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Commentary on: Laura Pinto's "When politics trump argumentation: Financial literacy education policy"

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1. INTRODUCTION

Laura Pinto's paper is about the rationale for a specific educational policy decision that came to be made in Ontario. It is a rich text that demonstrates well the complexities of educational policy making; how it is shaped and justified by "values, self-interest, mobilization efforts and lobbying power" and, we might add, wishful thinking. This is Laura's conclusion: "These arguments rested on little more than 'truthiness' (that is, claims of knowing something intuitively without regard for evidence, logic, or facts), rather than on the application of serious reasoning and the consideration of empirical evidence". To a large extent empirical evidence was neglected in this process, Laura argues, and this is her main concern.

As all commentary on rich texts, this one can take many different directions. For example, rhetorical analyses; how social problems are made into educational problems and thus left to schools to solve; the causal presuppositions underlying views of the possible effects of policy decisions, etc. After debating the possibilities with myself I have decided to concentrate mainly on one issue; namely question of what evidence can do for us.

2. EVIDENCE

The educational policy in question in financial literacy education (FLE) in 4-12 education, instituted worldwide in the wake of the 2008 global financial crisis. Sections 2 and 3 of the paper provide a thorough description of the background for FLE and of the political rhetoric that surrounded FLE in Ontario. This is no small task. A corpus of political rhetoric has been carefully collected, systematized, analyzed and interpreted. Laura describes her approach as *grounded theory*. GT has been criticized for being empiricist and (naively) inductive, so how comfortable should we be in using it?

Providing structure to this messy and rather bewildering field of political rhetoric around FLE is a daunting task, and can evidently be done in several ways. Laura has settled on a structure which foregrounds attitudes of various actors (different arguments for) toward FLE: 1a, 1b, 2a and 2b. I admit I found it hard to

keep them apart and remember which was which as the analysis moved on through its various layers.

1a: FLE is urgently needed

1b: FLE should be developed and enacted through a financial industry and government partnership

2a: While FLE might be valuable, it should be developed without industry involvement

2b: Principled support of FLE, but critical of the financial sector (that created the problems in the first place)

To make a long story short, they arrive at the basically same conclusion (in favor of FLE) but the routes by which they do so differ, as the paper makes clear. The problem with the arguments is that they over-simplify and rely on truthiness instead of evidence-based policy. They fail to use evidence effectively and accurately, and the nuances and complexities of the problems got lost in a truthy, rhetorical mist.

The truthy rhetorical mist is well-documented in the paper, it seems to me. But it may be that Laura is a bit too optimistic about what evidence can actually do for us, and I shall outline some questions that I hope can be food for further thought. I shall focus on FLE as a case of evidence-based policy, although many other types of facts and data have a role to play, as Laura points out.

Let us begin by looking at the desired effect, since this generally forms the basis for our judgments of how well a policy works. The wishful thinkers, those who think that FLE can solve the financial crisis (the 1a group), clearly had a long-term goal in mind for FLE. And complicated, in that FLE targets individuals whereas “financial crisis” is an aggregate entity – the idea must be that people learn to think differently, thereby they act differently, and in the long run their different actions aggregate in such a way as to alleviate the financial target. If this is what we expect of FLE, it is no wonder that no evidence exists to support its efficacy – it has still to show itself.

The same problem exists if you test people at the end of a FLE course and find that by and large their understanding of financial matters hasn’t changed much. That is the short-term effect, but we do not in principle know what the long-term effects might be. In many cases of evidence-based practice, the short-term effects are preferred because the results can be so easily traced to the intervention and we (think we) immediately can pass judgment on whether it works or not.

But let us assume that wishful thinking is okay. How confident can we be that our policy will achieve our desired outcome should we implement it? What kind of considerations should we make, and what can the evidence tell us? Laura suggests that data about FLE was largely ignored, but what could it have told us? Suppose data from several FLE experiments suggested that FLE worked well, that the assumed causal connection between policy and effect is established. That, Nancy Cartwright argues (e.g. Cartwright, 2012; Cartwright & Hardie, 2012), tells us only that it works *somewhere*, not that it works *generally*. But can we use that evidence to predict that the policy will work for us? No, she says. The evidence may be highly trustworthy (especially if it is an RCT design), but trustworthiness and relevance are two different things. The fact that FLE worked “there” does not imply that it will also

work “here”. Or vice versa, we might add: the fact that FLE is ineffective “there” does not imply that it will not work “here”.

Evidence in itself is thus not enough, on Cartwright’s view (and she does argue well for her case). A policy, no matter how well evidenced it is, is only one part of the large constellation of factors, and the overall effect (e.g. solving the financial crisis) depends on how all these factors add up. If we want to predict whether it will work for us, there is a whole host of other facts we also need to ascertain.

Policies may be highly complex, but basically they encompass what Cartwright calls causal principles: a causal connection between a cause and an effect, a policy and its result, input and output (both may be large entities). I think there is a tendency to think that these are general, they hold in and of themselves so that once we have established such a principle it can be safely generalized across many contexts (this seems to be a basic presupposition of most EBP approaches). But this is a mistake, argues Cartwright. Causal principles typically vary, they are in fact local and they hold *ceteris paribus*. They arise out of and are supported by the local causal/social structure, and since such local structures cannot be supposed to be homogeneous, we cannot simply transfer a principle that holds “there” to “here”. “Here” may have a causal structure in which the policy does not have the requisite “bridges” that allows it to travel to the effect. The world is too diverse, she says. Evidence of how a policy works “there” is thus useful, but of very limited relevance.

So we need facts about “there” and “here”. Are the local causal/social structures sufficiently similar? More importantly, are the factors necessary for the policy to work in place? If not, your policy will not work –it does not have the structure it needs to work through. This set of necessary factors Cartwright calls the “support team”.

3. CONCLUSION

I keep badgering my students with the importance of evidence; to provide it themselves and to demand it from others. It thus feels a bit odd to put forward a viewpoint that reduces the role of evidence. The crux is what we want evidence *for*. In research we want evidence to judge the trustworthiness of our claims about the world. But in the policy context things are somewhat different. Evidence for trustworthiness of, say, a causal claim is *not* thereby also evidence for what will happen when we implement that causal claim. In the policy context the role of evidence is changed and reduced, and other considerations are foregrounded: what we need to know to make a reliable prediction about whether FLE will work “here”; about what might happen when we implement it. True, some of these may also need evidence, e.g. in the form of statistics about people’s financial behavior, but some of the facts about local conditions we can ascertain by experience, by seeing for ourselves.

It would be fun, perhaps, to investigate the rhetorical political mist with an eye to the following:

- In the midst of all the factors featured in Laura's paper, what causal role can FLE possibly play?
- What might a good support team consist of if FLE is going to have good effect? I have no suggestion, really, except to say that it of course depends on what sort of effect you want, and that there might be many such factors and that they might be at different levels of aggregation (parental involvement, market regulation, perhaps?)
- How should we understand "will work"? That we can observe specific results? Some form of average value? Or simply that it contributes positively?

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