A reflection on why research is of little use to Nepal's Peace Process and why that might be hard to change

The call for papers for this conference is emblematic of a lot of efforts to improve the impact of research on policy and practice. Let me bore you a bit with a quick and dirty analysis of the argumentative structure of the call.

It basically says:

o Nepal is in a difficult situation and many kinds of organizations try to do something about it;
o However, there is little exchange between researchers on the one hand and policy makers and practitioners on the other about the effect of the interventions that the latter are involved in;
o If only these policy makers and practitioners would know about existing/ongoing research relevant to their work, they would do better.

However, on the basis of what is generally known about the role of research in policy making and political processes one cannot have high hopes that research is going to make a difference in the short term. The interface between research and policy was first mapped in the 70s and 80s. I conducted a study in 2000[1] that summarized what was known by then: nothing much new added during the 90s. Work since then, much of it funded by the UK's New Labour government[2] , added more summaries, and lots of 'how to improve communication' and other types of manuals. However, the core conclusions of the very first studies keep being repeated: very little direct influence of research on policy. And if direct influence can be demonstrated many factors play into it, validity and other quality criteria of research only being minor ones.

For those of you unfamiliar with the scientific exploration of research uptake, may I point out that it originated in (and remains rooted in) the world of evaluation, a very practical kind of policy research, the kind of research the applicability of which seems indisputable. If one is looking for researchers with a well developed sense for the intended research users' perspective, this is the part of the professional spectrum where to look. So the above conclusions are not easily dismissed as a typical 'problem' of academic research.

However that may be, nearly all social science researchers, from the most theoretical to the most applied, tend to assume that what they produce should be taken into account by policy and practice. Somewhere deep down we're all believers in the moral

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[1] Cross, M., Henke, R., Oberknezev, P. & Pouliasi, K. 2000. *Building Bridges. Towards effective means of linking scientific research and public policy: Migrants in European cities.* Utrecht: AWSB research papers 99/07, p.177. Available online: http://www.unesco.org/most/scspbuilding.pdf (accessed 22-03-2010)

[2] Activities relevant to this paper are e.g. ODI's research program 'Research And Policy In Development' (http://www.odi.org.uk/programmes/rapid/) and its 'Evidence-Based Policy in Development Network' (http://www.ebpdn.org/)

superiority of evidence-based policy and practice. So the above conclusions bother us, and we look for ways to change this uncomfortable reality. To make myself clear, I definitely believe that promoting the relevance of research to potential users is a laudable undertaking. We should continue trying to apply the very sensible lessons that four decades of research uptake study has resulted in[3]. We should just be realistic in what outcomes to expect.

So, if I am not here to tell you that discussing potentially relevant research findings with policy makers and practitioners is useless (although I am pessimistic about short term impact on the "ongoing peace process") , what am I here to tell you, which might contribute something? The answer is two things:

o Bring to your attention the kind of research efforts that might have impact in the short term but do not seem very popular with either the research world nor with research funders;
o Bring to your attention the importance of thinking through the longer term impact social science research can have and what that implies.

To start with the first: I want to argue that we, the research world, are not good at identifying and organizing the most obvious kind of research that has direct (short term) relevance to the socio-political reality in a desperately poor country 'in transition'. Again, to make myself clear, I'm not implying that the researchers present here, the organizers in particular, have not asked themselves the question what research topics are relevant in the context of Nepal's current peace process. The call shows that this question has been given explicit thought. It even outlines an exemplary list of topics, but it leaves out what I would consider the most obvious candidate: rigorous, systematic, comprehensive, ongoing, timely and (trend)-analytically reported fact finding.

Why is that the most obvious candidate? Because independently and transparently established relevant facts, and even more so if they show a trend, that are available on a timely and regular basis, are inherently political goods. That doesn't guarantee impact but it makes them not so easy to ignore. However, the many qualifications in the above description are quite crucial to facts being more or less easy to ignore. The qualifications are the abutments of facticity that give a trend a measure of robustness. Robust realities are not necessarily taken into account but they are reference points that need interpretation and negotiation.

I see the political role of a robust fact in very similar terms to Wardell and Lund's description of the role of formal law in Africa:

"… practice often differ[s] significantly from what the law (-makers) could be held to expect. Law is not implemented or enacted unscathed by everyday negotiations or more dramatic circumvention, by manipulation or outright nonobservance. Thus the meaning

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[3] Ensuring relevance for users, involving intended users in the research, the wealth of advice on good communication/presentation, on enlisting research champions, on timing, etc. etc.

and affect of law in a particular place depend on the history, the social setting, the power structure, and the actual configuration of opportunities. This does not mean that laws and regulations do not have an effect. In fact they constitute significant, though not exclusive, reference points for actors and politico-legal institutions in the negotiations of access and rights – even if they are not enforced"[4].

Is there nothing of that rigorous, systematic, comprehensive, ongoing, timely and (trend)-analytically reported fact finding happening? Well, actually there is, tellingly not labeled 'research' but e.g. 'investigative journalism', or 'monitoring'[5]. Let me list five examples of such fact finding in Nepal that I am aware of:

o   The Informal Sector Service Centre (INSEC) monitors the human rights situation in Nepal. It has human rights reporters in all 75 districts of the country. The monitoring that I want to highlight here are the trend analyses of Human rights violations that are being published online since mid 2005[6];
o   The World Food Program (WFP) has created the Nepal Food Security Monitoring System (NeKSAP) that collects, analyzes and presents information on household food security, emerging crop crises, markets and nutrition from across Nepal[7]. Its household food security data are actually research-based, the others are 'monitoring' data;
o   DFID and GTZ have created a field-based monitoring system to feed into an ongoing political economy analysis of the risks which their programs and their staff face in Nepal. The idea is the monitoring of 'development space';
o   The Carter Centre does something comparable for the monitoring of 'democratic space' but their field input is not project/program-based but is gathered by teams of observers[8];
o   United Nations Office for the Coordination of Humanitarian Affairs - UN OCHA maintains the UN information platform[9] which contains a wealth of monitoring information, all collated from a variety of sources. Some is actually research-based and analytic[10].

All five, in different ways get noticed, and play a role in socio-political debate (although most of that is not *public* debate). The INSEC trend data (and other fact finding reports) get attention from the press as do WFP and UN data, and Carter Centre reports. The DFID/GTZ data and analysis is mainly for internal use but some of it is shared with UN and donor peers, and because it's considered credible can be expected to regularly influence their assessments and thus actions. All in all, the "data infrastructure" in Nepal is impressive. This is not to say that much more could be collected – I will address this

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[4] D. Andrew Wardell and Christian Lund (2006), Governing Access to Forests in Northern Ghana: Micro-Politics and the Rents of Non-Enforcement, *World Development* 34 (11), p1887.

[5] This is not particular to Nepal but a very widespread phenomenon.

[6] See: http://www.inseconline.org/

[7] See: http://groups.google.com/group/NeKSAP?hl=en&pli=1

[8] See: http://www.cartercenter.org/countries/nepal.html

[9] See: http://www.un.org.np/index.php

[10] See e.g.: http://www.un.org.np/reports/OCHA/2010/2010-03-22-VDC-Secretary-Notes.pdf

part of the problem later – but, compared to many other 'transition phase' LDCs, Nepal is well resourced.

But the analysis of the data – which includes the way field report information is 'coded' in databases – in most cases this stands for throwing away most of the raw information - leaves much to be improved. If it is coded in databases at all that is. And datasets are only very superficially analyzed. In case you start having suspicions that I am a quant: yes, I do believe that it often makes sense to go beyond the anecdotal and/or the narrative, but I am not trying to promote sophisticated statistical analyses and hypothesis testing.

The core of my argument is that it would be relatively 'easy' to get much more out of what has already and is currently being done by data-basing and analyzing it better. Let me use the example of what Licadho, a Cambodian Human Rights organization with a mission very similar to INSEC, has done over the last two years. Licadho has a monitoring department that does fact finding regarding human rights violations. Monitors write narrative reports that contain a wealth of information. This information was entered into a database at headquarters and allowed them to produce reports very similar to the ones INSEC currently produces. Their new database[11]:

o Preserves all information of the original files. Violations, victims and perpetrators, places or incidents of interest, can be entered with an exceptional level of detail. E.g. for perpetrators all kinds of relationships beyond what the INSEC database (I believe: state/non state, party affiliation, sex) would code. The original case file, including any visual info, is also added;
o Is fully searchable, both the entered data and the case file attachments;
o The info can be analyzed, visualized and cross-linked in near limitless ways. I do not exaggerate when saying that it is difficult to dream up an analytic question that one cannot ask to this database. And the answer can be given, directly from the database in tabulated or graph form if that is what one requires.

What did it take to create this? A techy staff member build the database for them. I know him well and this was one of the many projects he was involved in so you talk one part time professional. Then he worked with monitors and data entry personnel to make it as user friendly and comprehensive as possible. When they switched to this database they temporarily employed a couple of additional data entry staff to back feed their archive into the database which now contains 8000 cases (some of which comprise of many incidents) spanning a decade. Given the above described power of this resource one can now explore and visualize the trend over that decade regarding nearly any aspect regarding the Human Rights violations stored in the database. What does it take to now get the added value? Nothing beyond what it took to use their old database. Or maybe one research minded and database savvy analyst who regularly tracks and explores trends and gives feedback to monitors as well as produces situation reports and trend reports. And I mean reports on real trends, not the very dodgy short term stuff

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[11] A somewhat comprehensive presentation of their database runs over 13 pages so this is just a taste

– a couple of months, maybe a year - that is the usual content of such reports. We all know that the longer the time the more telling the data becomes.

After the above sales pitch you're also entitled to be told about the weaknesses of this very powerful database. The most essential one: the power of the data is only as good as its comprehensiveness. Although Licadho does a lot of fact finding, they are not the only ones doing it and they cover some parts of the country much better than others. Coordination of data gathering and sharing of data gathered between the different organizations and groups gathering info on Human Rights violations is very poor. The two other major Human Rights organizations both have their own monitoring systems and databases[12]. Sound familiar? Besides INSEC the Advocacy Forum and the Office of the United Nations High Commissioner for Human Rights in Nepal (OHCHR)[13] also database their monitoring information[14]. One doesn't have to be a tech wizard to understand that one cannot compare database output without assessing the extent of overlap. One doesn't have to be very bright either to understand that an x number of unconnected databases are resource wise very wasteful and on top of that much less powerful than having all in one. Sounds a bit like the American health system: much more expensive than other options and only having worse results to show for it.

This is a known and predictable problem, occurring everywhere. It is part organizational sociology 101, issues like competition for resources, reputation, control and other survival necessities, understandable but regrettable mistrust; part practical considerations of capacity, logistics, budgetary constraints and difficulty to organize the required coordination infrastructure; part security considerations, especially relevant regarding the politically sensitive nature of some of the most relevant fact finding. So the problem is here to stay. But that is not to say that not a whole lot more could be done.

For a variety of reasons the absence of researchers and their organizations as serious stakeholders in the above described kind of fact finding is regrettable. The ones now doing most of it are driven by short term program and project based objectives. Attention to comprehensiveness, building the base for trend analyses, and deeper probing of available material for patterns, are typical research 'hang ups'. For the organizations that do the monitoring, research is a supporting activity, not their core business.

Researchers, on the other hand do not have the required 'field work machinery' so they can only produce the data if they hook into the already existing policy and practitioners' infrastructure. In theory the way forward would be evident. Natural entry points for such symbiosis would be NGO umbrellas. Anyone with a bit of experience anywhere in the world knows the difficulties of coordinating umbrella organizations so 'natural' is not to say 'easy' or even 'feasible'. Maybe that plays into rigorous, systematic, comprehensive,

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[12] See: http://www.adhoc-chra.org/article.php?language=english&art_id=103&currentpage=1 and http://www.cchrcambodia.org/English/index.php
[13] And maybe others that I am not aware of
[14] See: http://www.advocacyforum.org/departments/human-rights-documentation-and-monitoring-department.php and http://nepal.ohchr.org/en/index.html

ongoing, timely and (trend)-analytically reported fact finding not getting any mention in the call for this conference. The research community here is only being realistic, they opt for the possible. But they also act like the person who lost her car key and searches for it in the light of the street lamp to use that worn out simile.

I would very much hope that at least some in the local research community would take up the difficult challenge of:

o Negotiating your way into the existing policy and practice oriented field based infrastructure with (underutilized) data collection potential;
o Managing the mine field of political and logistic barriers constraining inter-organizational coordination and collaboration;
o Institutionalizing any progress made so that it doesn't remain in the project 'mode' but becomes an ongoing mechanism and resource;
o Contributing specific professional methodological and analysis expertise.

And all of that while being not very visible – and thus lacking the satisfaction of being recognized for one's intellectual work.

Before reflecting on possibilities for changing this obnoxious reality, let's first have a look at the longer term impact social science research can have and what that implies. If social science does tend to have an impact it's an impact on agenda setting, and on general ways of conceptualizing situations and issues, and that takes time to emerge. Often so much time, and the intellectual involvement of so many, that individual attribution is impossible. The uptake of a new idea is normally a very gradual process of 'common sensification' so that by the time it is mainstreamed its scientific origins are all but forgotten. Again no recognition….

Anyway, if research targets impact on how issues of social change are conceptualized, accepting that results are not to be expected soon, what kind of research is then needed? I believe that changing the 'agenda' assumes out-of-the-box thinking, questioning of current assumptions and practices, genuine openness to what one finds rather than looking for the expected. A mindset that is not after simple answers but recognizes that most social issues are complex seems paramount. However, both science and policy love simplicity, straight forward causal relationships, predictability. There have been various approaches that try to do justice to real world complexity and an upcoming umbrella for those is the science of complexity[15]. Box 1 describes some basics of the complexity approach. It is all not rocket science but when we, as either social scientists or as policy makers or practitioners get down to trying to figure out real-world issues, we rarely do justice to their complex, messy nature.

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[15] For a readable introduction: Ben Ramalingam (2008) Exploring the science of complexity: Ideas and implications for development and humanitarian efforts. ODI : WP 285. Online available at (accessed 25-06-2010): http://www.odi.org.uk/resources/details.asp?id=583&title=science-complexity

Box 1: Some basics of complexity science[16]

Complexity science is a science of understanding change, a loosely bound collection of ideas, principles and influences from a number of other bodies of knowledge, including chaos theory, fractal geometry, cybernetics, complex adaptive systems, postmodernism and systems thinking.

It distinguishes between simple, complicated and complex systems and phenomena (and chaos).
It distinguishes between associated theories of change based on linear causality, interdependent systems and relationships, and complex nonlinear dynamics.
It distinguishes between associated kinds of problems: puzzles, problems and messes.

Simple systems and phenomena have tight, centralised connections between their elements, based on simple linear cause and effect relationships. Anyone can see the things the way they are, and can figure out the recipe for solving puzzles posed by the system.
Complicated systems and phenomena have looser relationships between their elements which are still clustered around a central core. Cause and effect is non-linear, but relationships can be modelled and predicted by an expert.
Complex systems and phenomena have centers that are only loosely connected to a network of elements with complex nonlinear dynamics, giving rise to emergent phenomena that are not predictable, and understandable only in retrospect.

The *puzzles* of *simple* systems are well defined and well structured known problems with a specific "best" solution.
The "*problems*" of *complicated* systems are issues that have a known or knowable form or structure but there is no single clear cut way of doing things.
The *messes* of *complex* systems are issues that do not have a well defined form or structure, and solutions are never more than 'maybe's'.

The result?

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[16] I borrow from two presentations held at the Norad Evaluating Complexity Conference(29th -30th May 2008) :
Ben Ramalingam's Evaluation and the Science of Complexity and Michael Quinn Patton's Evaluating the Complex:
Getting to Maybe. Available online (Accessed 24-06-2010):
http://www.outcomemapping.ca/download.php?file=/resource/files/admin_en_Evaluation%20and%20the%20Science%20of%20Complexity.ppt
http://go2.wordpress.com/?id=725X1342&site=aidontheedge.wordpress.com&url=http%3A%2F%2Faidontheedge.files.wordpress.com%2F2009%2F09%2Fpatton_oslo.ppt&sref=http%3A%2F%2Faidontheedge.info%2F2009%2F09%2F28%2Finternational-conference-on-evaluating-the-complex-2nd-in-emergent-series-on-complexity-and-aid%2F

"Some of the greatest mistakes have been made when dealing with a mess, by not seeing its dimensions in their entirety, carving off a part, and dealing with this part as if it were a problem, and then solving it as if it were a puzzle, all the while ignoring the linkages and connections to other dimensions of the mess"[17].

Such a mindset is not conducive to furthering our understanding and making a positive difference to future policy and practice. One needs to take the emergence of the unplanned seriously, welcome surprises and take context really into account. But that is not easy. For no one, researchers included. Psychological biases hamper us as much as anyone else. The following Malcolm Gladwell quote sums it up nicely[18]:

"We hate surprises. We try to erase them from our memory. This is part of what keeps us sane. If, after all, we were always fully aware of the possibility of completely unpredictable events, would we be able to walk out the front door in the morning? Would we ever invest in the stock market? Would we have children? Generally speaking, people who have an accurate mental picture of why and how things happen tend to occupy mental hospitals—or, at the very least, a psychiatrist's office…."

So, is the required bias-resistant mindset very prevalent? Given we're hardwired against it I don't think we can complain, and I certainly hope and expect to hear something here that illustrates it. Will they influence anything? I doubt it but check again in a decade, who knows….Is more complexity aware and appropriate research possible? Sure. Likely? Less so.

Let me conclude with what I consider to be a major reason for that pessimism. My descriptions of what kind of research would be most relevant from the perspective of impact potential in the short term and in the long term already mentioned constraints that work against the emergence of more of either kinds of research. A constraint, a very major one, that I have so far not discussed, but that works against both more rigorous, systematic, comprehensive, ongoing, timely and (trend)-analytically reported fact finding and against more out-of-the-box complexity aware and appropriate research is the way (research) funding 'works'.

I obviously bite myself in the tail here. What kind of example setting is this focus on just one factor? Yes, indeed, 'money rules' is very much the answer to a puzzle. I do not claim, however, that changing the way funding works is sure, or even likely, to result in more of the mentioned kinds of research. On the other hand, I am sure that funding is such a fundamental material reality of the research sector that I cannot imagine a situation with more of those kinds of research taking place without a change in current funding modalities and conditions. Money as your entry point for change is a 'maybe', most probably a 'maybe, if also…..', but I bet my hat it's part of any solution package.

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[17] Unattributed quote, slide 45 Ben Ramalingam (2008), see footnote 15
[18] Taken from Patton (2008), slide 128

To borrow a term from a different discipline, I see the way funding works as a 'most binding constraint'[19].

Let's start with the rigorous, systematic, comprehensive, ongoing, timely and (trend)-analytically reported fact finding. The example used above – monitoring of human rights violations - to illustrate that a lot could be done without requiring (much) additional resources, shows that not only research funding but also development funding more general is the issue. Funding actively discriminates against collaboration. Funders want to see a direct link between their money and outcomes. Anything that makes that even more 'difficult' (I am being generous here…) than it already is, is not popular with those who use the money. This reality is replicated within all levels of the value chain of aid. Implementing organizations receive money from various donors. Most of that is project/program-tied and leaves the recipient with only partial control over the translation of their overall mission into activities[20]. If monitoring is funded, the funder will want to see results from his program. The various human rights violations monitoring programs in Nepal are all funded by different donors. Donor harmonization is not happening despite all their declarations of good intentions.

And the example I used is about as 'easy' as it get because in principle there is money for the rigorous, systematic, comprehensive, ongoing, timely and (trend)-analytically reported fact finding on human rights violations. For many other relevant facts there isn't any money for comprehensive and ongoing monitoring. In the development world, projects and programs are 'monitored' and evaluated, but that may not cost too much, and the results are expected to say something about the effectiveness, and efficiency of the project or program. The monitoring only covers directly intervention related facts, and near always ignores anything contextually relevant. The evaluation parts of the cycle may take some of that into account but only in the limited geographic area of intervention. There are hardly any organizations covering enough ground to even come close to comprehensive coverage. The result: no ongoing monitoring of potentially relevant ground realities and a plethora of one-off[21] surveys and other studies[22] that do not add up to anything. My guesstimate would be that 10% of all M&E budgets used in a coordinated manner would be sufficient to make the ongoing and comprehensive monitoring of quite a lot of relevant facts possible. So it is the way money 'works' not the availability of funds that is the issue.

Complexity aware and appropriate research that might have conceptual agenda setting influence in the longer term also suffers from the way funding 'works'. The development funding world does not have much interest in research that isn't immediately program or project related. So the bulk of research money is not available for studies with broader scopes.

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[19] Haussman, R. c.s. (2005) Growth diagnostics. Available online (accessed 25-06-2010):
[20] INSEC is an exception having a basket fund arrangement with a set of core donors
[21] The most elaborate cycle would be a baseline study, a mid-term review, and an end-of-project evaluation.
[22] The UN Nepal Information Platform maps that nicely:
http://www.un.org.np/maps/mapsublist.php?type=2 (accessed 26-06-2010)

The exception is the money that goes into (development) research think tanks like the Overseas Development Institute (ODI) and the Institute of development Studies (IDS) in the UK or the World Bank research programs. Occasionally these will produce something on Nepal. However, these studies tend to have two kinds of limitations. When they address Nepali ground realities they will have to pass pretty elaborate vetting procedures to ensure the messages conveyed do not upset particular political stakeholders too much. Be they Nepali or donors. If the risks of embarrassment are considered too great, the best case scenario is a report that is not officially released but informally circulated. The circle of those in the know is then limited, and because the report cannot be referenced officially its potential for what a nestor of the study of the research-policy interface has called 'knowledge creep'[23] is seriously diminished. The second limitation is that these think tanks are obliged – and this is obviously a consequence of the accountability expectations tied to their funding – to follow policy research conventions, i.c. simple implementation recommendations, not 'try out' suggestions, the uptake of which would have to include ongoing monitoring and responsiveness to changes[24]. My personal experience of reading the often fascinating in depth reports coming out of such institutes is that their concluding chapter turns a complexity aware and appropriate analysis into a set of recommendations that either suggest that despite what the preceding chapters said a simple cause-effect theory of change is actually sufficient or requires you to believe in miracles. In my view, those recommendations have the effect of 'neutralizing' the implicit 'knowledge creep' potential of the analyses.

That leaves pure academia. The PhD kind of work is then the most interesting as it allows the researcher to spend considerable time to gather data and familiarize herself with the context. Disadvantages are that in social science PhD work is nearly by definition an individualistic enterprise. And, unless that one individual can hook into a large existing network that gives access to relevant, often otherwise 'hidden' information – e.g. by being a staff member of a stakeholder organization rather than an 'outside' researcher - she is limited in what she can do. The occasional mid-career development professional with a decade or more of policy and practice field experience in a particular context who manages to graft a PhD onto that experience and network is another exception. Such exceptions have resulted in extremely interesting descriptions of development[25]. And some have indeed contributed to conceptual change in our way of thinking about social change. But they are rare (I am not knowledgeable enough to give you a Nepali example) and I do not expect that to change any time soon.

I don't think it is realistic to expect either an increase of such academic research, nor to expect (development) research funding to start working differently any time soon. I hope reality proves me wrong.

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[23] Weiss, C. (1980). Knowledge Creep and Decision Accretion. Science Communication March (1):381-404.
[24] Yes, I do simplify here. The World Bank also publishes a lot of theory-testing social science.
[25] E.g.

Attachment: Conference Call for Papers

Conflict, Transition and Possibilities for Peace in Nepal:
challenges to engagement, practice and scholarship

A conference to generate critical dialogue between researchers and policy makers
Kathmandu, 3-4 July 2010

Nepal entered a peace process in 2006 after nearly eleven years of insurgency. National forces, including civil society organisations and networks, as well as the international community are now heavily invested in steering the outcome of this 'transitional phase' and many experts in conflict and conflict transformation from different countries have been called upon to support the various peace-building, conflict transformation and constitution development programmes with both the government, and civil society. However, to date, there has been little systematic public or academic debate on the role, relevance and effect of the policies and approaches that have been mobilised for this 'transition' process in relation to Nepal's peace process. Neither has there been critical dialogue and evaluation of their resulting outcomes in terms of forwarding possibilities for peace and longer-term accountability. It is imperative to explore how different actors operating in the fields of transitional justice, conflict mitigation, reconciliation and reconstruction, including the donors who are funding these interventions, understand and contribute to 'transition processes'. Of equal importance is to understand the experience of those at the centre of these interventions and process, namely, former combatants and victims of the conflict on both sides.

This forthcoming international conference aims to initiate such a debate. We invite researchers working in these areas to present their work and discuss its significance with practitioners and policy makers for the ongoing peace process.

The local organisers are the two leading research institutions Martin Chautari (www.martinchautari.org.np) and the Social Science Baha (www.soscbaha.org).

The conference will focus on the mechanisms, practices and discourses of reconciliation, post-conflict social reconstruction and transition in Nepal. Papers will be research-based and bring out the socio-cultural dimension of these processes, highlight the perspective of actors involved and endeavour to adopt a comparative perspective. They may deal with issues related to the following:

- Transformations in the Maoist movement in the wake of the CPA

- Integration and rehabilitation of former combatants

- Victims of the conflict: managing the post-conflict period

- International aid, its influence and impacts on conflict transformation and peace building

- Civil society, its role and relations with the state in reconciliation processes

Interested researchers are kindly requested to indicate their interest by submitting an abstract of no more than 250 words, along with a short biography by 1st May 2010 to jrrconf@gmail.com Applicants will be informed soon afterwards about whether their abstract has been accepted.