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Pragmatism, Praxis, and Predictive Theory

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Pragmatism, Praxis, and Predictive Theory

Jason, Stevens, Ram, Miller, Beasley, and Gleason (2016) invoke particular views of "true science" (p. 4) and "hard science" (p. 7) in their call for "more rigorous and predictive theory" (p. 21) in community psychology. They explain that a theory (as opposed to a framework or a model) makes predictions about causal relationships that are specific enough that they can be empirically tested and either verified or falsified under various conditions. They claim that by moving from frameworks guiding inquiry and action toward unambiguous predictive theories, the field will be "of greater value to the larger scientific community" (p. 3) and could "go a long way toward making significant progress in understanding how complex systems and the contexts in which people live can influence their lives" (p. 7). Their article is valuable for the debate it will produce by staking out such a clear position on a topic on which, as the authors note, viewpoints vary. In my view, the central thrust of their article – away from a pragmatic action orientation to inquiry and toward testing of predictive theories – could actually hamper the field's progress toward its goals, if adopted more broadly in the field of community psychology.

As Jason and colleagues (2016) note, a pragmatic approach to social inquiry represents an alternative to the type of predictive theory testing that they are encouraging. The pragmatic view can be understood through a statement like this: we can learn a lot that is useful through applied research that examines strategies for action in communities and settings, but we may, in the end, be unable to pin down many of the specific causal mechanisms at play in these complex systems. Why on earth, you may well be wondering, would we prefer this pragmatic approach that seems to promise *less* clarity and precision? Isn't that just resigning ourselves to so-called *soft* science? To explore further, let's look at an example.

Shape Up Somerville began in 2002 as a community-based environmental change strategy to prevent childhood obesity. The initiative galvanized local leadership to create changes in policies, systems, and environments throughout Somerville, MA. These included changing the physical environment and transportation infrastructure to promote walking and biking, changing school food services, passing school wellness policies focused on physical activity and nutrition, creating a healthy menu certification program for local restaurants, and starting an annual 5K run and family fitness fair. Parents and community members were directly involved in making many of these changes, and they were regularly engaged through local media and newsletters containing project updates, health tips, and coupons for healthy food. There were also individual-level behavioral interventions including new health curricula and regular clinical screenings of children, and targeted provision of services for those found to be at greater risk for overweight and obesity. A result of this multi-level community intervention was something incredibly rare in the contemporary U.S.: a reduction in childhood overweight and obesity at the population level in the city (Economos, Hyatt, Must, et al., 2013). The initiative was adopted and sustained by the city after the initial funding period, and recent research indicates that not only has this initiative reduced childhood obesity, but it also had a similar effect on parents, even though adults were not the primary targets of the interventions (Coffield, Nihiser, Sherry, & Economos, 2015).

From some researchers' vantage points, initiatives like Shape Up Somerville are a bit of nightmare. It is nearly impossible to disentangle the effects of particular interventions. There is so much noise and contamination. How can we know exactly which of the many interventions were causally related to the outcome? An intervention science approach that prioritizes predictive theory would insist that we disentangle the initiative through testing of specific interventions on controlled populations and assessment of the effects on specific outcomes such as physical activity and nutrition. Such an approach, proponents would claim, would be more rigorous, scientific, and likely to produce generalizable knowledge.

Instead, the key finding is that *multiple* changes, when accomplished through inclusive community action, can have the effect of increasing wellness at a population level. We can surmise that each of the interventions may have had some small independent effects, but the combination of interventions and the processes by which they were implemented was likely producing some sort of multiplicative and contagious effect in the complex set of systems that comprise the community. Every one of the individual intervention strategies would be unlikely to have similar effects alone. Counter to the intervention science view, it could be argued that this approach of combining research and action on multiple interventions and assessing effects at a population level is more "rigorous" and "hard" than randomizing controlled sub-populations. That point aside, I would argue that despite the lack of clarity on the specific mechanisms of causality, the results are more *useful* than controlled tests of discrete interventions for addressing the persistent social problem of obesity in other communities.

An example with similar contours is provided in a position paper adopted by the Society for Public Health Education (SOPHE) (Livingood et al., 2011). Livingood and colleagues use the example of smoking cessation programs, which have been a topic of great interest in public health for decades. Reviews have been published on evidence-based approaches to prevention and intervention. These have emphasized individual clinical and behavioral approaches. In contrast, they have largely neglected community and environmental change strategies. This is because the standards for evidence have been based on the reductionist model of testing hypotheses linked to predictive theory. This model lends itself well to testing for effects of individually oriented interventions in defined sub-populations[2]. Policy, systems, and environment changes are meanwhile neglected because they range from extremely difficult to impossible to study in the ways that a predictive theory approach to science demands.[3] Yet, the widespread reductions in tobacco use are primarily attributable to such changes.

Livingood and colleagues point out that the prevailing paradigm in public health (which is similar to the predictive theory model advocated by Jason and colleagues, 2016) "does not identify accurately what is most likely to have produced reduction in tobacco use—the major cultural change of de-normalization and social intolerance of tobacco use. The profound impact of media-based health education and promotion programs, and local community mobilization and advocacy for smoke-free environments, scarcely are represented in most reviews of the published scientific evidence" (p. 527). In fact, the authors argue that although the research-to-practice predictive approach will continue to serve researchers well in natural science disciplines, including some forms of medicine, "the application of this model to community problems reflecting complex social determinants and health disparities is likely to be far less effective. There is too little similarity in structure, culture, politics, economics, and function of communities and

their populations to suggest that an RCT in one or more of them could produce highly generalizable results" (p. 528). Interestingly, these authors conclude that what is needed is applied social and behavioral science that is conducted in ways more akin to engineering than to physics. Jason and colleagues (2016) are clearly pushing in the opposite direction when they suggest that if researchers are not "engaging theory" in the linear science model that they describe, "their contributions are best categorized as an engineering endeavor rather than true science" (p. 4). Is an applied approach akin to engineering really less "true", "rigorous", or "hard"?

Along with these SOPHE leaders and others (e.g., Lewin, 1946), I often make the case that it is not. My sense is that a pragmatic approach is actually a more promising avenue for community research, both from a practical and an epistemological standpoint. Social and psychological science operating in a reductionist approach has had scant success at predicting important social phenomena. This is in large part due to the fact that the objects of study are themselves influenced not only by their characteristics and experiences, but also by the multiple contexts in which they are interacting with others. And, are influenced by theories – their own and others. There is therefore at least one thing that we can predict with relative confidence: human communities will consistently evade precise and accurate predictions.

In contrast, social science that has been conducted from a more holistic praxis orientation has often produced results that are both fascinating and influential, as in the *Shape Up Somerville* example described above. Forgive the lengthy quote, but urban planner Bent Flyvbjerg (2001) makes these points forcefully in the conclusion to his book *Making Social Science Matter*:

> First, we must drop the fruitless effort to emulate natural science's success in producing cumulative and predictive theory; this approach simply does not work in social science. Second, we must take up problems that matter to the local, national, and global communities in which we live… we must focus on issues of values and power like great social scientists have advocated from Aristotle and Machiavelli to Max Weber and Pierre Bourdieu. Finally, we must effectively communicate the results of our research to fellow citizens. If we do this, we may successfully transform social science from what is fast becoming a sterile academic activity, which is undertaken mostly for its own sake and in increasing isolation from a society on which it has little effect and from which it gets little appreciation. We may transform social science to an activity done in public for the public, sometimes to clarify, sometimes to intervene, sometimes to generate new perspectives, and always to serve as eyes and ears in our ongoing efforts at understanding the present and deliberating about the future. (p. 166)

Flyvbjerg locates foundations for this view of social science as practical wisdom (which he calls *phronetic* social science) in ancient and contemporary European intellectual traditions. As with pragmatism, it is concerned with clarifying and adjudicating differences between competing guiding values for praxis. Praxis, according to Prilleltensky (2001), represents "the unity of theory and action" (p. 748) with the goal of "translation of ethical reflection and social research into social action" (p. 749). In the pragmatist tradition, it is through this reflective theory-in-action that we can learn the most about the human systems that we are trying to improve, and therefore do the most to improve them. Although this is different from a vision of social and psychological theory that can reliably and accurately predict outcomes from specific interventions, it is not atheoretical, nor is it a rejection of the scientific method. To the

contrary, the most prominent pragmatists (James, Peirce, Dewey, Mead, etc.) were self-described radical empiricists. Peirce advocated for the cultivation of an experimental habit of mind (Bernstein, 1999). For a pragmatist, even though truth is always contextual and conditional, human intelligence can improve human conditions, and human intelligence is inherently scientific.[4]

I agree with some of the major points made by Jason and colleagues (2016). We do need measures that perform as consistently and accurately as possible. We also need to build on the foundational concepts of our field like the ecological model, which I agree is useful but insufficiently specific for targeting research and action. I disagree, however, with Jason and colleagues' (2016) conclusions regarding theory and research on empowerment and sense of community. The disagreement has less to do with their specific points on those bodies of work (although I could quibble) and more to do with the claim that a theory should be evaluated solely or even primarily on the basis of predictions and testability. Again from Livingood and colleagues (2011): "This approach reinforces and reifies prevailing linear approaches that make use of generalization from specific parameters but misses the mark of producing feasible, relevant, and politically acceptable solutions to real-world health problems intrinsically embedded in the widely varying complexities of behavioral, social, and cultural settings" (p. 526).

The pragmatic and praxis-oriented approaches that community psychology has pioneered in previous decades have had an influence on fields like public health and education, which are increasingly embracing research designed to complement, illuminate, and improve community-driven systems change efforts. There is great opportunity for the field of community psychology to bring the tools, frameworks, and, yes, theories that it has developed into important conversations on policy and practice. It would be a sad irony from my perspective if the field now turned wholesale toward a reductionist social science prioritizing predictive theory. This has been a relatively fruitless and frustrating venture for other applied social sciences. Yet, it also seems that this divergence between pragmatist approaches favoring a praxis orientation and more positivist approaches favoring a predictive vision of theory is a tension that has consistently animated the field. It is a potentially productive tension as we mix and match tools, research designs, theories, and frameworks to illuminate complex system dynamics and tailor actions to improve community life.

Notes

[1]Several University of Wisconsin–Madison colleagues provided insightful feedback on a draft of this commentary: Alex Adams, Amy Hilgendorf, Javier Nieto, and Randy Stoecker.

[2]Despite the feasibility of applying linear experimental methods to the study of individual-level human behavior, behavioral intervention effect sizes – even of "evidence-based" programs and interventions – tend to be quite small and contortions are often performed by evaluators to meet even generous thresholds for statistical significance (see Gorman & Huber, 2009).

[3]Researchers lack the power to, for instance, randomly assign otherwise comparable communities to different policy or environmental conditions to test effects, as is possible with, for instance, students in different classrooms or patients in clinical settings. Research on policy and environmental changes therefore tend to be less amenable to the type of *a priori* predictive

hypothesis testing endorsed by Jason and colleagues. Natural experiments, quasi-experiments, and fortuitous combinations of luck and clever research designs do nevertheless occasionally manage to produce compelling evidence for causality in systemic and population-level changes.

[4]I am not arguing that pragmatism, praxis, and phronetic social science are equivalents, but rather that they can be complementary perspectives for undertaking action-oriented research on community issues

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