars and Maxwell, 2009, p. 9). It is this type of intervention that tends to be mandated with the most ambitious objectives, which is why livelihood promotion interventions constitute the focus of this paper.

Before moving on to the main section of this paper, it must be emphasised that the original reviews on which it draws were conducted in 2011/12. An evidence base is

Table 1. Examples of livelihood interventions, by category

Type of intervention	Livelihood provision	Livelihood protection	Livelihood promotion
Objective(s)	Directly affecting outcomes through meeting basic needs and contributing to personal safety	Protecting assets and preventing negative outcomes	Improving strategies, creating assets, enhancing access to markets and supporting appropriate institutions and policies
Examples (as covered in Mallett and Slater, 2012)	<ul style="list-style-type: none"> • Food aid • Cash • Public works programmes • Infrastructure projects 	<ul style="list-style-type: none"> • Seeds-and-tools • Seed vouchers and fairs • Livestock interventions 	<ul style="list-style-type: none"> • Microfinance • Value chain development • M4P • Job creation • Training

Source: authors, drawing on Jaspars and Maxwell (2009).

not a static or monolithic thing, but is always on the move. While the lag time between carrying out the reviews and the publication of this paper does not discredit the analysis presented here, readers must be aware that the most recent contributions to the literature are not captured.

Do we know how to promote livelihoods in conflict? Evidence from a menu of five interventions

Aid-funded livelihoods programming during and after conflict has attracted a substantial literature. Much of this literature seeks to answer the question: what kinds of interventions should we be looking to implement in order to improve people's livelihoods in these difficult contexts? As a result, one does not have to look too far to find a mass of papers offering policy recommendations and programme guidance. From the perspective of the 'development community', as it were, the existence of this wealth of information is unsurprising: such papers are often written with the express aim of informing policy makers and practitioners. Their purpose is to guide and advise—which is perfectly understandable.

Drawing on a survey of the literature, this section lays out in brief but illustrative terms what the evidence base for five livelihoods interventions tells us about their effectiveness in conflict-affected contexts. For various reasons, it is not possible to delve into the specifics of programming, which is unfortunate: there is an important place for nuanced discussions of individual programmes. Nor is it our intention to provide direct, actionable guidance of our own making to those in the policy and practitioner communities. This is not a 'how to' piece, to be used to determine what should be implemented when and where. Rather, by drawing on illustrative examples, our aim is to sketch out an overview of what the literature tells us about the impacts of the five interventions under consideration—an overview that will be necessarily problematised in the following section. We begin with microfinance, before moving on, respectively, to value chain development (VCD), 'making markets work for the poor' (M4P) programmes, job creation programmes and training.

Microfinance

Microfinance is among the most widely discussed of livelihood interventions in fragile and conflict-affected situations. Drawing on empirical work from Matul and Tsilikounas (2004) and Santos (2003), the German Technical Cooperation (GTZ, 2006, p. 20, emphasis in original) claims that 'microfinance can serve as perhaps *one of the most efficient instruments* to support household incomes, self-employment and micro-enterprise formation during post-conflict reconstruction periods'. Frasier and Bne Saad (2003, p. 7) additionally suggest that microfinance has the potential to facilitate 'a smooth transition from short-term humanitarian assistance to longer-term development'.

However, our survey of the literature suggests that claims such as these are thinly evidenced. It may well be that microfinance is simply assumed to work, almost regardless of context—although this is increasingly being shown not to be the case (see Duvendack et al., 2011)—which would also help explain why the vast majority of the substantial literature on microfinance and conflict focuses on the technicalities of programme design and implementation rather than on impact. Indeed, a range of lessons, often derived from donors' own experiences, are available to practitioners wishing to deliver a microfinance intervention in a conflict-affected environment (see, for example, ESCWA, 2009; Nagarajan, 1999; USAID, 2009). But the empirical basis for such lessons is not always clear. Discussions of methodology and data are often limited to a brief statement assuring the reader that the agency in question has previous microfinance experience in conflict settings, and it is also sometimes unclear whether the evidence and lessons presented are particular to conflict-affected situations or drawn from a broader experience of delivery in more stable contexts. Indeed, we often simply do not know what the empirical basis for claims and conclusions consists of.

Nonetheless, microfinance and microcredit programmes have been supported and implemented quite extensively in fragile and conflict-affected situations, both by governments (see IMF, 2006; Longley et al., 2006) and by non-governmental organisations and donors. Generally speaking, evidence of the impacts of such interventions, assessed through programme evaluations, appears mixed. For example, an evaluation of an American Refugee Committee programme in Guinea found significant increases in capital assets among clients progressing from start-up grants to basic and advanced loans (de Klerk and Nourse, 2004).³ But when compared, the average size of capital assets owned by those who received a grant or loan did not differ significantly from those of the control group. Therefore, although it would have been difficult (if not impossible) for the more vulnerable among the refugees to develop their businesses without the grants and loans, there is no solid empirical evidence that the growth in asset portfolios was a result of access to microcredit through the programme (de Klerk and Nourse, 2004).

On a different note, van de Walle (2002) shows how certain credit schemes—such as one in Yemen where poor households were given 10 goats or five goats and a cow, taught to raise them and then expected to pay back 60 per cent of their value within two years—can carry significant risks for families who must pay back loans even if the animals die, leading to negative impacts on income. An alternative to such traditional microcredit interventions can be seen in PACT's Women's Empowerment Program in Nepal, which enabled 86,000 women to start a business over four years through the mobilisation of a total of USD 1.18 million in group savings. This demonstrated that, even in a country in the midst of conflict and poverty, women can build equity (rather than debt) to facilitate livelihood promotion activities (Ashe and Parrott, 2001; see also Upreti et al., 2012, for a discussion of this intervention).

It is the government of Afghanistan that has arguably gone furthest of all in terms of taking a leadership role in the microfinance sector in its country, with the establishment of the Microfinance Investment Support Facility for Afghanistan (MISFA) in 2003. A number of studies and assessments of MISFA have been carried out over recent years, the findings of which effectively illustrate the mixed nature of the evidence on microfinance in fragile and conflict-affected situations more broadly. We examine these studies in Box 1 below.

Value chain development

The fundamental objective of value chain development (VCD) is ‘to move poor individuals and households out of saturated, low-return activities and into higher-return, growing markets’ (Parker, 2008, p. 1). It involves ‘linking poor producers

Box 1. Competing findings: the case of the Microfinance Investment Support Facility for Afghanistan (MISFA)

In a major quantitative evaluation covering 79 per cent of the client population in nine provinces of five Afghan regions, Greeley and Chaturvedi (2007) found early signs that MISFA was having a major impact in terms of job creation. Expansions and start-ups were providing job opportunities for 414 entrepreneurs and allowing 264 clients to employ other people, from both within and outside the household, either full or part time. They estimated that every client generated one and a half employment opportunities, which, when extrapolated to all MISFA clients, gives a total of 500,000 jobs. MISFA also showed signs of success at the household level, with the rate of incidence of crisis situations lowest among clients and a statistically significant difference in the rate of incidence between clients, non-clients and dropouts. The evaluation also highlights positive findings in relation to gendered power relations and perceptions, with 44.1 per cent of women clients reporting ‘absolute control’ over the money they earned as opposed to 18.4 per cent of non-clients and 37.2 per cent of dropouts, and 80 per cent of female respondents reporting an ‘improved attitude’ towards themselves on the part of their husbands and other relatives since they joined the programme.

Despite the positive outcomes reported by this earlier evaluation, however, the effectiveness of MISFA can be questioned on a number of counts. Pain (2012), drawing largely on the primary research of Kantor (2009), covers some of these. First, objectives matter: the needs of clients, not the objectives of policy makers, ultimately determine the use of microcredit. Microcredit through MISFA has emphasised the provision of credit for productive purposes in order to stimulate economic activity and growth. However, evidence from rural areas points to its significant use to meet consumption smoothing requirements rather than productive investment. Thus, development of financial instruments to create social insurance has been limited and secondary to efforts to drive market-based engagement for the poor.

Second, context matters. There is a broader issue about the general lack of interest and understanding of how new, formal rules, organisational structures or programmes engage with existing social structures and informal institutions. Demand for microcredit exists in Afghanistan, but so does a vibrant informal credit system. MISFA microcredit thus constitutes just one option among many, and may not even be the best. Indeed, Kantor shows that people often draw from multiple microcredit systems, sometimes using informal credit to pay off MISFA loans, suggesting a build-up of informal debt. More generally, microcredit and informal credit markets are linked to such an extent that the success of the former may ultimately depend on clients’ access to the latter in order to support repayment of the microcredit loan.

Third, measurement matters. Indicators can be selected to support the public text of success without necessarily questioning or exploring what might underlie this. Early reporting on MISFA focused on the number of microfinance loans dispersed, the number of groups established and levels of repayment, without critically assessing how these loans were repaid and the economic benefits that might have accrued from them. In other words, the focus was placed on measuring outputs, which, while useful for gauging the internal effectiveness of MISFA (that is, how successful the project was in meeting its coverage), provided little insight into beneficiary outcomes.

to other private-sector actors who have access to growing markets and who have a clear business interest in partnering with poorer producers as part of their supply system' (Parker, 2008, p. 1). The available evidence on VCD in conflict-affected situations is broadly similar in nature to that on microfinance: dominated by technical guidance. Indeed, there is a rich literature on practical lessons from value chain projects in conflict-affected situations. Of particular note are papers from USAID's Value Chain Development in Conflict-affected Environments Project, which include case studies on, among others, the cotton industry in northern Uganda, tourism in Rwanda, fisheries in Sri Lanka, the poultry sub-sector in Afghanistan and the dairy industry in Kosovo.⁴ Parker (2008) synthesises these case studies, covering a total of 14 value chains in conflict-affected environments. Amongst other findings, the author concludes that VCD projects can generate significant economic results in terms of sales, employment and private sector investment (see Grygiel, 2007, on Kosovo; Henning et al., 2008, on Rwanda), and can be used to reach vulnerable populations, even amidst conflict (see Locke and Byrne, 2008, on northern Uganda; McMahan, 2008, on Afghanistan). While these are surely important lessons, the specific methodologies underpinning their recommendation are, once again, unclear. Furthermore, reports in this particular USAID series on VCD sometimes conflate output with impact.

'Making markets work for the poor' (M4P) programmes

M4P projects are systemic attempts to understand where market systems are failing to serve the needs of the poor and to correct them accordingly. They are a relatively novel approach to economic engagement in conflict-affected (most notably post-conflict) situations.⁵

As explained in the previous section, as part of the research process for our review, we undertook a systematic review of the impacts of M4P projects in fragile and conflict-affected situations. Out of 483 studies originally retrieved, just three were found to meet the criteria for final inclusion. It is therefore difficult to make statements about the effectiveness of M4P programmes based on this particular systematic review. While the three studies suggest that M4P can lead to increases in incomes and social cohesion amongst beneficiaries (via an accumulation of trust within economic networks), overall the key finding was that there is currently insufficient evidence on the applicability and impact of the M4P approach in situations of significant conflict and fragility (see Carpenter et al., forthcoming). Undoubtedly, further research is needed to test whether the impacts identified above can also be achieved using the M4P approach in such situations; for example, can they more generally facilitate increased social cohesion in these settings? This is an interesting question, particularly given the success of USAID in funding business projects in Kosovo that have helped bring together Serb and Albanian groups in the production and marketing of food commodities (UNDP, 2008). Another particularly pertinent question in these situations is whether, if grounded in comprehensive political economy and conflict analysis, they can effectively help facilitate the (re)inclusion of vulnerable groups, particularly women, cut off from markets as a result of conflict.

Job creation programmes

Given that employment creation programmes seek to create both jobs that transfer income to poor people and durable assets that have lasting value (Devereux and Solomon, 2006), they can be considered a livelihood promotion intervention. The literature on job creation relates mainly to efforts to create waged employment in the formal sector, and tends to be associated with short-term public works projects and short-run/emergency job creation (for example, food and cash for work), usually funded and implemented by the public sector. Again, the evidence on job creation tends to be provided by practical donor guidance and assessments of effectiveness, rather than on academic literature, which is relatively limited.

A look at the policy discourse around the need for jobs in conflict-affected settings makes it clear that a number of assumptions are frequently made about the violent causality of certain groups (usually young men) being unemployed. As Sommers (2006) observes, this line of thinking is closely connected to ideas about the youth bulge, which became politically influential among Western foreign policy communities from the 1990s onwards. Indeed, several writers have made similar points to Sommers (2006, p. 3), when he argues that ‘the youth bulge theory implies that young men with constricted options will automatically and necessarily respond with violent rebellion’.

However, there is a worrying lack of convincing evidence contained within the literature on job creation programmes to support such assumptions. In a background paper for the 2011 World Development Report, Cramer (2010) notes that the empirical data on the relationship between unemployment and violence, in general, are simply not there:

There is no remotely convincing evidence at the cross-country, large-N level, at the quantitative case study level, or at the ethnographic, ‘qualitative’ level, for any bold claims that unemployment is a mechanistic causal factor in violent conflicts in developing countries. The evidence on youth unemployment is even weaker (Cramer, 2010, p. 24).

Two recent reviews of the evidence on employment programmes in conflict-affected situations appear to back up Cramer’s conclusion. The first is a rapid mapping study by Walton (2010), which reviews donor approaches to addressing armed violence through youth job creation programmes. The central finding is that, although such approaches have become more nuanced and sophisticated—attempting to address a range of factors that cause social exclusion by combining with other forms of intervention, such as training in conflict resolution—both the ‘theoretical and the empirical cases for using youth employment programmes as a stand-alone tool for reducing violent conflict are extremely weak’ (Walton, 2010, p. 1). Walton (2010) identifies the main gap in the current literature as:

a lack of critical analysis of the impact and broader social, economic and political effects of youth job creation programmes [. . .] This gap is closely related to a failure to thoroughly articulate and examine the mechanisms via which job creation programmes impact upon local or national-level conflict (Walton, 2010, pp. 10–11).

The second is a recent systematic review into the impact of employment creation on stability and poverty reduction in fragile and conflict-affected situations (Holmes et al., 2013). This identifies just seven relevant studies, the majority of which either present anecdotal evidence or are based on secondary literature reviews. Further, the authors state that the policy literature ‘in this area is primarily intuitive [. . .] and based on the assumption that if unemployment contributes to instability, then employment creation will promote stability’ (Holmes et al., 2013, p. v)—an assumption, it seems, that the empirical evidence does not substantiate. The central finding of these two reviews is fairly damning: we simply do not know what the impacts of job creation programmes are.

Skills training

Training is an example of an intervention intended to promote greater ‘employability’ of individuals. By aiming to contribute to an improved supply of suitable labour, it serves as a complement to other types of intervention designed to increase the demand for labour. In addition, training is often promoted in fragile and conflict-affected situations, particularly in post-conflict situations, as a means of getting people into jobs, especially in terms of reintegrating youth and ex-combatants and rebuilding livelihoods.⁶

The literature contains evidence from a number of vocational training programmes, which shows there is often a mismatch between training and jobs. This evidence suggests that a failure to connect training to current and future demand on behalf of employers leads to a failure to meet the high expectations of ex-combatant participants (McKibben, 2011; UNDP, 2008). To select from an abundance of examples, only a quarter of the ex-militia forces trained in the New Beginnings Programme in Afghanistan secured long-term employment, with one ex-combatant noting that the programme ‘promises things [but] no one gives us a job [and] we are all so disappointed’ (Bhatia and Muggah, in McKibben, 2011, p. 19). Similarly, only 13 per cent of ex-combatants surveyed in Colombia who underwent a vocational training programme were subsequently employed (Arjona and Kalyvas, in McKibben, 2011).

Furthermore, across the programmes reviewed by McKibben (2011), there was a general failure to attend to psychological problems, and training for women ex-combatants, when conducted at all, was intended to support reintegration into domestic life but not the workplace. Failure to address mental health problems is especially significant in northern Uganda, given that 25 per cent of producers and traders there said they would not employ someone with mental health problems, yet 25 per cent of male youth experience moderate to high levels of emotional distress, and ex-combatants are 50 per cent more likely to experience emotional distress than those not previously involved in the conflict (McKibben, 2011). In addition, Grossman et al. (2009) argue that formal skills development courses are often inappropriate in many contexts, as they are usually based within a structured, officially approved system where entrance is regulated and which provides access to an officially recognised

qualification. This kind of training lacks relevance in many conflict-affected contexts, where formal sector employment is usually difficult to find. Further, the costs of these kinds of formal training courses are high (on a per capita basis), thereby limiting their reach. Instead, the authors recommend the provision of training courses, which have a lower educational entrance requirement and which are flexibly organised to meet the needs of the trainees.

However, not all training programmes in fragile and conflict-affected situations have been unsuccessful. For example, in northern Iraq, the Food and Agricultural Organization (FAO) and the UN Industrial Development Organization (UNIDO) implemented technical and business management training that led to 61.3 per cent of beneficiaries who were unemployed before training gaining employment or self-employment by the time of the evaluation (UNIDO and FAO, 2010). Another important dimension of this project was that it enhanced capabilities in the Ministries of Industry and Agriculture, thus potentially contributing to increased project sustainability and institutional strengthening. Meanwhile, in Aceh, a GTZ vocational training programme achieved a major impact by controlling for the factors that often contribute to the vocational training-jobs mismatch (see McKibben, 2011).

How far does this take us? Reflections on the fragile state of the impact evidence base

The evidence cited above under each programme heading may not capture all that is out there, but it does tell us some important things about the nature of the evidence base more generally. We centre this section on one overarching question: how far does the evidence (on programme effectiveness in fragile states) take us? We attempt to answer this through a critical engagement with the literature, drawing on our own review work as well as the critical contributions of others. The aim is to highlight key issues or concerns in order to inform the way researchers and decision makers handle evidence in the future. In a sense, the following six points can be considered ‘things to watch out for’ when navigating one’s way around a given evidence base.

1. Policy guidance is often based on minimal evidence

There have been important recent moves to improve the quality of evidence on the effectiveness of development and humanitarian programmes. We have seen the emergence and growth of important organisations tasked with generating and promoting the use of rigorous impact evidence, such as 3ie and the Active Learning Network for Accountability and Performance (ALNAP), as well as evidence- and methods-focused conferences such as the Big Push Forward programme.⁷ However, this does not remove the flaws affecting much of the literature published already. Our review of the evidence (at the time) was striking in that so many reports cited minimal empirical evidence to back up claims and recommendations—recommendations that were principally tasked with informing decision makers. Most of the

time, it was not the case that weak or questionable evidence was drawn on. Rather—and, arguably, more worryingly—data sources were simply not visible at all. The central question this raises is: how can an audience trust the findings? In short, the recommendations featured in many papers might be well-grounded, valid pieces of advice, but without reliable information on their foundations, they are essentially unusable from an evidence-informed policy perspective.

In addition, there are some questions regarding the trustworthiness of many reviews of the literature (that is, sources of secondary research). Such reviews are often intended to distil vast bodies of information into a series of key messages and findings. It is only relatively recently that an explicit focus on the quality of evidence has entered the *modus operandi* of many literature reviews in international development. This is partly a consequence of a stronger emphasis being placed on empirical information by those funding review research.⁸ However, a cursory reading of many synthesis reports based on secondary research methods reveals a tendency to take the findings of primary research studies for granted. In effect, findings are regurgitated unproblematically and free of critical perspective. In the worst cases, it is possible to find literature reviews uncritically repeating the findings of previous literature reviews. Against this backdrop, the recent emphasis on method type and evidence quality within mainstream discussions on development policy and programming is welcome, if not at times problematic (see footnote 8).

2. There is a focus in the literature on the technicalities of programme design rather than the socio-political realities of implementation

The way in which many documents with an explicit policy focus have been written is very prescriptive. They set out to tell people how to get the details of programme design right, based on an analysis of how well or badly things have worked in the past. There is often an assumption here that what has worked in one place can also work in another, despite the mounting evidence advising extreme caution vis-à-vis results transfer and extrapolation (Pritchett and Sandefur, 2013).

There are perhaps three main points to make about the predominance of technical discussions in the literature. The first is that important place-specific factors tend to be treated as secondary. It is quite common for authors to argue that ‘context matters’ but go little further. Designing and implementing livelihoods interventions in places affected by conflict is particularly difficult for a reason. For one, the introduction of new forms of physical and financial capital into settings that have recently seen violent conflict may, in some cases, contribute to the development of war economies. In such places, understanding what created conflicts and crises in the first place must be part of efforts to plan an appropriate response (Collinson, 2003).

Second, all aid interventions shape the social, economic and political dynamics of local areas (the ‘implementation sites’), but these will differ from one intervention to the next. There are multiple factors (some known, some unknown) that shape what kinds of outcomes occur, but programme design is certainly one thing that

plays a role. There is evidence, for example, that greater visibility of aid transfers may increase levels of violence in beneficiary areas through the (unintended) creation of high profile targets (Crost et al., 2014). Thus, we return to the point just made: understanding why and through what means conflicts manifest in the first place is an important part of understanding what kind of intervention to implement. Where conflicts involve widespread looting of households' physical capital, for example, programmes built around asset transfer may not be appropriate.

Third, a focus on the technical obscures the deeper ideological grounding of livelihoods interventions. There are normative dimensions to how aid agencies view their choices when it comes to programme selection. One could make a plausible case that many of the kinds of livelihoods interventions discussed here—microfinance, value chain development, skills training—have a strong neoliberal capitalist bent to them insofar as they orient beneficiaries' livelihood strategies towards market institutions. But by constantly asking how to do things right, in quite a narrow sense, those concerned with post-conflict recovery essentially take these ideological questions for granted. This is perhaps a difficult one to address given that pressures to act relatively quickly work against taking a step back.

3. Too much of the time, the evaluation literature privileges output over impact

When an evaluation is designed to assess whether something in the real world has got better or worse, it must be concerned with measuring outcomes and impacts. Certainly, this is what one would expect to see from a report claiming to show evidence of whether an intervention has worked. While the science of evaluation has increasingly been taken more seriously within international development, the literature on 'impact' is replete with cases where authors have written about outputs rather than impacts. That is, their concern is with whether a particular intervention has met its own internal goals, such as hitting a distribution target or completing the construction of some infrastructure within a set timeframe. While this tells us something about the implementation of a programme, it fails to generate any information about whether the programme has made the end-users (or beneficiaries) better off or safer. This problem is related to the way in which monitoring systems tend to operate. As Knox Clarke and Darcy (2014) put it:

Organisational programme monitoring systems tend to concentrate on the degree to which inputs have been converted successfully into outputs, rather than on the way in which these outputs have influenced levels of need. One humanitarian organisation calculated that only 13% and 32% of its monitoring systems were capturing information on the actual effects of programmes on the humanitarian context (Knox Clarke and Darcy, 2014, pp. 32–33).

Thus, attempts to generate information on programme performance are often designed to meet the bureaucratic interests of technicians and funders. These interests have not emerged out of a vacuum, but are a result of—and are embedded within—

organisational reporting and accountability structures, which have tended to demand assessments of how well ‘inputs have been converted successfully into outputs’, to use the words of Knox Clarke and Darcy above. That said, there are promising moves towards better impact measurement within the donor community, with bilateral agencies such as DFID now placing a greater emphasis on the need to evaluate their programmes from the perspective of whether they are actually contributing to poverty reduction and addressing the underlying causes of poverty (DFID, 2014).

4. Where information on impact is available, it is usually inconclusive

Much of the time, the problem with the evidence base on programme effectiveness in conflict-affected places is that very little exists. But even where evidence is available, there can still be issues. This is partly related to context and concerns around extrapolation. A systematic review might claim that a body of 25 impact studies on a particular intervention (school feeding programmes, for example) is ‘large’. In relative terms, this may be a valid classification—the number of impact studies of other interventions may be far lower. But the degree of contextual variation from one place to the next—in terms of cultural practices, social norms, institutions, governance structures, local economies—make generalising the findings of those 25 impact studies quite problematic. It is also possible, as we have found in our own review work, that an evidence base may be quite concentrated geographically. Out of 25 studies, for example, seven may have been conducted in (a certain part of) Kenya. Much of the time, therefore, an evidence base on a particular intervention is not representative across multiple countries and regions.

Another issue concerning this theme is whether the findings of multiple impact studies (assuming multiple exist) actually tell us anything coherent and consistent. It is quite common for different studies to reach different conclusions about impact; one need only look as far as microfinance to observe the extent to which claims regarding effectiveness differ. This is not a problem per se; policy makers should not expect a single intervention to work everywhere, every time. What is problematic, however, is when we lack causal evidence that tells us why something has worked in this place and at this time, and why it did not in another place at another time. Without an understanding of the conditions that enable a livelihood intervention to create the impacts it sets out to, programming will invariably suffer. This is why investigating failure is just as important as measuring success (Harford, 2012).

5. The underlying assumption that unemployment fosters violence and instability is questionable

Recent discussions within the international donor community have re-energised the objective of creating jobs within the world’s fragile and conflict-affected economies. In the past few years, two World Development Reports have championed the need for more jobs in countries emerging from war (World Bank, 2011; 2012), and the World Bank’s President, Jim Yong Kim, has argued that the ‘creation of many new

jobs' can help a 'fragile state lose its fragility' (Kim, 2012). A particular logic underlies this enthusiasm: unemployed youth are normatively framed as a threat to stability and economic growth, and, in order to raise their opportunity cost of participating in political violence, must be given jobs.

The enthusiasm expressed by the World Bank and others for jobs as a means to growth and stability is mirrored by the general consensus within the wider literature that employment creation programmes are important in fragile and conflict-affected situations, particularly in post-conflict environments (Beasley, 2006; Collier, 2007; ILO, 2010; Mendelson-Forman and Mashatt, 2007; USAID, 2009). The work of Collier (2007) has been influential in this respect; once peace agreements have been signed, 'dangerous young men' should be given jobs in order to raise the opportunity cost of fighting and subsequently to buttress peacebuilding efforts. The logic of this is appealing and may appear deductively sound, but it unfortunately fails to take into account both the complexities of labour markets and the realities of impact.

As discussed above, however, the policy discourse surrounding job creation in conflict-affected places does not square with what we actually know about the relationship between (un)employment and participation in violence. Upon first look, it may appear obvious that giving someone a job—filling their time with 'productive' economic activity—might contribute to a better livelihood for that person and a lower likelihood of their 'taking up arms' (if the conditions enable such activity). But any form of employment, in economies both affected and not affected by conflict, must be understood from a labour market perspective. Job creation is not a simple case of responding to unemployment by providing more jobs, nor a technical exercise in matching supply with demand, but an intrinsically political and social issue (Kabeer, 2012). True, there are many technical aspects to programme design and implementation (see ILO, 2010), and there is a fair amount of existing programmatic evidence that provides a valuable resource to donors and implementing agencies in this regard (see Mallett and Slater, 2012, p. 49). But new jobs do not emerge in a vacuum or on a 'blank slate'.

War distorts and reconfigures labour markets, introducing new dynamics that shape access and participation. And while conflict destroys some markets, it creates others, often brutally. While these markets emerge in response to wartime economic activity and are therefore often illicit in nature, their characteristics (weak regulation, high risk, high return, high rent) all create a demand for labour (Cramer, 2005). In addition, conflict can increase the fluidity of labour market dynamics, meaning that a job creation intervention that might make sense at one point in time may not at another.

More importantly still, the evidence available suggests that the specific nature of employment—the terms, the limits, the dynamics of an individual's economic activity within a particular labour market—must be part of any analysis into what a job means for effects on livelihoods and violence. Again relevant here is Cramer's (2010) World Development Report work. Following an assessment of the literature, he concludes that it is often the experience of employment—rather than *un*employment—

that drives people to participate in political violence: poor and exploitative working conditions, extremely low pay and a lack of formal mechanisms through which to express dissatisfaction all help create the conditions for violence. Evidence from Sri Lanka that ethnic discrimination in labour markets was a driver of conflict (Mayer and Salih, 2006) both reinforces this point and illustrates how labour markets tend to be shaped heavily by broader societal structures and dynamics. Raeymaekers (2011) offers further evidence on the politics of labour markets, this time from Butembo in eastern Democratic Republic of Congo. He finds that, although displaced youth are keen to pursue urban- rather than rural-based livelihoods, 'youth access to decent jobs is severely blocked, not so much by their social capital [. . .] but rather by a murderous market equilibrium' (Raeymaekers, 2011, p. 27). Access to decent jobs is controlled by a closed group of oligopolistic gatekeepers, meaning the governance of local economic activity is highly exclusionary and deeply political. Labour markets, therefore, are as much about dynamics of power as they are about equations of supply and demand.

Unfortunately, however, the lack of empirical data on the relationship between (un)employment and violence is symptomatic of research on labour markets in conflict-affected situations more broadly. As Cramer (2010, p. 2) notes, 'the rapid growth of interest among development economists in the past twenty years or so in violent conflict and its aftermath in developing countries has made many advances but has devoted very little attention to labour markets'. As a result, with the exception of a small number of studies (Adam, 2008; Aysa-Lastra, 2011; Beall and Schutte, 2006; Calderón et al., 2011; Ibáñez and Moya, 2009; Walraet, 2011), we still know relatively little about how conflict affects labour markets and how these changes affect people's livelihoods. This can be partly accounted for by the complexity involved in understanding labour markets and by the challenges associated with making sense of their social and political characteristics.

6. The focal points of the mainstream livelihoods and conflict policy discourse are not always reflected in the objectives of programming

There is an apparent disconnect between, on the one hand, the overarching policy narrative vis-à-vis the instrumental role that jobs and livelihoods are seen to play in contributing to peacebuilding and, on the other, the realities of livelihood programming 'on the ground'. While post-conflict livelihood interventions are frequently assigned this transformative potential by key spokespersons of the international policy community (see, for example, Kim, 2012), it is clear that achieving specific peacebuilding or social cohesion outcomes is quite often absent from the design of interventions in conflict-affected places. In short, despite what the mainstream policy discourse might say, many (if not most) livelihood programmes ultimately remain concerned with the more orthodox material objectives of income generation and asset accumulation. This is more pronounced among certain interventions than others. For example, while some value chain programmes feature a clear design focus on

strengthening horizontal relationships between different social groups (Parker, 2008), there is very little evidence to suggest that building social cohesion comes anywhere near the *modus operandi* of the average microfinance venture.

Conclusion

Overall, with reference to our original research question, we find that claims of the ‘transformative potential’ of livelihood promotion interventions in conflict-affected situations are not backed up by strong empirical evidence. Perhaps as a result of the potency of conventional wisdoms and narratives of success, there does not appear to have been, up until recently at least, much of a culture of doing impact assessments—something that may be partly the result of a lack of institutional space for robust critique and admissions of failure (Carr, 2013). Moreover, many evaluations appear to conflate outputs with impacts, the former of which are more related to the internal success of programme design than to outcomes for beneficiaries. This is not to say that good evidence on impact does not exist in the literature, but rather that it is counterbalanced by a far larger body of writing seemingly unconcerned with either impact assessment or methodological clarity.

More fundamentally, many studies and reports fail to include adequate information on their methodologies or data sources, making it difficult to appraise accurately the reliability of their conclusions and recommendations. Given the primary purpose of these studies—to provide practical policy guidance on ways to promote livelihoods—this silence is particularly concerning. Similarly, what is also found with remarkable regularity are literature reviews or syntheses that offer best practice guidance and policy recommendations based on unclear empirical data and shaky foundations. (As a very basic recommendation, those responsible for writing such reports should include a minimum level of information on the methodologies employed and the empirical basis for claims.)

At a time when donors are particularly keen to engage in places affected by conflict and fragility, and to invest in activities that generate transformational change, this dearth of evidence is highly concerning. Indeed, it is arguably because of the absence of high quality impact data that largely unjustified assumptions about the effectiveness of particular interventions often shape policy and programming choices. To take but one example, job creation has been championed by various segments of the donor community as a pathway towards stability and prosperity in conflict-affected countries. However, such enthusiasm appears to be based largely upon a weak understanding of complex and contextual labour market dynamics, a simplistic reading of the relationship between (un)employment and violence, and a misinterpretation of the evidence base. Our review, at least, suggests that the impacts of job creation programmes in fragile and conflict-affected situations are largely unknown. In light of this, as well as the various other examples included in this paper, there is a strong case to be made for a less zealous and more restrained handling of such interventions in contexts of fragility and conflict.

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Endnotes

- ¹ The Secure Livelihoods Research Consortium (SLRC) is a six-year global research programme funded by the UK Department for International Development (DFID). It explores livelihoods, basic services and social protection in fragile and conflict-affected situations.
- ² The selected interventions included school feeding programmes, water committees, seeds-and-tools programmes, social funds and 'markets for the poor' (M4P).
- ³ As de Klerk and Nourse (2004) explain, '[c]apital assets owned by grant clients who did not advance to loans totalled on average \$16. Clients who advanced to basic loans owned \$41, and those who went on to take advanced loans had on average \$84'.
- ⁴ For a full list, see <https://www.microlinks.org/good-practice-center/value-chain-wiki/value-chain-and-conflict-resources>.
- ⁵ See <http://www.m4phub.org/debates/> for more information.
- ⁶ A recent report defines reintegration as 'the process by which ex-combatants and other beneficiaries acquire civilian status and (hopefully) gain sustainable livelihoods and income' (UNDP, 2008, p. 66).
- ⁷ See <http://bigpushforward.net/about>.
- ⁸ The rise of systematic reviews in international development is symptomatic of this. However, systematic reviews are far from objective and apolitical; choices about how to assess the quality of evidence have profound implications for a review's findings (see Hagen-Zanker et al., 2012; Mallett et al., 2012).

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