

GLOBAL JOURNAL OF

Community Psychology Practice

PROMOTING COMMUNITY PRACTICE FOR SOCIAL BENEFIT



Theories, Models, and Science in Community Psychology

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Keywords: Theory, Science, Community Psychology, Framework

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Recommended Citation: Tebes, J.K. (2016). Theories, Models, and Science in Community Psychology. *Global Journal of Community Psychology Practice*, 7(2), pages 1-11. Retrieved Day/Month/Year, from (<http://www.gjcpp.org/>).

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Theories, Models, and Science in Community Psychology

I appreciate the opportunity to comment on the paper by Jason, Stevens, Ram, Miller, Beasley, and Gleason (2016) entitled, "Theories in the Field of Community Psychology." The authors tackle an important, but often neglected issue – the use of theories in science – and consider how well theories in community psychology advance scientific inquiry. Jason et al. (2016) provide a valuable service to our field in their examination of theories in community psychology, and specifically, theories in three major areas: ecological theory, sense of community, and empowerment. Their brief review of theory in each area provides a useful summary and a call to action for further specification of theory. They have started a conversation vital to our field's future.

Despite my enthusiasm for several parts of their paper, I have a number of concerns. These include the authors': 1) assumptions about contemporary science, 2) assessment of theories in community psychology relative to those in other fields in psychology, 3) overemphasis on theory as opposed to models in science, and 4) lack of attention to other epistemologies in community psychology that hold scientific promise for our field. I discuss each of these briefly below.

Assumptions about contemporary science

The authors' implicit view of science is based on a philosophy of science known as logical empiricism, which is a twentieth century adaptation and extension of nineteenth century logical positivism (Tebes, 2012). Logical empiricism replaced positivism because it offered a practical way – the (hypothetico-deductive) scientific method – to conduct science; in which a researcher begins with an *a priori* hypothesis derived from theory that is then tested inductively by observations made under specific conditions (McGuire, 1986). Until about the 1960s, this was the standard way to do science, and was defensible in the philosophy of science. However, as philosophers noted at the time, this view

assumed that observations were independent of theory, when in fact, they were constrained by the very theories they purported to test (Popper, 1935/1959). Kuhn (1962/1970) and other philosophers of science showed that theories, and the paradigms in which they were embedded, were shaped by extra-scientific factors – social, historical, cultural, and psychological forces – that influenced the norms and acceptability of a given theory among scientists. As a result, the implicit assumptions underlying the standard view of science based on logical empiricism were inherently flawed (Longino, 2013). As Gergen (1988) has noted, one cannot separate out "observational language from theoretical presuppositions" (p. 2) because theories determine "what counts as evidence, confirmation and disconfirmation" (p. 3). Further, because words acquire meaning within specific contexts and theories are posited in language that is context-free, "there is no viable account of how it is that theories actually predict phenomena in nature (Gergen, 1988, p. 3). Thus, if observations and the language used to describe them are flawed representations of reality subject to extra-scientific factors, theories in science are an imperfect "house of cards" upon which conceptions of reality

are based.

The above critiques are well known and have been made by philosophers of science (e.g., Bhaskar, 1975; Giere, 2006; Longino, 2013; Morgan & Morrison, 1999), scholars in the social sciences (e.g., Manicas & Second, 1983; Manicas, 2006; McGuire, 1986, 1989), and community psychologists (Tebes, 2005, 2012; Tebes, Thai, & Matlin, 2014; Trickett & Espino, 1994). I am sure the authors know them well. However, the authors' extended discussion of Reichenbach's (1938) "context of discovery" vs. "context of justification," and its central place in their argument, suggests that there is a disconnect between their knowledge of this history and its implications for the importance of theories in science, and in community psychology.

The authors repeatedly return to the distinction made by Reichenbach (1938) – that is, the context of discovery vs. the context of justification, which philosophers refer to as the DJ distinction (Hoyningen-Huene, 2006) – as central to why theories in community psychology do not quite measure up as scientific. However, as philosophers since Kuhn have maintained, this distinction is less relevant for theory and science if one accepts Kuhn's central thesis: that the practice of science is inextricably embedded in social, historical, cultural, and psychological factors (Hoyningen-Huene, 2006). For Kuhn (1962/1970), the DJ distinction is mostly a false dichotomy since the context of justification is just as embedded in extra-scientific factors as those that shape the context of discovery (Hoyningen-Huene, 2006; Nickles, 1980). The authors are mindful of this issue, and cite work that has sought a rapprochement between the proponents

and critics of the DJ distinction (Hoyningen-Huene, 2006). The authors note that such a rapprochement seeks to establish a "normative perspective" among scholars for "the evaluation of scientific claims" (Hoyningen-Huene, 2006, p. 130). However, after summarizing the adequacy of theories in community psychology, they return to characterizing the DJ distinction as one between conducting "exploratory" research vs. research that seeks to "develop a good theory" and that utilizes "rigorous methods" (Jason et al., 2016, p. 18). Consistent with Kuhn and with other critics of the DJ distinction, I would argue that both types of DJ research – discovery and justification – are effectively exploratory and both use rigorous methods, depending on the context of inquiry and questions examined.

Status of theory in community psychology relative to theory in other fields in psychology

Another concern I have is that, when it comes to theory development, Jason et al. (2016) hold community psychology to a higher standard than other fields in psychology. More specifically, if community psychology has yet to produce adequate theories for scientific inquiry, one must ask: Compared to what? I give two examples to illustrate the complexity of answering this question generally for psychology, and specifically for community psychology.

A well-developed theory in social psychology is the Theory of Planned Behavior (TPB, Ajzen, 1985), which is an extension of the Theory of Reasoned Action (Fishbein & Ajzen, 1975). TPB stipulates that intention immediately precedes a given behavior and itself has

three intersecting influences: one's attitude toward the behavior, subjective norms about the behavior, and perceived behavioral control (Ajzen, 2015). Each of these influences is further affected, respectively, by one's beliefs about the likely consequences of the behavior, normative expectations for others important to the person faced with the behavioral choice, and various factors that control effective behavioral performance. TPB has been widely studied in a variety of health contexts related to health behavior change and public health. Its specificity has enabled the theory to be examined carefully in both cross-sectional and longitudinal observational studies as well as in randomized controlled trials (Ajzen, 2015; Sniehotta, Presseau, & Araujo-Soares, 2014). The utility of the theory has had mixed assessments as a theory of behavior change, but it has been useful as a theory to specify conditions under which behavioral intentions result in specific behaviors (Ajzen, 2015; Sniehotta et al., 2014). My guess is that Jason et al. (2016) would regard TPB a theory to which community psychologists should aspire, and I would agree.

In another example, in the fields of neuropsychology, physiological psychology, and cognitive science there has been considerable research conducted on the proposed theoretical principles of equipotentiality and mass action (Lashley, 1929, 1950). Mass action (Lashley [1929] originally called it mass function) refers to the notion that the brain functions as an integrated system (mass action) in which specific neurons, when necessary, can take on functions ordinarily carried out by other neurons (equipotentiality). Lashley based his

theory on ablation studies of rats, and we now know that his observations were the result of how he measured performance; he used an approach that was standard at the time and produced results which challenged the prevailing models of the brain which emphasized localization of function (Elliott & Carson, 2000). Nevertheless, Lashley's theory about equipotentiality and mass action in the first half of the 20th century was enormously influential theoretically in explaining how laboratory animals and some humans could recover specific functions, such as speech and language, despite having lesions in regions of the brain usually associated with those functions. However, as scientists were able to map brain functions with greater precision, the original principles were shown to have less utility because of previously unrecognized interconnections within the brain that accounted for the observed effects (Elliott & Carson, 2000). Despite these limitations, the influence of Lashley's theoretical principles were considerable as scientists conceptualized the brain as an integrated neural network (Hebb, 1949), and identified pervasive and distributed brain functions, including those for cognition, language, and action (Hagoort, 2005; Sporns, 2011; Willems & Hagoort, 2007).

Clearly, Lashley did not have a fully developed theory of the brain in articulating his principles of equipotentiality and mass action, and once they were assessed using more varied measurement approaches, they did not survive what Jason et al. (2016) would consider the context of justification. However, their influence as a model of brain function was quite

significant, and arguably, quite relevant to contemporary theories of the brain. Their abiding influence represents the value of the context of discovery in science and how models of behavior can have enormous scientific influence, even in the absence of a well-articulated theory.

In my view, Jason et al.'s (2016) analysis of ecological theory is similar in many ways to Lashley's principles of equipotentiality and mass action. Jason et al. (2016) summarize ecological theory as described by Kelly (1968; 2006) as well as Bronfenbrenner (1979), noting that Kelly's emphasis on interdependence, cycling of resources, adaptation, and succession provides somewhat greater specificity than Bronfenbrenner's embedded socio-ecological contexts for human development. However, for both aspects of ecological theory as used in community psychology, Jason et al. (2016) correctly point out that this "theory" operates mostly as a "framework" because it provides a basis for specifying various models and hypotheses, but not specific predictions based on the theory. This is very similar to what Lashley introduced to neuropsychology when he proposed a theoretical framework for the brain that described the two principles of equipotentiality and mass action. Lashley's framework was not a fully developed theory of the brain, but it stimulated subsequent productive science on the brain; ecological theory has had a similar impact on science in community psychology.

Overemphasis on theory as opposed to models in science

The authors focus their paper on theories in the field of community psychology.

However, in recent years, a number of philosophers of science (Frigg & Hartmann, 2012; Giere, 2006, 2010; Morgan & Morrison, 1999; Nersessian, 2006) have emphasized the centrality of models, even more so than theories, to advance science. Although the authors make a case for the value of theory, I think much of what takes actually place in science – especially when doing research in what the authors would refer to as the context of discovery – involves conceptualizing and testing models. Models represent some middle ground between theory and observation, are intended as representations of the world, and are usually tied to the contexts in which observations take place (Frigg & Hartmann, 2012; Giere, 2006; Nersessian, 2006). This view is consistent with Jason et al. (2016), although to be fair, they did not go into any depth describing models. One value of models is that they are non-linguistic; that is, they are less constrained by language as a representation of the world (Giere, 1988; Morrison & Morgan, 1999). This allows models to transcend some of the limitations of language endemic to theory. As a result, models function as physical, graphical, mathematical, or computational depictions of the world (Frigg & Hartmann, 2012; Haig, 2013; Nersessian, 2006) that serve as autonomous agents between theories and observations to mediate their relationship (Morrison & Morgan, 1999). This affords scientists freedom to characterize the world apart from the language of theory (Giere, 2010), thus producing more useful, accurate, and locally-grounded observations (Dunbar, 1999; Giere, 2010). Examples of this are found in studies of how scientists think;

their use of model building through analogous reasoning and distributed cognition in teams illustrates how science advances in the context of discovery (Dunbar, 1999, 2000; Nersessian, 2006).

Since community psychology is centrally focused on action and inquiry that is culturally situated and local, and often involves participatory approaches with various stakeholders, a focus on model-building over theory-building is likely to have significant benefits for advancing science. There are numerous successful examples of how such collaborative, culturally grounded research has advanced action as well as science in the field (Allen & Mohatt, 2014; Andrews et al., 2012; Brodsky, 2009; Jason, 2013; Tebes et al., 2014).

Lack of attention to other epistemologies in community psychology that hold scientific promise

The authors' scope was ambitious – Theories in the Field of Community Psychology – and so it is natural that they were unable to incorporate most of the relevant work related to their topic. One area that needs further consideration is other major epistemologies in community psychology and their implications for scientific inquiry. The term epistemology refers to how one comes to know the world and to make knowledge claims about it (Tebes, 2005). I conclude this commentary with a brief discussion of this issue and its implications.

As noted at the outset of this essay, the authors adopted a framework for science grounded in logical empiricism, a view no longer accepted by philosophers of science. What replaced logical empiricism in philosophy was various forms of realism (Bhaskar, 1975; Giere, 2006;

Harre, 2009; Manicas & Secord, 1983). This view holds that there is a reality “out there” independent of one’s construction of it (i.e., realism) and that the natural world, including human beings, can be known (albeit imperfectly) through direct engagement and observation (i.e., naturalism) (Tebes, 2005, 2016). For scientists, the shift from logical empiricism has had little practical consequence; they retained the scientific method but accepted its philosophical limitations by adopting a pragmatic solution; that is, a stance that allows for theoretical and methodological pluralism in which no method or theory is considered, *a priori*, to be better than another (Tebes, 2005, 2012). Certainly scientists have their biases about which theories and methods they find most useful, but norms are generally established within each field for what constitutes scientific inquiry. Within methodology, this pragmatic solution has come to be known as “critical multiplism” (Cook, 1985; Shadish, Cook, & Campbell, 2002).

Realist philosophies of science depart from the approach adopted by the authors because they do not regard explanation and prediction as the *sine qua non* of science, but rather, one component of a broader science that seeks to describe how phenomena interact in the world (Manicas, 2006). Theory directs the scientist “to provide an understanding of the processes which jointly produce the contingent outcomes of experience” (Manicas, 2006, p. 1). In this view, understanding the interactions of various aspects of the world is what science is all about, and theories – in the natural and social sciences – determine where scientists should look.

Giere (2006)'s scientific perspectivism is another realist philosophy of science that further emphasizes how observations of the world are a distinctively *human* activity; they constitute a perspectival reality from the point of view of the observer, something I have written about elsewhere in describing perspectivism, which I have argued is a foundational, constructivist philosophy of science for our field (Tebes, 2012, 2016; Tebes et al., 2014). Although the authors briefly summarize perspectivism in their paper, I believe they do not fully appreciate the implications of perspectivism for their thesis: that while science may include a focus on explanation and prediction, those are not its only or even primary purposes. The scientist seeks to construct a contextualized understanding of the natural world for various purposes, such as understanding and action as well as explanation and prediction. Thus, the task for contemporary science is *broader* than it was for logical empiricism, and requires more pluralistic theories, models, methods, and voices.

Community psychology embraces a number of epistemological traditions relevant to this task that the authors do not address. Along with perspectivism, our field also has strong philosophical roots in pragmatism, feminism, and critical theory, which elsewhere I have argued serve as foundations for a philosophy of science of community psychology (Tebes, 2016).

Pragmatism (Biesta, 2010; Dewey, 1905; Mertens, 2007; Vanderstraeten, 2002) provides a basis for understanding meaning and action, including action research, mixed methods, and program evaluation (Biesta, 2010; Tebes, 2016). The interdependence in our field between

practice and research or social action and science is consistent with Dewey's emphasis on action as the source of knowledge. Feminism and critical theory have contributed essential frameworks, theories, and models to advance scientific understanding in community psychology, often in direct resistance to the standard view of science embodied by logical empiricism (Campbell & Wasco, 2000; Nelson & Prilleltensky, 2010; Tebes, 2016). As a central foundation for community psychology, feminism's contribution to our field's scientific inquiry has emphasized situated knowing, participatory approaches, diverse methods, and action (Bond, Hill, Mulvey, & Terenzio, 2000; Campbell & Wasco, 2000; Haraway, 1988; Harding, 1986; Lather, 1991; Longino, 1990, 2013; Riger, 1992). Critical theory, as another philosophical foundation for community psychology, has focused on human experiences of power and oppression as well as resistance, emancipation, and liberation through individual and collective action (Fox, Prilleltensky, & Austin, 2009; Horkheimer, 1931/1993; Marti'n-Baro, 1994; Mertens, 2007; Nelson & Prilleltensky, 2010; Teo, 2015). Each of these approaches has moved our field away from a logical empiricist approach to science to one that embraces catalytic or transformative change (Lather, 1991; Mertens, 2007).

These four philosophical foundations for our field – perspectivism, pragmatism, feminism, and critical theory – have overlapping constructivist epistemologies with implications for science in community psychology (Tebes, 2016). In future work, the authors may wish to incorporate these into their analysis, and discuss how each offers opportunities for

scientific theory- and model-building for community psychology.

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