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COMMENTARY

Reflections on community psychology's past and future: What if a stitch in time only saves six?

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Abstract

This paper expands comments made upon receipt of the Distinguished Contributions to Theory and Research in Community Psychology, August 2015. Examination of the discipline's originating intent is reviewed as a foundation for considering its potential for distinguishing it from other social and behavioral sciences.

1 | INTRODUCTION

In June 2015, I was privileged to receive the Distinguished Contributions to Theory and Research in Community Psychology award. In receiving the award, I reviewed some of the discipline's history and its relevance to community psychology's pursuit of preventive solutions to long-standing social injustice. My intent then was to examine elements of our discipline's origins as foundational to its possible futures. I will anchor this expanded version of my comments to 18th-century Irish philosopher and British House of Commons member Edmund Burke's warning that "Those who don't know history are doomed to repeat it." I do so in the hope that reflecting on our discipline's early days may sharpen its frequently stated overarching goals to pursue social justice; empower the disempowered; prevent emotional and behavioral disorders in those at risk for disorder; promote positive life choices; and pursue related goals for diverse segments of the population but especially the disenfranchised, the disadvantaged and the discriminated.

Has the discipline met its goals in each of these pursuits over its more than 5 decades? Do we have shared goals and are we systematically pursuing them? If our achievements in these pursuits are limited, does that reflect reluctance to set their parameters? If the discipline were to reach consensus on the meaning, measurement, milestones, and priority for each pursuit, might that enable us to integrate our efforts and increase recognition of our science and practice within the academy and across the social and human service and sciences? Given the increasing methodological sophistication of the discipline's members, especially its recent generations (Jason & Glenwick, 2012, 2016), do we now have the analytic wherewithal to operationalize measurable steppingstones and gather evidence of the degree to which our admittedly lofty goals have been achieved and on the basis of which our strategies can change?

Therein lies the meaning behind my somewhat cryptic subtitle, "What if a Stitch in Time Only Saves Six?" If the aphorisms "a stitch in time saves nine" can be applied to our secondary/selective prevention work and the related "an ounce of prevention saves a pound of cure" applied to our primary/universal strategies, then how do we evaluate preventive interventions that save only six stitches or the number of ounces required to reduce needs for cure? If planned a priori as intermediary steps to the achievement of overall objectives, partial victories become evidence of progress. Reaching identified milestones could inform modifications to intervention components, thereby increasing the likelihood of ultimate success. Importantly, moving forward along a predefined trajectory aimed at less disorder and improved health

can also inform and ultimately confirm our underlying theories of pathogenesis and positive development. I have long asserted that achievement of our conceptual and program goals will most likely occur through successive approximations rather than direct links between initial versions of an intervention and its desired outcomes. Implicit in that assertion is the belief that successive approximations must be continued for goals to be attained!

My comments advocate for sharpening the focus of our discipline's efforts. Toward that end, Burke's warning suggests that a reasonable first step would be to review the issues that catalyzed community psychology's emergence and examine our achievements on those issues. Related to my point about partial victories is the question of whether the discipline moved on to other goals before completing those that led to its emergence? Did those early catalysts fortuitously lead to the discipline's birth without any obligations to continue their pursuit? Does the fact that community psychology was conceived and brought forth in the 1960s justify its seeming openness to a thousand flowers in bloom? From the outset, community psychologists have repeatedly debated the definition of their discipline without reaching consensus (Iscoe, Bloom, & Spielberger, 1976; Levine & Perkins, 1987). Seemingly, the operating definition of our discipline is that community psychology refers to "whatever community psychologists do, have interest in, take action on, or place as a defining value."

After four decades of involvement with the discipline, during nearly three of which I served as editor of the *Journal of Community Psychology*, I would respectfully argue that our amorphous definition is a major reason why our discipline has yet to gain the respectful recognition of others within academic and applied psychology and related human service fields. Going forward, I suggest that we base our discipline's theories and practices on achievement of our originating and subsequently agreed-upon purposes. I would further suggest that adherence to shared objectives will organize, rather than disconnect, us from many of our varied contemporary goals. That pursuit, I believe, will enhance disciplinary recognition and respect, thereby providing footing for our future solutions to persistent social problems.

At this stage, I believe that it is appropriate for the discipline to undertake a serious examination of several overarching questions:

- Does the discipline have an organizing theory that defines its boundaries?
- Will the discipline give priority to some interests over others?
- What methodological strategies have we developed rather than adopted?
- Is, as charged, the discipline ambivalent toward professional expertise and guilty of biases in the questions we ask?

In raising these questions, I do not pretend that my career did not mirror the discipline's open pathways. My goal in obtaining a doctorate in clinical (later labeled *Clinical-Community*) psychology was to prepare for a career in providing mental health services to working-class adults and families in a community mental health center (CMHC) near where I was raised (admittedly with a private practice on the side). In seeking his support as my doctoral advisor, I openly stated to Emory Cowen that I did not intend to pursue an academic career nor focus on either prevention or children. Four decades later, I have had multiple academic positions and most of my research and writing has focused on the identification and prevention of factors placing children at risk. Instead of involvement with CMHCs, I have partnered with schools and school systems and, most recently, served as dean of a College of Education. So much for following my original priorities!

In response to partner priorities and funding opportunities, I have focused on topics as varied as kindergarten preparation; implementation of Public Law 94–142 (Education of All Handicapped Children Act); early onset of substance involvement; exposure to pervasive community violence; and evaluation of the statewide impact of Race to the Top funding. Although each can be gathered under a broad "risk factor research" umbrella, only some reflect the discipline's originating focus on schools and the prevention of emotional disorders. I do not intend to suggest that my work has had no impact, but rather that it was not directed by an overarching definition of community psychology or its acknowledged prioritization of finding scalable solutions to targeted problems. Hopefully, the analysis that follows may assist in consolidating my successors around a set of issues and methodologies that bring recognition and results to community psychology's efforts. That future, however, should be based on appreciation of the discipline's past!

1.1 | Antecedents to Swampscott

More than a half-century ago, a small group of psychologists (mostly but not only academically based clinical psychologists) met in Swampscott, Massachusetts, to consider whether and how to join the nation's efforts to broaden access to mental health services for underserved segments of the population. Several factors made the gathering at Swampscott timely. Increasingly the public recognized that most people assigned to mental hospitals were never released and that in too many cases inpatient treatments were grossly insufficient, if not inhumane. Second, the end of two wars brought not only significant increases in the prevalence of emotional and behavioral disorders, but also signs that timely and localized responses could mitigate symptoms and offer alternatives to long-term psychotherapy and hospitalization.

Third, medications increasingly controlled the symptoms of serious mental illnesses, thereby avoiding inpatient placement for some and allowing inpatient release for others. If, as anticipated, significant numbers of inpatients were to be returned to their communities, they would need access to as yet unavailable and mostly unknown local supports and services. Finally, emerging efforts in community consultation, crisis intervention, and child guidance suggested that some emotional and behavioral disorders could be reversed early in their genesis and perhaps even prevented (Caplan, 1964).

In 1955, President Eisenhower signed the Mental Health Study Act, thereby establishing the Joint Commission on Mental Health and Mental Illness. Members of the Commission were charged with understanding the nation's mental health needs and improving services to those with mental illnesses. Toward that end, the Joint Commission sponsored a series of national studies including two major epidemiological surveys: the Midtown Manhattan study (Srole, Langner, Michael, Opler, & Rennie, 1962) and the New Haven study (Hollingshead & Redlich, 1958). Each study confirmed assumed system inadequacies, especially for the disenfranchised and the disadvantaged. The Midtown Manhattan study examined the *need* for mental health services in the general population by conducting interviews with a randomly selected sample to estimate the prevalence of self-reported symptoms. Findings from these interviews highlighted the importance of understanding emotional disorders in relation to characteristics of the social community.

The New Haven study documented imbalances between the *demand for and access to* outpatient services. Low-income and minority individuals seeking treatment were disproportionately assigned to inpatient services of uncertain duration and effectiveness. As the pharmacological revolution gained momentum, these same patients were more likely to be medicated than assigned to psychotherapy. Generally, their request for services did not occur until symptoms had become chronic or debilitating. Interpretations of these and related findings supported the conclusion that any solution would need to deliver more services to more people as early, effectively, and accessibly as possible!

A third survey study by Gurin, Veroff, and Feld (1960) examined lay views of and appropriate sources of help for mental disorders. Participants distinguished serious mental illnesses (e.g., psychoses) that required hospitalization from other emotional and behavioral disorders. Viewed as problems, rather than illnesses, the resulting distress was much more likely to be brought to physicians or clergy for assistance than to mental health professionals.

Finally, the Joint Commission also considered whether professional resources would be sufficient to respond to anticipated demand once deinstitutionalization accelerated. Communities to which inpatients would return would require an array of both services and service providers. Reflecting this reality, Albee's (1959) report, Mental Health Manpower Trends, bluntly predicted significant inadequacies in the nation's response capacity if only psychiatrists, psychologists, and social workers provided services. Members of those professions were not only too few and their training capacities too limited to meet anticipated need, but also most of them were unprepared to provide culturally sensitive and effective services to indigent and minority patients. Albee's report argued for broadening service providers to include indigenous paraprofessionals trained to deliver community-focused services.

By teaming with individuals with deep understanding of the traditions and values of the communities to which former inpatients would return and from which new patients would come, localized mental health services could be developed and delivered that would attract and retain in treatment those heretofore underserved. Albee (1959) explained that teams of professional and indigenous caregivers could contribute to the design and delivery of interventions that could disrupt incipient pathogenic processes, thereby minimizing the secondary effects of disorders. Like other health

disorders, mental disorders were not only to be treated but insofar as possible, shortened or avoided! Planting one seed for our discipline's commitment to social justice, Albee (1959) also urged that major contributors to emotional and behavioral disorders, that is, poverty, poor health care, and inadequate education, be addressed within a comprehensive assault on the nation's unmet mental health needs.

Informed by these and other studies, the Commission's final report, Action for Mental Health (Ewalt, 1960), recommended that President Kennedy undertake "a bold new approach" whose central element was the creation of a nationwide network of CMHCs. These centers would provide five essential services (inpatient, outpatient, day treatment, emergency services, and consultation and education supports). These five services were to be integrated within a system of care delivered through CMHCs serving the residents of designated catchment areas. This proposed "bold new approach" challenged each of the mental health disciplines to acknowledge the limitations of their respective areas of expertise and experience. Central to these shortcomings was their limited understanding of those who were to be served and the circumstances under which they lived and their symptoms arose. In 1963, legislation provided funds for constructing community mental health centers (The Community Mental Health Facilities Act of 1963). Funding for staffing and even further expansion of services to be provided (e.g., children's services) would come later but the shift to community-based services had been set in motion!

1.2 | The Swampscott conference

While not directly addressing the multiple studies supported by the Joint Commission, many psychologists appreciated their salience for the discipline generally and its practitioners specifically. The convergence of these forces created the impetus for a new discipline within psychology, community psychology. Meeting in Swampscott in 1965, approximately three-dozen psychologists, including John Glidewell, Louis Cohen, Ira Iscoe, Robert Reiff, Donald Klein, James Kelly, and others, discussed what they viewed as the limitations of psychology generally and clinical psychology specifically in responding to demands that would arise as CMHCs came into operation. Conference attendees examined the implications of the emerging CMH movement to expand the focus of mental health services and service providers to including consideration of how the overall community shapes health as well as pathology (Anderson et al., 1966). Beyond the creation of CMHC networks, the "bold new approach" was to include the recruitment, training, and deployment of indigenous service providers in collaboration with mental health professionals. Together, they would deliver outpatient services accessible to and accepted by those who were released from inpatient settings and other underserved community residents.

Who those indigenous providers were to be, how they were to be prepared, and especially how they were to partner with mental health professionals needed to be addressed. To do so, however, the emerging community psychology discipline would need to recognize how little it knew about how communities operate and how much clinical psychology would need to change to understand and affect the indigenous processes that enabled dysfunction or obstructed positive adaptation. If clinical psychology were to affect recognized social problems, it would need to leave the medical settings and circumstances in which its services were generally provided at that time and enter the very community settings and circumstances in which such problems emerged. If professionals and paraprofessionals were to partner in serving community needs, the former would need to appreciate the latter's expertise and redefine their respective roles in designing and delivering services.

Conference discussion of such changes focused around three keynote addresses provided by John Glidewell, Robert Reiff, and Lewis Cohen. Reportedly, each address was extensively and at times strongly debated during the balance of the gathering and for years thereafter. For Glidewell (1966), the challenge for clinical psychology to respond to anticipated CMHC-based services lay in its need to shift attention from individuals to their interactions within small groups, linked to other small groups that ultimately form social organizations and communities. Within such interconnections, he located the potential to alter values, motives, and feelings that shape behavior and thus adaptation. As was his pattern, he also warned conference attendees that the changes for the discipline under discussion held the potential for both risks and rewards. He noted that separating from clinical perspectives and practices could be liberating but it could also thrust those identifying with an as-yet-unformed discipline into the unknown and the unrecognized! To serve

those living and working within communities of color and of limited economic resources would necessitate developing the capacity to interact directly with residents of those very communities.

For Reiff (1966), the challenge lay in the significant chasm between professional and lay perspectives on mental health and illness, help seeking, and, importantly, assumptions about the delivery of help and support. Reiff's presentation included his concerns about how little most professionals know about the lives of the disadvantaged and the disenfranchised. Whereas professionals view health and illness as anchoring a continuum of emotional and behavioral functioning, for example, Reiff explained that for the working-class and poor, mental illness referred to the impairments in reasoning and the loss of emotional and behavioral control that define psychoses. Reiff's comments echoed the findings reported by Gurin et al. (1960) concerning lay views of mental illness. Reiff further explained that whereas middle- and upper-income individuals may enter and pay for therapy to "self-actualize" and understand the motives behind unwanted feelings and actions, those with less education and income primarily seek *solutions* to problem interactions and situations. As Reiff explained, this segment of the population wanted to know how to act and react to events in real time in order to allow work and life to proceed.

Working-class and low-income models for the delivery of health services included general physicians and dentists who listened, examined, diagnosed, and treated typically within a single or few sessions. Emotional and interpersonal problems, if admitted, were brought to friends, family, and the clergy who listened and shared their advice and support. If fees were involved, they were relatively low, fixed, and for a very limited number of interactions. None of these helpers expected the person seeking help to come to their own understanding through a series of conversations that at some unspecified future date could result in a solution. Rather, seeking help included the expectation that it would be given directly, quickly, and affordably! Reiff's point was essentially that before one can hope to reduce the problems that will be brought to CMHCs, psychologists must understand how they are perceived and experienced within such settings and especially how solutions must be structured and offered if they are to be accepted.

Cohen's (1966) comments focused on the implications of refocusing clinical psychology on community processes for the "scientist–practitioner" model adopted at the Boulder Conference on Clinical Psychology (Raimy, 1949) and endorsed at the 1955 National Conference of Psychology and Mental Health (Strother, 1956). Under that model, service providers were to both generate and apply empirically based findings. Cohen challenged the validity of the model arguing that few clinical scientists provided services and fewer clinicians actually engaged in rigorous studies of their services. Moreover, he questioned the applicability of the model within the fluctuating and unfamiliar circumstances of delivering traditional (e.g., psychotherapy) and emerging (e.g., consultation and prevention) services to community populations. Cohen argued that new models of inquiry would be needed for psychologists to deliver services responsive to local needs; to understand the association between locality and disorder; and to shift their perspective of disorder from intrapsychic to ecological sources.

If, as debated at the Swampscott meeting, one reconceptualized mental health problems as "problems in living" generally and living in a distinct community with specific demands, resource limitations, and histories specifically, then community psychologists would need to understand both situational and individual factors and design interventions that could resolve problems in real time and increase individuals' capacity to do so in the future. Therein lies the discipline's defining purpose as it moved to prepare mental health professionals capable of designing and delivering forms of intervention that were ecologically based, responsive to current needs and resource availability and increased the capacity of those served to avoid or resolve future problems in living.

1.3 | The roads not taken

As admitted earlier, my original career goals were for a CMHC position serving working-class, low-income, and minority populations and for a part-time private practice. Both didactic and clinical elements of my doctoral preparation challenged the CMHC goal. Faculty preference for program graduates to pursue academic, rather than applied, careers was not explicit but generally understood. I heard multiple rationales from clinical supervisors and later colleagues for why these segments of the patient population were inappropriate for verbal psychotherapy. Included among these

were assumptions of their limited verbal skills, inability to delay gratification, unrealistic expectations concerning the temporal requirements for psychological change, and insufficient abstract cognitive ability.

As I began my internship in an outpatient mental health unit, I was advised to schedule low-income and minority patients early in the morning since they would likely not appear anyway. I later discovered that local bus service made attending early sessions quite challenging. During my internship and initial years of service, I repeatedly heard therapists express doubts about the efficacy of their treatments for these patients. As explained by Reiff (1966), working-class patients made evident their biases against mental health labels. Interactions with my patients confirmed their skepticism that seeing me would help them. In agreeing to "try therapy," they frequently make explicit that it needed to ease their pain and discomfort and that I needed to assure them that being in therapy did not constitute "being crazy."

My dissertation and subsequent forays into services research revealed that therapists and patients generally misunderstood each other's messages, frequently resulting in patient attrition from treatment (Lorion, 1973, 1974a). Therapists' assumptions appeared to be a major obstacle to recognition of the potential benefits of psychotherapy for patients from diverse backgrounds and cultures (Lorion, 1974b). Therapists' expectations concerning the duration of therapy with such patients, for example, were highly predictive of treatment outcomes. Patient expectations about how many treatment sessions would be required correlated highly with how many sessions patients attended (Lorion, 1975). In many cases, their estimate reflected what they could afford in time away from work and the direct and indirect (e.g., lost wages) costs of attending sessions. In fact, sophisticated examinations of psychotherapy effectiveness (Garfield & Bergin, 1978) countered both sets of misperceptions. Low-income patients remaining in psychotherapy beyond four sessions had an equal if not better likelihood of successfully completing long-term psychotherapy as those with greater education and higher incomes (Lorion, 1978).

Although my initial research interests and career goals appeared closely aligned, the anticipated clinical path was not taken for two basic reasons. As my doctoral studies approached completion, my wife and I chose to remain in Rochester in anticipation of the birth of our daughter. Fortunately, Emory Cowen had hired me to serve as research coordinator for the Primary Mental Health Project (Cowen et al., 1996). That work exposed me directly and deeply to the potential of early detection and intervention for altering pathogenic trajectories. Initial skepticism about the likelihood that the promises of prevention could be kept gradually weakened as I analyzed Cowen's data and that of others working on understanding early stages of pathology.

The second reason emerged through my clinical appointment in the department of psychiatry, delivering time-limited, problem-focused psychotherapy in a clinic targeted to low-income patients. That treatment modality had just been introduced as a potentially valuable CMHC approach. By all indices (e.g., full waiting room; extended wait list; and patients completing more sessions in our time-limited approach than comparable patients in long-term therapy), our clinic was successful. I seemed on course to follow my original career path!

What changed that goal was reading Dumont's (1968) book, *The Absurd Healer: Perspectives of a Community Psychiatrist*. As noted, by all indices our time-limited clinic was a success. Dumont reported comparable findings from a similar clinic at Harvard. Diluting the sense of success, however, was Dumont's stated conclusion that successful clinical services would never *empty* the waiting room! Although time-limited treatment appeared to be a demonstrably effective intervention for those previously underserved in traditional long-term outpatient clinics, that clinical success would not reduce the number of new cases. Reluctantly, I accepted the longstanding public health mantra voiced by Cowen (1973) and others (e.g., Caplan, 1964) that no disease was ever controlled by treatment but only by prevention! That acceptance convinced me to travel the academic, rather than clinical, route. It also raised my appreciation of the importance of the scientist–practitioner model for clinical psychology as a means to distinguish itself from its psychiatric and social work colleagues.

Community psychologists met several times following Swampscott, most notably in Austin in 1975. Reflecting the discipline's evolution since Swampscott, those attending focused on the breadth of interests; the growth of doctoral and subsequently subdoctoral programs to prepare clinical–community scientists and practitioners; and the increasing diversity of the discipline's ranks. The event celebrated the expansion of the field and of its interests. Contrast the title of the Swampscott meeting's report, that is, A Report of the Boston Conference on the Education of Psychologists for *Community Mental Health* (emphasis added), with that from Austin's, that is, Community Psychology in

Transition (Iscoe, Bloom, & Spielberger, 1977). The former meeting was organized in response to a national need; the latter focused on our discipline!

Austin might have been an opportunity to confirm the discipline's originating intent, "the education of psychologists *for community mental health*," thereby at least narrowing, if not defining, its foci. The zeitgeist in 1975, however, would never have permitted such an outcome! I expect that I, like many of my colleagues attending the event, felt validated by its message that all pathways were leading to desirable ends and that our work exemplified the discipline's promise as an antidote to clinical psychology's concerns with psychopathology, with professional acceptance within the health professions, and with competition with psychiatric practice. We left Austin without shared purpose but with shared pride as those who resisted what we deemed the failings of our clinically inclined colleagues.

Throughout the 1970s and 1980s, our efforts to prevent disorder and promote health (both defined generically) presented political and scientific challenges as well as missed opportunities for community psychology to establish itself as one branch of psychology with a commitment to solve intransigent social issues. To do so, however, would have likely required significant changes in how our work was evaluated within the academy and how we defined collaboration with our community partners. As explained by Sarason (1981, 1982), the academy's reward structure gave priority to individual and entrepreneurial, rather than collaborative, efforts, and external funding structures too often determined what issues would be studied and which would be orphaned (Sarason, 1976, 1981). Sarason also noted the intractability of social problems such as chronic poverty and cross-generational educational failure (Sarason, 1978). Whatever solutions we would develop for such issues would be transitory and in need of continuous reinvention given that they and other social-interpersonal issues reflect relative rather than absolute human conditions.

These caveats notwithstanding, however, community psychology could have committed to a comprehensive Manhattan Project-style effort to understand and alter the lives of those who are trapped in cycles of poverty, ineffective education, and continuing under- or unemployment. Applying what we now refer to as "participatory action research" methods and the array of contemporary qualitative and quantitative approaches to inquiry (Jason & Glenwick, 2016) adopted, and in some cases developed by our colleagues, we could have leveraged Albee's (1959) call to partner with indigenous colleagues to tackle those community processes and structures enabling cycles of poverty, thereby identifying and enabling escape routes.

Swampscott and Austin might have been followed by a series of planning meetings creating interlaced programs of applied research that would alter the life courses of the very segments of the population targeted by our ongoing diverse efforts. Such work might have reshaped elements of those communities about which we expressed concerns in ways that reduced the incidence of disorder by promoting emotional health, academic success, and pathways for successful entry into the workforce. As noted, those opportunities remain to be embraced. Given the National Institutes of Health's growing support for translational research and collaboration mirrored by similar support from state and local governments and philanthropic foundations (e.g., Gates; Kellogg; Annie E. Casey), the time seems ripe for leading such efforts.

Not having openly debated and resolved underlying disagreements, many of us left Austin convinced that our work and our discipline were headed in the correct direction. Doubts about the validity of mental disorders and their biological elements were expressed but not openly challenged. Those of us engaged in the delivery of clinical services downplayed the importance of this debate to our professional identity and failed to argue against those suggesting that interconnections between biological and psychosocial factors[1] were wrong. There seemed to be little doubt about our deserving credit for concepts of prevention and promotion and for the primacy of primary prevention over its lesser siblings, secondary and tertiary.

Prevention advocates boldly promised that the incidence and prevalence of unspecified mental disorders would be reduced once federal sources funded (a) doctoral training programs for prevention scientists, (b) centers for preventive intervention research centers, and (c) cutting-edge approaches to understanding and promoting mental health. Disagreements among those claiming the community psychology mantle reflected questions about which mental

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[1] One consequence of not having presented this argument earlier is psychology's current embracing of biological and neuroscience explanations for emotional and behavioral disorder to the exclusion of its social and community elements.

illnesses were to be prevented and whether behavioral problems (e.g., substance involvement) and problems in living (e.g., marital issues; adolescent adjustment) were to be included. Absent from these disagreements were theoretical and empirical bases for stated positions.

Serving as the acting associate administrator for ADAMHA's Mission of Prevention from 1982 to 1984, I observed first-hand competition, rather than collaboration, across the discipline for limited funds to support items on the afore-mentioned wish list. Much to the chagrin of many colleagues, I expressed concern about prioritizing primary over sec-ondary approaches. Further alienating me from my colleagues, I argued that federal funding for preventive interven-tions should require evidence of pathogenic links between targeted risks, proposed interventions, and expected health outcomes (Lorion, 1983, 1990). I explicitly argued for evidence of reductions in incidence/prevalence rates to justify funding dissemination efforts.

The politics of the time may have encouraged ambitious promises concerning primary prevention, but available evidence suggested that interrupting pathogenic sequences early through secondary prevention efforts could inform subsequent primary prevention strategies and provide immediate evidence of the benefits of early detection and inter-vention (Lorion, 1983)! I mistakenly assumed my positions would provide support to ongoing community psychology efforts. Instead I was soundly criticized for taking such a radical and disloyal position by some of the discipline's leading prevention advocates! My critic argued that our primary and secondary prevention efforts should be no more obligated to scientific rigor and documented evidence than treatments provided by our psychiatric colleagues (Albee, 1986)!

Over the next decade, the breadth of the scientific challenges presented by prevention research was gradually uncovered by the hard work by community psychologists and our social–clinical colleagues (e.g., Cowen 1983; Kellam et al., 1991; Olds, 2012; Price, 1983; Sameroff & Fiese, 1989). We came to appreciate that interventions to prevent dis-order and promote health required systematic studies of the etiology of disorder; the epidemiological distribution of risks and protective factors contributing to their actualization; and identification of opportunities along the etiological sequence to avoid, interrupt, or mitigate that pathogenic sequence. Jason's Oxford House work on alternative treat-ment for those addicted to substances provides increasing evidence for secondary and tertiary efforts (Jason, Olson, Ferrari, & Lo Sasso, 2006).

Public health protocols for physical disorders indicated that etiological pathways can be confirmed prospectively and retrospectively by merging findings from studies of developmental pathways and case control analyses. Exempli-fying this complexity was recognition that some outcomes emerged from an array of risk factors and that some unique risk factors were antecedents to an array of outcomes (Cichetti & Rogosh, 1996; Sameroff & Fiese, 1989).

From this perspective, prevention studies fit the scientist–practitioner model but did not fully align with its aca-demic profile given its emphasis on work *in* the field and *with* the field. Unquestionably, our involvement with risk-factor research evolved in sophistication as we began to understand the complex challenges confronting prevention science. Public health's diathesis–stress model was broadened for many mental health outcomes as a *diatheses–stresses* model. Epidemiological evidence confirmed that inherited vulnerabilities or their experiential, ecological, and situational trig-gers rarely occur alone. Rather, outcomes frequently result from permutations and combinations of both.

This complexity is reflected in the specificity of intervention outcomes measured by public health specialists and epidemiologists. Although not yet diffused across community psychology's efforts, indices such as the preventive frac-tion were gradually introduced as indices of intervention success. If the target mental health outcome is determined by multiple factors, only one or two of which are addressed in the intervention, the maximal reduction in incidence or prevalence is proportionally limited. If we predict that our "stitch in time" will save nine when only four, five, or six is possible, then is the work a success or failure? If ounces of prevention must be delivered sequentially, then are they still worth a pound of cure? Careful estimation of the preventive fraction is necessary to determine whether a proposed intervention is worth the cost in time, money, public expectations, and our professional credibility. Similarly, our work needs to consider the fact that diagnosable mental illness as well as many persistent problems of living have profound effects on mortality, life expectancy, years of life lost, and other indicators such as disability adjusted life years, quality adjusted life years, and others.

Acknowledging the advanced state of prevention science within public health is not meant to imply that this work did not emerge from humble beginnings. After all, Dr. John Snow revealed the power of limiting exposure to a

contagious bacteria when he removed the handle of the Broad St. pump in 1854. He did so without understanding that cholera was caused by the vibrio cholera bacterium or identifying its etiological pathways, incubation periods, and transmission routes. His intervention emerged from systematic observations that linked the source of water from a particular pump for home use and patterns of cholera infection. Learning from and collaborating with our public health colleagues can increase the reach of our efforts. In the process, however, we should be prepared to encounter their political battles.

That our discipline has the tools to benefit from public health perspectives is concretely reflected in Jason and Glenwick's (2016) *Handbook of Methodological Approaches to Community-Based Research: Qualitative, Quantitative, and Mixed Methods*. The title of that handbook merits careful attention–qualitative methods have equal billing with quantitative and mixed methods approaches. Based on the content of the entire volume, qualitative methods may even have more pages! When our founders came to a fork in the road, most, but not all, followed the scientist–practitioner path!

Others pursued a road less traveled, whose markings could be found by shifting emphasis from positivistic scientific rigor to the traces detected if one observed, discussed, partnered, and generally retreated from what Sarason (1981) referred to as "professional preciousness." Sarason was without equal in highlighting our foibles with surgical skills. "Preciousness" refers to the tendency (attitude) of professionals to dismiss the views of their clients, patients, and potential recipients of their services because of the professional's superior preparation in the substance of their discipline and in the practice or their profession. Obviously, true participatory action research should be negatively correlated with our levels of professional preciousness! Looking within ourselves and seeking reflections of others may, however, contribute to moving that correlation in the desired direction. But, that discussion can wait for later! *Paths open for future exploration*: As noted, the intent of my comments is to suggest future pathways that might renew community psychology's potential as an applied social science.

Those who know me may be as surprised as I am that, at this stage of my work, I find myself again nearing that fork and preferring the path leading to tangible changes in the lives of the underserved, undereducated, and underrespected over that which leads to rigorous evidence and publications. As I mentioned earlier, from the outset community psychology has wavered between being accepted into clinical psychology's scientist–practitioner club and doing whatever was necessary to change the lives of those who are underserved, underrecognized, and disempowered. Community psychology began as a CMHC ally at the Swampscott conference to serve the needs of those with limited access to and acceptance of the reigning treatment modalities. The lack of access was to be addressed by relocating services to the communities in which the underserved lived. We were to partner with CHMCs professional and indigenous service providers to create new forms of intervention tailored to the lives and needs of intended recipients. The lack of effectiveness for those in need was to be addressed in part by broadening the range of options in terms of (a) length (e.g., time-limited therapies), (b) service provider (e.g., paraprofessional and natural caregiver agents), and especially (c) point of intervention (e.g., primary and secondary prevention) along the etiological pathway.

Our originating intent was to explore heretofore uncharted routes by which our clinical colleagues could respectfully and knowingly enter communities and positively affect the lives of those who, to that point, had been ill-served or underserved. The fork confronting the discipline then and now is to what extent we operate within the expectations of the academy and scientist–practitioner boundaries or within the pragmatic demands of those who serve and live in communities in need!

In preparing my foreword for Jason and Glenwick's (2016) volume on methodological options, I noted that same directional tension in many of its chapters. Focused on explaining the rationale and procedures of their methods, the authors provide the technical details that introduce readers to the potential applications and informational benefits of their procedures. Woven through their recipes and especially their case examples are variously stated themes of gathering new and deeper insights into the lives of the disenfranchised, the disempowered, and the underserved. At times subtly stated and at times explicit, the agenda for applying these innovative quantitative, qualitative, and mixed methods is to create, enable, and accomplish *change*!

Understanding the status quo is precedent to designing its alteration in a nonrandom, intentional direction. Research methods are explicitly defined as routes to confirming existing beliefs! Woven throughout that volume as

well as many of the papers submitted to the *Journal of Community Psychology* have been affirmations of the value of the reported work for theory building without exactly identifying a recognized and defined theory. References to paradigm rarely explain what is paradigmatic about the work, the exact nature of the paradigm, and the breadth of its scientific implications (Kuhn, 1962). Both "theory" and "paradigm" as well as references to the "ecological" basis of work appear to be stated more as evidence that the work described is truly scientific than referring to an identifiable theory or paradigmatic system. Rather than debate the meaning of theory and paradigm, might we at least tentatively accept the possibility that work derived from origins other than paradigmatic origins has value? Arguing for the value of qualitative findings, too often submissions to the *Journal of Community Psychology* and other disciplinary outlets ignored scientific criteria for replicability, generalizability, and either internal or external validity of measures and methods. Observations were deemed salient because they were "real" rather than theory-based or useful to design interventions!

Interpreting the motives of our discipline's founders as they chose which path to follow may, admittedly, reflect projection on my part. My career can be perceived as blindly subservient to the principles of positivism or as focused on seeking and applying practical solutions to real problems. Of increasing concern to me over the years was the perspective that engaging with a community required commitment beyond the terms of funding or the acceptance of published research. Too often, we have left communities feeling used and exploited after our experimental interventions were field tested, our funding expired, and competing demands on our time and expertise arose. Compare this to the model provided by the Olds (2012) team's decades of work on the Nurse-Home Visitor Program; of Cowen's career-long commitment to the Primary Mental Health Project (Cowen et al., 1996); and to Jason's work to unravel the mysteries of chronic fatigue syndrome (Jason, 2015); and reveal the processes underlying Oxford House's approach to addiction services (May, Hunter, & Jason, 2017).

Early in my editing life when confronted with qualitative studies with "too few" subjects," "too little control," and "too much speculation," I argued for rigor! Over time, I would call upon colleagues such as Seymour Sarason and Bob Newbrough for reassurance that I was not limited to a single path. Essentially, they echoed Yogi Berra's oft-quoted advice, "When you come to a fork in the road, take it!" Early in my editorial career, for example, a manuscript was submitted by a high school principal commenting on the attitudes and behaviors of the students for which he was responsible. I shared with my predecessor, Bob Newbrough, that although the piece was provocative, unfortunately it could not be published because it did not meet scientific standards. Bob's response surprised me–"You're the editor, simply tell the readers why you decided to publish it!" I did! Later in my career, I found the need to set limits and distinguish qualitative research per se and simply the documentation of opinion and preconceived notions!

Paul Dokecki is another fellow traveler along that "path not taken." His contribution to a special issue (edited by Newbrough, 1992) of the *Journal of Community Psychology*, which focused on the future of the discipline in a postmodern world, made me reflect on alternatives paths. In his paper, Dokecki (1992) explained how Schon's (1983) concept of the "reflective practitioner" can be a valid alternative to the scientist–practitioner model. The scientist–practitioner gathers knowledge to inform and shape practice. The reflective–practitioner "intends to improve the human situation through the close interplay of knowledge use and knowledge generation" (Dokecki, 1992, p. 27). My reading of Dokecki's words reframed the branches of the fork to "what do we want to know?" on one side and "what do we want to do?" on the other. It reminded me that community psychology emerged from a sense that the CMHC movement offered a federally supported opportunity to *do* something about the inequitable distribution of mental health services to those segments of the population most underserved. One catalyst for our separation from clinical psychology was our focus on *learning how to serve the underserved but especially to serve those in need*. The tension between learning and doing marked us from the beginning! Evidence of learning something new is marked by publications and grant awards; evidence that we have done something is marked by changes in the condition of those we serve. These pathways intersect but which has initial priority is key!

As noted, our discipline began with the intent to *change* the status quo, that is, *to do something*! That purpose is echoed throughout Jason and Glenwick's (2016) volume. It is also reflected in an increasing proportion of the submissions to the *Journal of Community Psychology*. Consider for the moment the implications of the aforementioned dichotomy between knowing and doing. Studies responding to the "what do I want to know?" question have to

justify the work in terms of filling a gap in our knowledge of some aspect of some phenomenon. A scientifically valid and sufficient gain in knowledge achieves its purpose and points the way for further work. This process is the very essence of Kuhn's (1962) "work of normal science!" By contrast, studies responding to the "what do I want to do?" have to document: (a) the need for action, (b) the action taken, (c) those taking and receiving the action, and (d) evidence that what needed to be done has been achieved. Each of these steps requires deep immersion into the setting and circumstances to understand that need. Doing so requires acceptance of the perspectives and input of those experiencing the need or committed to but unable to serve that need.

Participatory action research thereby becomes an essential element of community-based intervention by placing the work in a setting with documented need and requiring those to be impacted to acknowledge and participate in mitigating that need. Participatory action research allows those receiving services to define both the nature and the limits of the application of services. "Better" is determined by participants rather than by provider! Accepting the possibility that community psychology's purpose is to affect the quality of life and effectiveness of communities for their residents does not lessen its worth but focuses its efforts. It also places a high priority on understanding the limits of our impact and, as justified, on bringing what works to scale. It also leaves to those served the basis for deciding if enough stitches have been saved and the number of ounces of prevention are justified!

From 2004 through 2015, I served as a dean of a College of Education and from 2010 to 2015 as executive director of the Center for Application and Innovation Research in Education (CAIRE), an applied research center formed initially to conduct formative and summative evaluations of the $250,000,000 Race to the Top award to the Maryland State Department of Education (MSDE). Their efforts focused on four "assurance areas": (a) standards and assessments, (b) data systems, (c) great teachers and leaders, and (d) turning around low-performing schools. Clearly an ambitious undertaking to be carried out over 4 years! They proposed to meet these goals through 54 statewide projects and approximately 125 school system-specific initiatives. CAIRE was awarded $5,000,000 to monitor their efforts; confirm adherence to commitments; measure their progress; and determine the impact of their work. To do so, we adopted a mixed methods approach that incorporated observational studies; 13 longitudinal case studies; and longitudinal surveys of school system and school-based personnel concerning the "common core" standards and associated assessment procedures.

The longitudinal case studies provided fodder for identifying the processes of school reform across levels ranging from the State Department of Education, across three school systems and within nine separate schools. Surveys included text boxes whose contents were thematically analyzed to inform data interpretation and, if indicated, survey revisions. Lessons learned in this work can be applied to other community-based efforts. First, changes of any kind take time to percolate through systems regardless of legislative or policy mandates. Time is a critically important and generally ignored or denied aspect of any intervention. MSDE's program commitments could not be achieved because the duration of funding was insufficient to document the sequential nature of change elements.

Similarly, promised outcomes from preventive interventions are often impossible to document because pathogenic processes rarely operate within the temporal limits of external funding. As we move forward, a portion of our responsibility must be to inform policymakers, funders, and especially participants in our interventions of the temporal realities of change. Repeatedly in our Race to the Top assessments, we needed to remind schools (especially Pre-K–3 settings) participating in interventions that few if any of the children currently enrolled might benefit from implementation of an emerging intervention. True educational reform typically requires 12+ years to percolate across grade levels and convince educators of the value of sustaining incipient changes. Providing information about the true duration of change processes was a justifiable element in obtaining informed consent but it also made more difficult gaining such consent from school boards, teachers, parents, and students! Numerous conversations were required with each stakeholder group to garner the parental support necessary to move forward.

Second, designing and offering empirically validated services may be refused by intended consumers for a variety of reasons. Millions of dollars spent to develop a web-based instructional resource, for example, was little known and rarely used. "Training the trainers" models to disseminate interventions were of limited effectiveness absent funds and time required for trainers to master knowledge transfer skills. If developed, that mastery has little effect if those they are to train have limited time and interest in being trained and applying acquired skills.

Finally, a last minute addition to our periodic survey asked if the new standards would improve children's mastery of content areas. Only a limited number of teachers believed it would improve mastery, with the majority expecting little difference and some expecting it to harm learning. Imagine the challenge this presents to those responsible for the change! Now think about how often you have heard about community interventions that have asked the targeted recipients of those interventions (or even the community-based providers) if they expect that what is proposed will succeed? Would that we had asked that question earlier; perhaps the federal and state policymakers might have similarly thought about that issue.

Acknowledging that we engage with communities to "do something" together does not mean we abandon the accumulation of information that has theoretical or paradigmatic import. It does mean, however, that *doing* takes priority over *knowing* and that our work and our responsibility are not completed with the acquisition of knowledge or the advancement of science. Those accomplishments add value to our efforts and, admittedly for those in the academy may lead to tenure, external funding, and disciplinary recognition. They do not, however, lessen our professional responsibility to remain engaged, to continue _our_ participation in the work until released by our community partners. To truly enact a participatory action effort requires genuine empowerment of partners over *us*! If we initiate the effort and commit members of a community to engage in assessing their needs, analyzing their resources, and committing to collaboratively moving toward sustainable change, we necessarily commit ourselves (and in many cases our institutions) to remain engaged, however long it takes.

Community psychology's dependence on methodological diversity lies not simply with its evolution as an applied science but with its founding commitment to understanding human needs that would otherwise go unrecognized, underserved, disrespected, and devalued. Our discipline is unlike other psychological, social, public health, or public policy sciences, and that difference lies in our *defining commitment to become part of the community*, wherein we can collaborate with the community as it defines and activates sustainable responses to its needs. Beyond the "participant–observer," we must be the "participant–doer." As noted, our discipline arose out of a need for real change in how human problems were understood and served. Our shift from revising who, where, and how mental health services were delivered to engaging the conditions leading to health and pathology required us to move from primary reliance on quantitative methods to increasing expertise in qualitative strategies and especially mixing findings from both information gathering approaches.

Our approach to applied work requires learning enough about the conditions that need change to appreciate that those at risk are frequently best prepared to identify necessary parameters of and circumstances to sustain change. I am not proposing that we abandon one path for another, but rather that we open a new path between the two and, in the process, change the reward structures within our academic homes and create viable roles for our community-based partners in the academy. I leave to readers to decide appropriate answers to the questions presented earlier:

- Does the discipline have an organizing theory that sets definable boundaries?
- Will the discipline continue to embrace seemingly endless interests?
- Have we adopted rather than discovered methodological strategies?
- Is there validity to criticisms of the discipline as ambivalent toward professional expertise and guilty of biases in the questions we ask?

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