Crossing the river by feeling each stone: How big bureaucracies can find their path in fragile states*

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June 18, 2014

Fragile states are tough places to plan and program. We have little data, and arguably each fragile situation is unique. The drivers of conflict, the constraints to prosperity, and what states and aid can do about it—these are largely unknown.

So, the big question I want to pose is how one plans and programs in this environment. How can a big bureaucracy—be it a government or the World Bank or the UNDP—develop systems for learning and scaling what works in fragile, uncertain environments, and changing course as new information comes in? To me the question, "what process?" comes before "what program?". Or at least it should.

The answer, I think, is to be a little of what Karl Popper called the piecemeal social engineer. Tinkering at small scale with many things. Crossing a river by feeling each stone.

*Text of a presentation to a World Bank Technical Workshop on *Sustainable Jobs and Livelihoods in Fragile and Conflict Countries: What Will It Take? Defining Questions, Analytical Tools and Way Forward*, June 18-20, 2014, The World Bank, Washington, DC.

Here's an example. Suppose we want to know what incentives—economic and not—drive young men to fight, riot, steal or rebel. What's a process for answering this question?

I'm going to argue it's not a market study or country diagnostic. It's through experimentation. And by that I do not mean randomized trials.

I want to use an accidental example from Liberia in 2008. It began when the UN Peacebuilding Fund (PBF) gave Liberia $15 million in order to, well, build peace.

A group of ministers, and donor/UN office heads formed a committee to decide on the money's best use. They did a cursory diagnostic—a "conflict analysis"—just to set some priorities and motivate calls for proposals. Then they invited pretty much anyone to suggest projects.

Dozens of organizations applied, and the committee selected winners on merit and fit with aims. The call was a little unfocused, and so were the projects, but in the end they funded more than a dozen fairly innovative pilot interventions.

In essence, they funded trial and error.

What's amazing is also how many of these projects were evaluated in one way or another. Colleagues and I ran randomized evaluations of two. Another pair of scholars had a randomized trial of a third project, and careful qualitative evaluations were done on a couple of others. This happened mostly because the calls for proposals simply said they'd give more weight to projects with some kind of evaluation.

What resulted was an immense amount of learning, and not just because of the large evaluations.

For instance, one funded program targeted high-risk ex-combatants in hotspots. Thousands of men were engaged in illicit resource extraction (such as illegal gold mining or rubber tapping), and who considered risks of mercenary recruitment. The program offered them agricultural training and in-kind capital.

Just testing and trying the program generated a number of insights:

- Unexpectedly (to me at least) the men were almost all interested in agriculture, even relatively senior commanders.
- A curriculum and program was worked out that could be replicated.
- The organization learned, often the hard way, how to manage a class of hundreds of ex-combatants peacefully.
- The organization learned a lot about how hotspots operated, what the highest risk men wanted, and how to engage them non-violently.
- By observing the program in action, we noticed that re-socialization of the men seemed as important as skills training.

On top of this there was the formal evaluation.[1] In the space of 18 months, what we learned was interesting:

- A combination of skills and capital increased agricultural earnings and activity by about 20%.
- Accidentally, some men didn't get the capital, and didn't increase their farming. This implies lack of access to capital was a binding constraint.
- People shifted out of illicit activities, but did not exit them. This makes sense: When incomes are risky, people diversify. Even with higher wages from legal work people will be hard to extract from illicit industries.
- When war broke out next door in Cote d'Ivoire, the treated men were less likely to partake in mercenary recruitment. So economic incentives matter.

[1] See the working paper and policy note.

- While you'd think the ones who didn't attend mercenary recruitment meetings were the ones who got capital and were the most successful at farming, this was only partly true. The men who were least likely to express mercenary interest (by attending meetings, agreeing to a contract to go, or moving towards the border) were the men who didn't get the capital. Why? They'd been told to expect it soon, in cash if not in kind. So they hung around because they had a hard, cash incentive to do so.

So we confirmed some expected things—that skills and capital stimulate self-employment, and that illicit labor supply responds to peaceful earnings opportunities. We also learned some unexpected things—that expectations of future transfers were potentially better at deterring mercenary recruitment than past completed programs.

It didn't stop there. Our early observations about the importance of socialization led to pilot programs where we experimented with methods for behavior change. So several colleagues and I responded to field findings in real time with new programs and a study.[2]

What we found was amazing: that a cheap, intensive, 2-month behavior therapy program reduced drugs, crime, and violence among urban street youth, and that the changes persisted at least a year.

If the PBF call had been more employment focused, we would have had a bevy of other evidence. But it was spread out. But another cluster of programs and studies centered around dispute resolution and local justice, with similarly important lessons.[3]

What's more, we were able to leverage all the data collected to do more novel things, such as start to use machine-learning tools to predict local-level con-

[2] A working paper will be available in Fall 2014 here. In the meantime, see the project page, this Foreign Affairs article, or this NPR Planet Money podcast for project highlights and early findings.

[3] See the published paper.

flict—essentially piloting early warning systems for local violence. This was highly successful, with results to be released soon.[4]

The PBF approach was, in my mind, wildly successful. Altogether, a cluster of promising pilots with some kind of evaluation, rigorous or not, not only offered answers but also new questions and new pilots, and eventually programs that could be scaled.

What they did I call "experimentation". Not experimentation in the sense of randomized evaluation (though there was that too), but in the more traditional sense of the term: small-scale trial and error with attention to what is working and why. The act of a piecemeal social engineer.

This is a very different process than the usual program. Here, for instance, is a cynical take on the average World Bank project:

1. Decide on an intervention, preferably something off the shelf like vocational training
2. Write the program manual without piloting the intervention to see if it minimally works
3. Immediately go to scale
4. Implementation problems make it hard to tell whether the program actually works well, even when done right
5. Nine months before the money dries up, suddenly become open to different ways to accomplish the goals and fund a bunch of random stuff

That is, project managers end up being piecemeal social engineers by accident.

[4] An early policy report from the first phase is here. We made predictions based on this model and tested them on new data in 2012. A working paper will be available in Fall 2014 here.

To me, process is key. I'd like the UN, World Bank, governments, and no-profits to think less about diagnostics and large programs and more about subsidizing innovation and trial and error.

I often hear that this is hard to do institutionally. I'm sure that's right. But I'm hoping if you can be do it by accident you can also do it on purpose.